This is a digital copy of a book that was preserved for generations on library shelves before it was carefully scanned by Google as part of a project to make the world's books discoverable online.

It has survived long enough for the copyright to expire and the book to enter the public domain. A public domain book is one that was never subject to copyright or whose legal copyright term has expired. Whether a book is in the public domain may vary country to country. Public domain books are our gateways to the past, representing a wealth of history, culture and knowledge that's often difficult to discover.

Marks, notations and other marginalia present in the original volume will appear in this file - a reminder of this book's long journey from the publisher to a library and finally to you.

Usage guidelines

Google is proud to partner with libraries to digitize public domain materials and make them widely accessible. Public domain books belong to the public and we are merely their custodians. Nevertheless, this work is expensive, so in order to keep providing this resource, we have taken steps to prevent abuse by commercial parties, including placing technical restrictions on automated querying.

We also ask that you:

+ *Make non-commercial use of the files* We designed Google Book Search for use by individuals, and we request that you use these files for personal, non-commercial purposes.

+ *Refrain from automated querying* Do not send automated queries of any sort to Google's system: If you are conducting research on machine translation, optical character recognition or other areas where access to a large amount of text is helpful, please contact us. We encourage the use of public domain materials for these purposes and may be able to help.

+ *Maintain attribution* The Google "watermark" you see on each file is essential for informing people about this project and helping them find additional materials through Google Book Search. Please do not remove it.

+ *Keep it legal* Whatever your use, remember that you are responsible for ensuring that what you are doing is legal. Do not assume that just because we believe a book is in the public domain for users in the United States, that the work is also in the public domain for users in other countries. Whether a book is still in copyright varies from country to country, and we can't offer guidance on whether any specific use of any specific book is allowed. Please do not assume that a book's appearance in Google Book Search means it can be used in any manner anywhere in the world. Copyright infringement liability can be quite severe.

About Google Book Search

Google's mission is to organize the world's information and to make it universally accessible and useful. Google Book Search helps readers discover the world's books while helping authors and publishers reach new audiences. You can search through the full text of this book on the web at `http://books.google.com/`

BOSTON
MEDICAL LIBRARY
8 THE FENWAY

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

17116

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

VOLUME EIGHTY-ONE.
1854.

EDINBURGH:
ADAM AND CHARLES BLACK;
LONGMAN, BROWN, GREEN, AND LONGMANS, LONDON;
AND HODGES AND SMITH, DUBLIN.
MDCCCLIV.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL:

EXHIBITING

17116

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND PHARMACY.

VOLUME EIGHTY-ONE.

1854.

EDINBURGH:
ADAM AND CHARLES BLACK;
LONGMAN, BROWN, GREEN, AND LONGMANS, LONDON;
AND HODGES AND SMITH, DUBLIN.
MDCCCLIV.

Neill & Co., Printers, Edinburgh.

1-1c C 51

THE EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. CXCVIII.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Illustrations of Gout and the Gouty Diathesis. By JAMES BEGBIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physician in Ordinary to the Queen in Scotland, Page 1

II. Cases of Xerophthalmia, with Remarks. By R. TAYLOR, Surgeon to the Central London Ophthalmic Hospital, . . . 21

III. Two cases of Partial Chorea in Pregnancy; with Remarks. By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E.; Lecturer on Midwifery; Physician-Accoucheur to the Royal Dispensary, Edinburgh, &c., &c., ... 35

IV. Tropical Fever and Dysentery; being the result of an experiment by order of Government, in the General Hospital, Calcutta, and Garrison Hospital, Fort-William, in the years 1849–50, with a Sketch of the Theory resulting. By E. HARE, Surgeon 1st Bengal Fusiliers, ... 40

V. On the Nature of Inflammation. By JAMES CAPPIE, M.D., Edinburgh, ... 58

VI. Case of Death occurring forty hours after the administration of Chloroform; with Remarks on the Causes of Death from the Use of Anæsthetic Agents. By KELBURNE KING, M.D., 79

VII. Report of Surgical Cases occurring in Hospital Practice. By JAMES DUNSMURE, M.D., F.R.C.S., Surgeon to the Royal Infirmary, Edinburgh, ... 92

VIII. Clinical Surgery—Report on Surgical Cases occurring in Hospital Practice, &c. By R. J. MACKENZIE, Esq., F.R.C.S., Surgeon to the Royal Infirmary, Lecturer on Surgery, . . 103

IX. On the Inflammatory Affections of the Mucous Lining of the Larynx. By ALEXANDER WOOD, M.D., F.R.C.P., Lecturer on Practice of Medicine, ... 120

PART II.—CRITICAL ANALYSIS.

ART. I. The Science and Art of Surgery. By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon in University College Hospital, ... 150

II. A Treatise on Auscultation and Percussion. By Dr JOSEPH SKODA, 174

III. 1. Elements of Psychological Medicine. By DANIEL NOBLE, F.R.C.S., ... 187

2. On the Nature and Proximate Cause of Insanity. By JAMES GEORGE DAVEY, M.D., ... 187

3. Popular Errors on the subject of Insanity examined and exposed. By JAMES F. DUNCAN, A.M., M.D., . . 187

IV. Urinary Deposits, their Diagnosis, Pathology, and their essential Indications. By GOLDING BIRD, A.M., M.D., F.R.S., &c., &c., 190

V. On the Prevalent Treatment of Disease. Two Lectures delivered in the Theatre of the Royal College of Surgeons in July 1853. By FREDERIC C. SKEY, F.R.S., ... 193

PART III.—Summary of the Progress of Medical Science.

Case of Pregnancy in a rudimentary horn of the uterus, with probable advance of the ovum from the right ovary into the left horn of the uterus, 201
The Inner Surface of the Uterus after Delivery, . . . 202
Phlegmasia Dolens in a Virgin, ... 202
The Secretion of Milk in Newly Born Infants, . . . 202
Treatment of Aneurism by Compression, ... 203
Intra-Cranial Suppuration following Scalp Wounds, . . . 205
Minie Rifle Ball Wounds, ... 206
Case in which Sarcina Ventriculi was found in the fluid removed from the Ventricles of the Human Brain, ... 206
On the Characters of Urine depositing Oxalate of Lime, . . 207
Cholera successfully communicated to the lower Animals, . . 208

ERRATUM.

In Volume Eightieth, page 462, footnote, *for* opium 20 ounces, *read* opium 20 grains.

PUBLICATIONS RECEIVED.

Practical Treatise on Aural Surgery. By W. R. Wilde, F.R.C.S.I. London: Churchill. 1853.

A Treatise on Auscultation and Percussion. By Dr Joseph Skoda, translated by W. O. Markham, M.D. London. 1853.

Faraday's Lectures on the Non-Metallic Elements, arranged by permission from the Lecturer's Notes lent for the Occasion. By J. Scoffom, M.B.

Urinary Deposits, their Diagnosis, Pathology, and their essential Indications. By Golding Bird, A.M., M.D., F.R.S., &c. Fourth Edition, enlarged. 1853.

The Chemistry of the Four Seasons. By Thomas Griffiths. London: Churchill. 1853.

On Perforating Ulcer of the Stomach from Non-Malignant Disease. Part I. By Edward Crisp, M.D., Physician to the Metropolitan Dispensary. Teape and Son, London. 1852.

Crisp's Treatise on the Structure, Diseases, and Injuries of Bloodvessels, with Statistical Deductions. 8vo, Plates.

The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases frequently mistaken for, or associated with, Phthisis. By John Hughes Bennett, M.D., F.R.S.E., &c. Edinburgh. 1853.

Six Lectures on Materia Medica, and its Relations to the Animal Economy, delivered before the Royal College of Physicians in 1852. By John Spurgins, M.D., F.R.C.P. John Churchill, London. 1853.

On the Nature and Treatment of the Deformities of the Human Frame. By W. J. Little, M.D. London, Longmans. 1853.

Homœopathy: its Tenets and Tendencies, Theoretical, Theological, and Therapeutical. By James Y. Simpson, M.D., F.R.S.E., &c., &c. Edinburgh: Sutherland & Knox. 1853.

On the Decline of Life. By Barnard Van Oven, M.D. London : Churchill. 1853.

The Sophistry of Empiricism. Churchill, London. 1853.

On Continuous Molecular Changes; more particularly in their Relation to Epidemic Diseases. By John Snow, M.D. Churchill, London. 1853.

Effects of Lead on the System. Lumleian Lecture, by James Alderson, M.D., F.R.S. Savill & Edwards, London. 1852.

Letter to Lord Shaftesbury, containing Suggestions as to the Expediency of submitting certain branches of Trade and Manufactures to Government Inspection. By J. B. Harrison, M.R.C.S., &c. Manchester, 1851.

W. W. Cooper, on Near Sight, Aged Sight, Impaired Vision. Second Edition, 7s. 6d.

Dr J. Pereira's Elements of Materia Medica and Therapeutics. Vol. ii. Pt. 2.

The Harmonies of Physical Science in Relation to the Higher Sentiments; with Observations on the Study of Medical Science, and the Moral and Scientific Relations of Medical Life. By William Hinds, M.D., &c., &c. Churchill, London. 1853.

On the Nature and Proximate Cause of Insanity. By James George Davey, M.D. Churchill, London. 1853.

Notes worth noticing relative to Cholera. By D. Gillkrest. London : Golbourn. 1852.

Stevens on Asiatic Cholera. Baillière. 8vo.

Elements of Psychological Medicine. By Daniel Noble, F.R.C.S. London : Churchill. 1853.

Highley's Popular Medical Series. No. I. A Practical Treatise on Sea-Bathing and Sea-Air. By George Hartwig, M.D., London. 1853.

Sketch of the Operation and some of the most striking Results of Quarantine in British Ports since the beginning of the Present Century. By Gavin Milroy, M.D. London : T. Richards.

On the Prevalent Treatment of Disease. By Frederick C. Skey, F.R.S. London : Chapman.

On the Physiological Uses of the Ganglionic Nervous System. By James George Davey, M.D. London : Churchill. 1853.

The Rights of Marischal College, Aberdeen, vindicated. By one of the Professors. Aberdeen : L. Smith. 1853.

Valedictory Address to Edinburgh Medical Society. By William M. Dobie, M.D. Edinburgh : Murray & Gibb. 1853.

The Medical Committee of the Charing-Cross Hospital, and Edward Smith, M.D., &c. Churchill : London. 1853.

Report on the Cholera in Jamaica. London, 1853.

Smallpox and Vaccination—Correspondence and Report, printed by Order of the House of Commons, 1853.

Quackery and its Strongholds; a Sheet from the Manchester Weekly Advertiser, August 27, 1853.

On Animal and Vegetable Fibre. By Martin Barry, M.D., &c. From Proceedings of the Royal Society.

Inflammation of the Breast and Milk Abscess. By Thomas William Nunn, Surgeon to the Western Dispensary; Demonstrator of Anatomy at the Middlesex Hospital. Renshaw, London. 1853.

On the Pathology and Treatment of Acute Rheumatism. Lumleian Lectures, delivered by James Alderson, Senior Physician to St Mary's Hospital, &c. Parker & Son, London. 1853.

Toynbee on Artificial Membrana Tympani, &c. London : Churchill. 1853.

Association Medical Journals for the Quarter.

Gazette Hebdomadaire for October.

El Porvenir Medico. Madrid, Oct. 5.

The Medical Examiner of Philadelphia for October and November.

The American Journal of Dental Science for July.

Thoughts on Cholera. By Edwin Hearne, M.B., formerly House-Surgeon to University Hospital. Churchill, London. 1853.

Asiatic Cholera: its Symptoms, Pathology, and Treatment; with which is embodied its Morbid Anatomy, General and Minute. Translated from a Paper by Drs Reinhardt and Leubensher. By Richard Barwell, F.R.C.S., &c. Churchill, London. 1853.

Transactions of New York Medical Society.

On the Remote Cause of Epidemic Diseases. By John Parkin. Hatchard. 1841.

The Remote Cause of Epidemic Diseases; or the Influence of Volcanic Action in the Production of General Pestilence. Part II. By John Parkin, M.D. 1853.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. 1st & 2d Fasciculi, folio. Churchill, London.

The Anatomy and Diseases of the Prostate Gland. By John Adams, Surgeon to the London Hospital. Second Edition. Longmans, London. 1853.

Parliamentary Reform. A Letter to Richard Freedom, Esq., by a Revising Barrister. Ridgeway, London. 1853.

The Diseases of the Heart and the Aorta. By William Stokes, Regius Professor of Physic in the University of Dublin. Hodges & Smith, Dublin. 1854.

An explanation of Chemical and Pharmaceutical Processes, with the method of testing the purity of the preparations, &c. By Dr J. C. Wittstein. Translated from the second edition by Stephen Darby, Practical Pharmaceutist. London. 1853.

On Diseases of the Rectum. By James Syme, F.R.S.E., Professor of Clinical Surgery in the University of Edinburgh, &c., &c. Third edition. Edinburgh: Sutherland and Knox. 1854.

The British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Medicine and Surgery. October 1853. Highley; Churchill.

The Chemist.

Weekly Returns of Births and Deaths.

Transactions of the Pathological Society of London, including the Report of Proceedings for the Session 1852–1853. London, 1853.

Quarterly Return of Births, Marriages, and Deaths; Remarks on the Report furnished by Assistant-Surgeon Hare of his Treatment of Fever and Dysentery during the time he was attached to the General Hospital, Calcutta, in 1849–50; submitted by the Medical Board by Order of Government. Fort-William, 1851.

Orr's Circle of the Sciences, consisting of Short Treatises on the Fundamental Principles and Characteristic Features of Scientific and Practical Pursuits. No. I.

Hooper's Physician's Vade Mecum; fourth edition, enlarged and improved, with an outline of General Pathology and Therapeutics. By William A. Guy, M.B. Cantab., &c. Renshaw, London. 1854.

The remainder of Books received in next Number.

⁂ *The Editor of this New Series is desirous of arranging for an interchange with such Journals as are not already on that footing.*

THE EDINBURGH MEDICAL AND SURGICAL JOURNAL.

No. CXCIX.

CONTENTS.

PART I.—ORIGINAL COMMUNICATIONS.

ART. I. Illustrations of Gout and the Gouty Diathesis. By JAMES BEGBIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physician in Ordinary to the Queen in Scotland, Page 209

II. On Diverticula from the Small Intestines, anatomically and pathologically considered. By JOHN STRUTHERS, F.R.C.S., Lecturer on Anatomy, Edinburgh, ... 230

III. On the so-called Fatty Degeneration of the Placenta. By JAMES M. COWAN, M.D., Edinburgh, ... 254

IV. Experiments on the Communicability of Cholera to the Lower Animals. By W. LAUDER LINDSAY, M.D., late Resident-Physician to the Surgeon Square Cholera Hospital, Edinburgh, 275

V. Suggestions in regard to the performance of Post-mortem Examinations. By W. T. GAIRDNER, M.D., one of the Ordinary Physicians of the Royal Infirmary of Edinburgh, and lately Superintendent of Morbid Anatomy in that Institution, . 302

VI. On the Displacement of the Uterus. By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E.; Lecturer on Midwifery; Physician-Accoucheur to the Royal Dispensary, Edinburgh, &c., 321

PART II.—CRITICAL ANALYSIS.

ART. I.—1. A Treatise on Diseases of the Heart. By O. B. BELLINGHAM, M.D., ... 349

2. The Diseases of the Heart and Aorta. By WILLIAM STOKES, 349

II. The Dissector's Manual of Practical and Surgical Anatomy. By ERASMUS WILSON, F.R.S., ... 376

III.—1. The Pathology and Treatment of Pulmonary Tuberculosis; and on the local medication of Pharyngeal and Laryngeal Diseases, mistaken for, or associated with, Phthisis. By JOHN HUGHES BENNETT, M.D., F.R.S.E., ... 396

2. Clinical Lectures on Pulmonary Consumption. By THEOPHILUS THOMPSON, M.D., F.R.S., ... 396

PART III. PROGRESS OF MEDICAL SCIENCE.

Luxation downwards and backwards of the three Internal Metatarsal bones; a Form of Dislocation of the Foot not previously described, 415
Case of Lodgement of a Musket-ball in the left Ventricle of the Heart, 417
Traumatic Aneurism of the left Axillary Artery; Ligature of Subclavian, 417
Amputation at the Hip-joint, ... 418
Excision of the Knee-joint, ... 418
Military Surgery, ... 419
Fibrous Tumours of the Uterus, ... 420
Fatty Degeneration of the Uterine Walls, ... 421
Fatty Degeneration of Placenta, ... 421
Adventitious Structure in Ovarian Cyst—Effects of Temperature on Contents, ... 421
Inversion of the Uterus during Labour, ... 421
Retroversion of the Uterus—Pregnancy—Death, ... 422
Instrumental Labour, Different Results of, ... 422
Medical Facts connected with Emigrant Ships, ... 422
Affection of the Heart, Thyroid Gland, and Eyeballs, ... 423
Successful Treatment of Acute Rheumatism by the Acetate of Potass, . 427
On the respective Offices of the Minute Biliary Ducts and the Cells of the Parenchyma of the Liver, ... 428

⁂ *The Editor of this New Series is desirous of arranging for an interchange with such Journals as are not already on that footing.*

PUBLICATIONS RECEIVED.

The Dissector's Manual. By Erasmus Wilson, F.R.S.

Appendix on Diseases of the Bloodvessels. By Edward Crisp, M.D., &c. Reprinted from the London Medical Examiner. Teape & Son, London, 1852.

The Irish School of Medicine as it is, and as it ought to be. An Address, &c. By Thomas S. Holland, M.D. 8vo, pp. 23. Cork, 1853.

On the relation between Therapeutics and Pathology, with especial reference to some forms of Cardiac, Renal, and Nervous Diseases. By George Johnson, M.D., &c. London.

Druit's Surgeon's Vade Mecum; a Manual of Modern Surgery. Sixth Edition. London.

The Anatomy and Diseases of the Prostate Gland. By John Adams, Surgeon to the London Hospital, &c. Second Edition. London, 1853. 8vo, pp. 178.

The Nature of Cholera investigated; with a Supplementary Chapter on Treatment, &c. By George French, F.R.C.S., &c. Second Edition. London, 1854. 8vo, pp. 152.

Some Remarks on the Medical Evidence given at the Trial of William Bourke Kirwan. By John L. Field, M.D.; with a Commentary thereon by Frederick C. Skey, F.R.S. London, 1853. 8vo, pp. 15.

On Fatty Degeneration. By the late W. F. Barlow, F.R.C.S. 8vo, pp. 92. Churchill, London.

Clinical Lectures on Pulmonary Consumption. By Theophilus Thomson, M.D., F.R.S., &c. London, 1854. 8vo, pp. 211.

Handbook of Chemistry, Theoretical, Practical, and Technical. By F. A. Abel, and C. L. Bloxom; with a Preface by Dr Hoffmann. London. 8vo, pp. 724.

The Pathology and Treatment of the Urethra, both in the Male and Female. By Henry Thompson, F.R.C.S., M.B. London.

Smallpox and Alcohol two Poisons; with Remarks on the Registrar-General's Reports of Deaths occasioned by them. By John Chadwick, M.D., Bury. 12mo, pp. 11.

The Medication of the Larynx and Trachea. By S. Scott Alison, M.D. of Edinburgh, and F.R.C.P. London. Pp. 48. Churchill, London, 1853.

The Remote Cause of Epidemic Diseases. John Parkin, M.D. 1841. 8vo, pp. 198.

The Cause of Blight and Pestilence in the Vegetable Creation. By John Parkin. 1846. Pp. 16.

The Prevention and Treatment of Disease in the Potato and other Crops. 1847. Pp. 84. By John Parkin, M.D.

The Remote Cause of Epidemic Diseases, or the Influence of Volcanic Action in the Production of General Pestilences. Part II. By John Parkin, M.D. Hatchard, London.

The Expository Lexicon of the Terms, Ancient and Modern, in Medical and General Use. By R. G. Mayne, M.D., &c. Part I. Churchill, London.

Transactions of the Medical and Physical Society of Bombay, No. 1 (new series), for the years 1851 and 1852. Bombay, 1853. 8vo, pp. 356.

The Progress of Improvement in the Treatment of Consumption, and other Pulmonary and Laryngeal Diseases, and on some new remedial means. By J. Turnbull, M.D., Physician to the Liverpool Infirmary. 8vo. Churchill, London.

The Science and Art of Surgery. By John Erichsen, Professor of Surgery in University College, &c. 8vo, pp. 950.

The American Psychological Journal. Vol. i., No. 3.

Report of the Eastern Asylum in Williamsburg, Virginia. 1849.

Essays on Asylums for persons of unsound mind. By John Galt. 1st and 2d series.

Hysterical, Hypochondriacal, Epileptic, and other Nervous Affections; their Causes, Symptoms, and Treatment. By —— Anderson, F.R.C.S. Churchill, London.

The Treatment of Obstinate Ulcers and Cutaneous Eruptions of the Leg, without confinement. By H. T. Chapman, F.R.C.S. Second Edition. 8vo. Churchill, London.

Surgical Anatomy. By Joseph Maclise, F.R.C.S. Second Edition. Fasciculus III. Folio. Churchill, London.

Climate of Italy in relation to Pulmonary Consumption; with remarks on the influence of Foreign Climate upon Invalids. By T. H. Burgess, M.D. Longmans, &c., London. 1852. 8vo, pp. 206.

What to observe at the Bedside and after Death in Medical Cases. Published under the authority of the London Medical Society of Observation. Churchill, London. 1853. 12mo, pp. 136.

Weekly Reports of Births and Deaths in London. December 24, 1853, to March 1854.

Association Medical Journals for the Quarter.

Medico-legal Observations upon Infantile Leucorrhœa, arising out of alleged cases of felonious assaults on young children, recently tried in Dublin. By W. R. Wilde, F.R.C.S., Surgeon to St Mark's Hospital, London. Churchill, 1853. Pp. 40.

Unsoundness of Mind, considered in relation to the question of responsibility for Criminal Acts. By Samuel Knaggs, F.R.C.S., &c. Churchill, London. 8vo, pp. 96.

Guy's Hospital Reports. 2d series. Edited by Edward Lloyd Birkett, M.D., and by Alfred Poland, F.R.C.S. Vol. iii., Part II., 1853. Churchill, London.

Iconographie Ophthalmologique; ou descriptions, avec figures coloriées, des Maladies de l'organe de la vue, comprenant l'Anatomie Pathologique, la Pathologie, et la Therapeutique Medico-Chirurgicales. Par J. Sichel, docteur en médecine et en chirurgie de Berlin et de Paris, etc., etc. Livraison 1, 2, 3, 4. Paris, Baillière, 1852. L'Iconographie Ophthalmologique, sera publiée en 20 livraisons, composée chacune de 28 pages de texte, grand en 4to de 4 planches d'apres nature.

Traité Elementaire des Maladies de la Peau. Par Maurice Chausit, docteur en médecine de Paris, &c., d'après l'enseignment théorique et les leçons cliniques de M. le Docteur A. Cazenave, médecin de l'Hôpital de St Louis.

Dictionnaire d'Hygiéne publique, et de Salubrieté; ou, Repertoire de toutes les Questions relatives à la Santè publique, etc. Par Ambroise Tardieu, Professeur de la Faculté de Medecine de Paris. Tome 1 et 2. 8vo, pp. 567 et 532. Paris, chez Baillière.

Retrospect of Medicine, being a Half-yearly Journal, containing a retrospective view of every discovery and practical improvement in the Medical Sciences. Edited by W. Braithwaite, Lecturer on Obstetric Medicine at the Leeds School of Medicine. Vol. xxviii. July—December 1853. London, pp. 476. 8vo.

Practical Observations on the History, Nature, and Treatment of Cholera Asphyxia, by John Coghlan, M.D., Wexford, F.R.C.S. Dublin, 1853.

Remarks on the Examination of Recruits, intended for the use of young Medical Officers on entering the army. By H. H. Massy, A.B., M.B., 4th Light Dragoons. Churchill, London. 1854.

Half-yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M.D. Cantab. &c., and C. B. Radcliffe, M.D., London, L.R.C.P., &c. Vol. xviii. July–Dec. 1853. Churchill, London.

The Odontalgist, or how to preserve the Teeth, cure Toothache, and regulate Dentition from infancy to age. By J. Paterson Clark, M.A., Dentist Extraordinary to Prince Albert. Churchill, London, 1854.

British and Foreign Medico-Chirurgical Review, or Quarterly Journal of Practical Medicine and Surgery. No. 25. Jan. 1854. Churchill and Highley.

The Chemist for the Quarter.

New Hampshire Journal of Medicine. No. 1. Edited by George Hubbard, M.D. Parker Lyon, Concord.

The Indian Annals of Medical Science, or Half-Yearly Journal of Practical Medicine and Surgery. October, 1853. No. 1. Pp. 375, 8vo. Lepage, Calcutta and London.

Introductory Address to the Students of the Extra-Academic School of Medicine, Edinburgh. By R. J. Mackenzie, M.D., F.R.C.S.E, Lecturer on the Principles and Practice of Surgery, Surgeon to the Royal Infirmary, Edinburgh.

Important Sanitary improvement in House-painting. Ry D. R. Hay & Co., Edinburgh. Pp. 8.

Disease of the Rectum. By Richard Quain, F.R.S., Professor of Clinical Surgery in University College, London. 1854. Post 8vo, pp. 285.

The Common Sense of Cholera. By a Practical Practitioner. Churchill, London, 1854. 12mo, pp. 68.

Functional and Sympathetic Affections of the Heart; a paper read before the Society of Statistical Medicine. By John W. Corson, M.D., &c. New York, 1854.

Annual Report of the National Freehold Land Society. 1853.

Monthly Journal of Medical Science. Sutherland & Knox, Edinburgh.

The American Medical Monthly. Vol. i. Nos. 1 and 2. New York, G. B. Putnam & Co.

Quarterly Return of Marriages, Births, and Deaths,

Births, Deaths, and Causes of Death during 14 years, on a sheet published by authority from the Weekly Reports.

Our British Tritons. By John Higginbottom, F.R.S. Taylor & Francis, London. 8vo, pp. 16, with plates.

The Dublin Quarterly Journal of Medical Science, No. 33. February 1854. Hodges & Smith, Grafton Street, Dublin.

The Dublin Hospital Gazette. Nos. 1, 2, & 3. Vol. i. 1854.

Comparative Anatomy. By C. Th. Siebold, and H. Stannius. Translated from the German; edited, with notes and additions, recording the recent progress of the Science. By Waldo J. Burnett, M.D. Vol. i. Anatomy of the Invertebrata. Trübner, London; Gould & Lincoln, Boston, U. S. 8vo, pp. 470.

The Medical Examiner of Philadelphia.

The New York Journal of Medicine and the Collateral Sciences. Vol. xii. No. 1. January. Purple & Smith, New York. Pp. 158.

The Modern Syphilitic Diseases, both Primary and Secondary. By Langston Parker, Surgeon to the Queen's Hospital, Birmingham. Churchill, London, 1854. Pp. 345.

A Treatise on the Diseases, Injuries, and Malformations of the Rectum and Anus. By T. J. Ashton, Surgeon to the Blenheim Dispensary, &c., &c. Churchill, London, 1854. 8vo, 356.

Indigestion, and certain Bilious Disorders often conjoined with it. By George Chaplin Child, M.D., consulting Surgeon to the Westminster General Dispensary. Second Edition. Pp. 200. Churchill, London.

The Elements of Natural Philosophy, or an introduction to the Study of the Physical Sciences. By Golding Bird, M.A., M.D., &c., and Charles Brooke, M.A., M.B., &c. 4th Edition, revised and greatly enlarged. Churchill, London, 1854.

Report of Epidemic Cholera, drawn up at the desire of the Cholera Committee of the Royal College of Physicians, London. By William Baly, M.D., and William Gull, M.D., members of the Committee. Churchill, London, 1854.

Class-Book of Botany, Part II., being an Introduction to the Study of the Vegetable Kingdom. By J. H. Balfour, M.D., Professor of Medicine and Botany in the University of Edinburgh. 8vo, pp. 1114. A. & C. Black, Edinburgh, 1854.

The Asylum Journal, published by authority of the Association of Medical Officers of Asylums and Hospitals for the Insane. No. 3. Feb. 1854. Pp. 12.

A Clinical Introduction to the Practice of Auscultation, and other modes of Physical Diagnosis in Diseases of the Lungs and Heart. By H. M. Hughes, M.D., F.R.C.P., &c. Longmans, London, 1854. 12mo, pp. 302.

The varieties of Pock delineated and described by Walter Cooper Dendy, &c., &c. Pp. 31. Highley, London, 1853.

A short Exposition of the Circulation and Nervous System, with reference to Disease and Treatment. By C. Hamilton Bell, F.R.C.S.E., formerly of the Madras Medical Establishment. Pp. 67. Maclachlan & Stewart, Edinburgh; Churchill, London.

The Microscope in its special application to Vegetable Anatomy and Physiology. By Dr Hermann Schacht, translated by Frederick Curry, Esq., M.A.; with numerous illustrations. Pp. 130. Highley, London.

Physiology in Harmony with the Bible respecting the value and right observance of the Sabbath. By James Miller, F.R.S.E., Professor of Surgery in the University of Edinburgh. 12mo, pp. 78. Johnston & Hunter. 1854.

Elements of Human Anatomy, General, Descriptive, and Practical. By T. G. Nicholson, M.D., Demonstrator of Anatomy in the Medical Department of the University of Louisville. Lippencoll, Grambo, & Co., Philadelphia.

Statistical Reports on the Health of the Navy from 1837 to 1843. Part II. East India Station. Presented to Parliament by Her Majesty's command, and ordered by the House of Commons to be printed, June 1853. Folio, pp. 94.

The Dublin Medical Press for the Quarter. Published weekly by Thomas Decy, 15 Molesworth Street, Dublin.

The Journal of Physiological Medicine and Mental Pathology. Edited by Forbes Winslow, M.D. No. 25. January 1854. Churchill, London.

The American Journal of the Medical Sciences. Edited by Isaac Heys, M.D., &c. Trübner, London; Blanchard & Lea, Philadelphia.

Dr Conquest's Outlines of Midwifery, intended as a text-book for Students and a book of reference for junior practitioners. A new Edition. By James M. Winn, M.D., &c., &c. 12mo, pp. 323. Longmans. 1854.

On the use of the artificial Membrana Tympani, in cases of deafness dependant upon perforation or destruction of the natural organ. By Joseph Toynbee, F.R.S., &c., &c. Second Edition. Churchill, London. 8vo, pp. 31.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1st JANUARY 1854.

PART I.
ORIGINAL COMMUNICATIONS.

ART. I.—*Illustrations of Gout and the Gouty Diathesis.* By JAMES BEGBIE, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physician in Ordinary to the Queen in Scotland.

SYDENHAM has painted gout with the hand of a master; and all who have followed him in the task (and there have been many of the highest class) have, more or less faithfully, copied the picture. But the portrait thus drawn has been that of regular gout, and we still desiderate a fellow in that of the gouty diathesis. While Sydenham and others have made us familiar with the leading features and the more prominent characters of the disease, so that what is designated a fit of gout hardly requires a diagnostic mark, we are still ignorant of its real nature, and another artist must arise to draw the lines of its more obscure recesses, and portray its curious essence.

Sydenham, writing in 1683, has said that "either men will think that the nature of gout is wholly mysterious and incomprehensible, or that a man like myself, who has suffered from it thirty-four years, must be of slow and sluggish disposition not to have discovered something respecting the nature and treatment of a disease so peculiarly his own." It will be a bright age that will bring the charge of "slow and sluggish disposition" against the illustrious Sydenham; and his admirers may be well reconciled to the want of discovery when they find Sir Henry Holland in our own day commencing his thoughtful reflections on the disease in question, by quoting the words of Aretæus, to the effect, that the gods alone were acquainted with the real nature of gout, that men knew it only by external characters; and acknowledging that even now, with the lapse of nearly eighteen centuries between, it would be difficult to state our knowledge of the intimate nature of this disease in very different terms.

It is certainly not to the field of observation being limited, that our want of knowledge is to be traced. The disease is of very frequent occurrence; and the notion of its rarity in particular times and places, has arisen, no doubt, from the fact adverted to by Dr Gairdner, one of the latest and best writers on the subject, that we are apt not to consider a man as gouty, unless he has suffered under a regular fit of the disease, a fact which was long ago noted by Captain Grant in his "Observations on the Bills of Mortality," when he remarked that "there dies not one of a thousand of the gout, although I believe that more die gouty." I concur most entirely with the former author in his belief that the gouty diathesis is often very perfectly developed in individuals who never see its local manifestations, and that the strumous is not more frequent than the gouty habit.

The study of gout has been rendered perplexing and unprofitable, by the endless variety of forms and species which have been classified by systematic writers; and every practitioner must have again and again lamented of how little avail these minute distinctions were in guiding him through the varying and obscure phenomena of the disease, or in directing him to a rational mode of treatment. Nor is it difficult to arrive at the conclusion, that our knowledge of the disease would have been more exact, if physicians had been led to recognize but a tithe of the typical forms in which it is presented to our view, and gained their experience of its nature and treatment under the names only of acute and chronic, or of sthenic and asthenic gout.

The terms podagra and arthritis are unhappy names for gout; and their adoption may have led many to overlook and undervalue the constitutional disorder, the true disease, in the more ordinary form of its local manifestation. Gout is a blood disease: and may disclose itself in every organ of the body, and complicate and involve every disturbance of the system. It may visit every part and every texture, from the crown of the head to the sole of the foot, and molest and vitiate every function appertaining to life. We discover it in the head, where, not unfrequently, in the form of intense and continued headache, its first manifestation in the system is perceived; we trace it in fits of giddiness, and in transient affections of sight and hearing; and often we are called to acknowledge its presence in apoplectic and paralytic seizures, and its intimate association with lethargy and coma in connection with serous effusion or other cerebral disease. We discover it in the spinal sheath or spinal marrow in the form of paraplegia; and in every part of the nervous system in the shape of neuralgia, whether under the name of lumbago or sciatica, of hemicrania or toothache, or any of the numerous forms of tic douloureux. We find it in the ear, and we trace it in the throat. We discover it often in the eye, where, under the name of ophthalmia, or iritis, or sclerotitis, it lights up intense and alarming inflammation, which not unfrequently terminates in the destruction of vision. It plays a part, a most important part, in many of the most formidable affections of the chest. We detect it in many instances in pneumonia, and particularly in pleurisy, and still oftener in bronchitis; and it is the source of many cases of hydrothorax. Gout is the cause of great embarrassment to the heart and bloodvessels, and, perhaps more than any other agent lays the foundation of irreparable mischief in these vital parts. It is the immediate cause of some acute attacks of endocarditis and pericarditis, and the proximate cause of numerous chronic affections of the heart and arteries. Gout has frequently its seat in the organs of digestion, complicating their functional derangements, hurrying on their structural diseases, and terminating their organic lesions: witness its frequent connection with disorders of the liver, and its influence on their progress to visceral obstruction and incurable disease. It is intimately connected with the disorders of the urinary organs, is frequently the origin of granular degeneration of the kidney, of puriform discharge from the urethra in the form of genuine gonorrhœa, and the well-known source of stone in the bladder.[1] Gout lurks in many of the functional derangements of the uterus, and discovers itself in some of its organic diseases. It is the fountain of many hemorrhages and many fluxes. It is the origin and essence of many cutaneous eruptions, and in the joints the fruitful source of crippling, lameness, and deformity. Everywhere its ravages are discernible by those who have studied its course,—a course, however, which is too often run unperceived and unsuspected.

---

[1] It has been remarked that the gouty parent often transmits the nephritic affection without the gout to his offspring, and that this frequently happens on the *female* side. It has also been remarked that the urinary concretions of the gouty belong to the lithic acid diathesis. It has not been sufficiently, if at all, noticed that they often belong to the oxalic. I have two gouty patients now under my care, who derive their disease from hereditary descent. The eldest son of the one, before his thirtieth year, has had frequent attacks of nephralgia, and lately voided a urinary concretion which was found to con-

It is of the first consequence to attend to what are called the premonitory signs of gout; for it is chiefly at that season that we can employ with advantage remedial means, and the neglect of these signs never fails to be followed by those which are characteristic of progressive constitutional disorder. The improvement in the general health which has long been remarked to follow the earlier attacks of gout, and the notion too generally entertained that a fit of the disease relieves the system of other disorders, have led to supineness at one time, and to dangerous and fatal experiment at another. Some, trusting too much to what is believed to be the salutary nature of gout, have been content to look on during the course of symptoms which are acknowledged as precursory of the coming fit; while others, more daring, have too often attempted, perhaps successfully, to force it on by prescribing continued indulgence in the vicious habit which has engendered the existing disorder, forgetting that gout is a morbid poison which may at any time seize on a vital part, and which cannot accumulate in the blood without imminent hazard to life, or sure and certain deterioration of health.

In correspondence with usually received opinion, though contrary to the observation of a late writer, my own experience leads me to the conclusion that dyspeptic symptoms invariably precede even the first invasion of gout; and that these occur with even greater regularity at this than at any subsequent attack of the confirmed disease. Indigestion and malassimilation are the first steps in the great constitutional disorder which, aided by hereditary predisposition, is developed in the gouty diathesis. It is true, no doubt, that the first attack of the disease often comes on suddenly and unexpectedly in the midst of apparently perfect health, and in the full play of vigorous exertion; yet, I have never known one so seized who did not acknowledge that at a period not very

---

sist of the oxalate of lime. The eldest daughter of the other, while still a girl of twenty, has had frequent severe attacks of a nephritic kind, accompanied by hæmaturia, and in her case the urine is persistently loaded with large crystals of the same earthy matter.

distant, and for a time not very limited, he had been affected with precursors which to a less unsuspecting and more experienced eye foretold the coming invasion. He may at the moment have forgotten, but his memory can readily recall, the sufferings he lately endured in connection with stomach and nervous derangement, and will be able to speak of the pain, and the acidity and flatulence of a recent indisposition, and descant on the fluttering and irregularity of the heart's action, and on the disturbance of sleep and irritability of temper which accompanied them; nor will he have failed to remark the state of the bowels, and the lateritious sediment of the urine on the occasion referred to—symptoms which, in all probability, are about to be renewed, if not already existing in connection with his present attack.

The gouty diathesis being once developed, the attentive observer will discover many indications of constitutional disorder which are referable to it. Besides the dyspeptic symptoms usually recognized, there is often observed remarkable irregularity in the heart's action, accompanied by painful uneasiness over the left side of the chest, and pangs shooting occasionally to the shoulder and arm, similating very closely those of angina pectoris, and rendering it difficult or impossible for the patient to lie on the affected side. There is also in most cases much mental uneasiness and hypochondriacal depression, the patient manifesting undue anxiety as to the nature and issue of his ailments, and much peevishness and fretfulness to all concerned in the treatment of them. Along with these dyspeptic and nervous symptoms, there is often remarked a peculiar *aura* or rapid twittering motion under the skin, as it were, chiefly in the back and limbs. I have frequently known this sensation occurring at intervals, for many weeks or months, as a precursory sign of gout in the extremities. The skin is early affected in the gouty diathesis. It becomes dry and harsh, and in some places hot and itching; the soles of the feet and palms of the hands are particularly obnoxious to these conditions: urticaria, erythema, eczema, and psoriasis are ordinary attendants on the gouty habit.[1] At this stage I have frequently remarked the deposit of the oxalates and oxalurates in the urine, and have found them afterwards displaced by, or alternating with, the lithates and lithic acid.[2] The kidney begins to labour, and the function of the liver is impeded. Swelling and uneasiness of the right hypochondrium are observed, the bowels are found torpid and irregular, the secretions morbid; and hemorrhoidal affections begin, for the first time, to manifest themselves. It is a curious fact in the history of gout, that its local manifestation in the foot or hand is often impeded, interrupted, or altogether prevented, by the occurrence of a hemorrhage; and that, on the other hand, the gout is often successful in displaying itself in a regular form, and in so doing, bringing to a close a long-continued hemorrhagic discharge. Hæmoptysis, hæmatemesis, and hemorrhoids, have thus been found to yield only to an attack of gout. Examples of these vicarious discharges, and anomalous conversions, will be found in the sequel; and as it is not my present purpose to describe gout or the gouty diathesis, or enter on the question of their nature, I shall proceed at once to the object of this paper, namely, the illustration of the disease as it has occurred to me in practice, noting chiefly those signs or features which may be considered characteristic of its natural history, and avoiding any detail of its more ordinary form of manifestation in the foot or hand.

---

[1] The offspring of gouty parents are often found affected with these skin disorders, without any other indication of the gouty diathesis; and it is worthy of remark, that in them the disorders are found amenable to a plan of treatment suitable to gout.

[2] Dr Prout has remarked that the oxalate of lime calculus occasionally *follows* an attack of gout.

Dr Graves has justly remarked that there is no proposition in pathology better established than that there exist several constitutional affections capable of generating and modifying local inflammatory action; and that local inflammations depending on a constitutional cause, are subject to very different laws from those which regulate the phenomena of common inflammation.

The cases which follow illustrate this in the case of gout as the constitutional origin of some local inflammations.

Case I.—At the age of thirty-five, and at the close of autumn, some twenty years ago, Dr B. had his first attack of gout. He had hereditary claims to the disease; and had long been familiar with the dyspeptic derangements which usually precede or accompany its development. More particularly, he had often suffered from cardiac pain and uneasiness, from flatulent distension and acidity, from broken rest and depressed spirits. But with these exceptional ailments, he enjoyed good health, and continued in the discharge of professional duty. At the period referred to, he was undergoing more than usual fatigue, and on returning from a long walk in the country, was seized, suddenly and unexpectedly, with acute pain in the inner part of the left knee-joint, immediately in the line of articulation. He was instantly brought to a halt, and a minute or two elapsed before he was able to resume locomotion, which was now conducted homewards with considerable difficulty, in consequence of a painful sense of heat, swelling, and stiffness of the parts. In the course of a few hours the synovial capsule was distended with fluid, and motion was painful and restricted. The effusion increased still more, and early next morning, after a restless night, the knee-joint was greatly swollen and painful, without discoloration, however, of the surface, except what arose from the turgid veins around. Considerable constitutional disturbance was lighted up, the skin became hot, the pulse quick and irritable, the tongue loaded, the bowels confined, and the urine scanty and high-coloured; much restlessness of mind, and irritability of temper accompanied these symptoms. Dr Abercrombie was in attendance; but the nature of the illness was not recognized, and the latent poison was not then discovered. In conformity with the practice of the day, the strict antiphlogistic treatment was adopted; repeated leeching, brisk purging, and severe starvation were prescribed and followed, but little or no relief resulted. The constitutional derangement continued day after day; and weeks and months elapsed before the local pain and swelling yielded to nature what had been refused to counter-irritants and other resources of art. Five years elapsed, with occasional attacks of dyspeptic derangement, when at the same season of the year, and under similar circumstances, Dr B. had a renewal of the former attack; but in the opposite knee-joint. Depletion in this instance was avoided, and under the employment of rest, warm applications, and the internal use of colchicum, the pain, swelling, and other symptoms rapidly declined, leaving the joint for a time weak and crippled. A few more years intervened, and early in the winter of 1849, when suffering from severe domestic affliction, he was again subjected to a return of pain, stiffness, and rapid effusion into the knee-joint first affected. The constitutional disturbance was as formerly; and under fomentations, and the diuretic effect of colchicum, both it and the local symptoms rapidly disappeared. The subsidence of the pain and swelling were simultaneous with the physiological effects of the remedy. Years have now gone by, and no severe fit of gout has occurred; but the deranged digestion, and the shooting pains in the heel, in the instep, and in the joints of the fingers, particularly after any departure from the rules of temperance, indicate too clearly that a diathesis prevails which requires watchful caution to mortify and overcome.

CASE II.—For many years I have been consulted by Mrs F., a lady approaching the age of seventy, for yearly, or half-yearly, attacks of gout in the foot, and for dyspeptic derangement, connected with a marked gouty habit transmitted by inheritance. By great prudence and circumspection in the matter of diet and regimen, these attacks have been limited both in duration and severity; and though the fingers demonstrate the chalky formations, and the heart has experienced the prejudicial effects of the circulating poison, and valvular disease and its consequences indicate very clearly the progress of events, there is still much to render life useful and desirable. The ball of the great toe, or instep of one foot, seldom both together, have hitherto been the chief seats of the disease; but on the last two occasions it has manifested itself in the synovial capsule of the knee-joint, attacking it through the night with severe pain, and completely disabling its victim by the rapid effusion, and consequent painful attempts at motion. On both occasions the affection has been successfully treated and speedily removed by the application of warmth and moisture by means of the spongiopiline, and the administration of such doses of the wine of colchicum, as experience had proved to be beneficial when the toe or instep was the seat of gout.

CASE III.—A pampered English maid, about the age of forty-five, long accustomed in her master's house to generous living on meat and malt, applied to me for relief from the premonitory symptoms of an ailment which her habits had long invited, and which indicated its approach by various painful affections of the stomach, and pangs in the chest. She had a speedy relief from them all after a night of severe pain in the right knee-joint, followed by copious effusion into its capsule in the course of the next morning. An active purge, continued warm applications, and the use of colchicum and neutral salts, speedily relieved the joint and the system together, for a time at least, of the material cause of the malady in both.

The eye is often the seat of inflammation, deriving its character from constitutional taint; and the gouty diathesis is well known to excite its specific inflammation in the different tunics. Of these affections none is more serious or alarming than iritis.

CASE IV.—Many years ago I attended, along with an eminent oculist, a young lady of twenty-five, belonging to a family highly predisposed to gout, but who had never suffered from any of its external manifestations in her own person, though her sisters, to the exclusion of her brothers, had suffered largely from its local effects. These parts of family history were unknown to us at the time of our attendance, which was required in consequence of a sudden and violent attack of iritis coming on in the course of the night, and without any assignable cause. Regarding the affection as one of pure iritis, the most prompt and energetic measures were had recourse to, cupping and leeching, calomel and opium, darkness, belladonna, and every available sedative, were all put in force, but the inflammation was not controlled, and effusion into the anterior chamber, adhesion of the iris, and contraction and obliteration of the pupil were the consequences. Melancholy to relate, the other eye, during the progress of these events, was attacked with the same form of disease, which, resisting all our remedies (among which, however, colchicum was not included), renewed the painful history of the first invasion, and closed its course in destruction of the eye, and in total blindness.

I know in Edinburgh, where gout is said to be an extremely rare disease,[1] at least one other case a parallel to the above. A gentleman, now fifty years of age, one of a family who have had sad experience of the damaging power of gout, who for many years has been deprived of sight, and can trace to iritis originating in the prevailing diathesis of his family, the privation he now so patiently endures.

In these cases of gouty iritis we learn from our best writers on the eye, that the disease neither requires nor admits that free use of mercury which is so advantageous in other forms of the complaint. Mr Lawrence declares that he has seen cases in which mercury, carried to salivation, has been injurious rather than beneficial. He is favourable to the use of colchicum. Bloodletting requires to be employed with caution, and the treatment regulated by the prevailing diathesis.

Gout is acknowledged to be the origin occasionally of tonsillitis, of laryngitis, of bronchitis, of pleurisy, and pneumonia, and the writings of the many authors who have treated of gout abound with examples of these affections in intimate relation with it. It is remarkable that it is so little suspected to be the cause of inflammation of the membranes of the heart. Dr Latham, writing in 1845, has made this remarkable admission: "My own experience of pericarditis is mainly derived from what it is as an accompaniment of acute rheumatism. I have seen the disease, indeed, under other circumstances; but it has been very seldom, so seldom, indeed, that I have little acquaintance with other conditions, external or internal, conducing to it, I can neither tell whence to look for it, nor when to expect it, except when it occurs as a part of acute rheumatism."[2] The late Dr John Taylor, however, was at the very time engaged in conducting an inquiry into the causes of pericarditis, the result of which has since been published, and been the means of throwing much additional light on this still obscure subject. And to the very valuable observations of Dr Taylor on Bright's disease as a frequent cause of pericarditis, we have to add those of Dr Scott Alison and others, on scarlatina as productive of similar effects. Other blood diseases have been noticed as the occasional source of inflammation of the membranes of the heart, and more particularly typhus and small-pox; but I am not aware that gout has been referred to as the cause of these affections.[1] This is the more remarkable, as the close resemblance between it and rheumatism as blood disorders, and their known tendencies to excite inflammatory action in certain tissues,[2] might have led us from analogy to conclude that the heart, which is so often the seat of rheumatic inflammation, should be also that of the inflammation arising from gout. Dr Watson, however, tells us that "in gout, though many functions suffer, there is no tendency to carditis." We have still however much to learn of the history of pericarditis, and of endocarditis, independent of rheumatism and renal disease, and have a wide field of discovery open to us in the investigation of the etiology of the milk-patches or white spots on the surface of the heart, which have engaged the attention and perplexed the minds of pathologists (and which Mr Paget has now demonstrated to be the products of inflammation), as well as of the various pathological conditions of the inner lining of the different cavities, and of the vegetations and wrinkled induration of the valves, which still baffle the speculations of the anatomist. Lobstein traces most of these morbid conditions of the endocardium to gout as their real source, and Hasse has acknowledged that the evidence he adduces is, in many instances, most convincing.

---

[1] See Review of Gairdner on Gout, in Monthly Journal of Medical Science for January 1850.

[2] Lectures on Diseases of the Heart, vol. i., p. 136.

I have long entertained the notion that the extreme frequency of pericarditis and of endocarditis, as demonstrated in these morbid alterations of the heart's structure found after death, is to be traced to the prevalence of the gouty diathesis. We know how slight and transient is the pain in the small joints of the fingers, which is occasioned by the gouty inflammation and the gouty deposit there; and how many are the years that elapse before these indications of diseased action reveal themselves to our senses by thickening, and stiffness, and swelling. May not the transient pangs in the cardiac region which so constantly affect the victims of the gouty habit, originate in the specific inflammation when it is in the act of depositing lymph which is, by slow yet sure degrees, to form the milky patches, and the small adhesions, and the puckered valves and slender vegetations which we find so often after death, and which teach the lesson that there is no structure of the body more liable to inflammation than the investing and lining membranes of the heart, though the evidence of diseased action has neither been detected nor perhaps been detectible during life?

---

[1] Corvisart, however, has remarked that rheumatic and gouty affections are frequent causes of adhesion of the pericardium.

[2] The material of gout appears certainly to have a greater affinity for the synovial and mucous surfaces; that of rheumatism for the fibrous and serous membranes. Hence we find in the former, besides the affection of the joints, an intimate connection with tonsillitis, bronchitis, gonorrhœa, and other inflammatory affections of the mucous membranes; while in the latter the most frequent connection is with pleurisy, pericarditis, and peritonitis.

There are few, or perhaps none, of the victims of gout we have seen who have not sooner or later given evidence of organic disease of the heart or arteries, progressive in its nature, yet often so slow and insidious that its existence has only been detected when some accidental illness or other circumstance has called for an examination; and all must acknowledge that the fatal diseases of the gouty are those consequent on alteration of the heart's structure, originating in valvular imperfection, followed by hypertrophy and dilatation, and terminating in palsy or apoplexy, or serous effusion in the brain at one time, or in asthma or hydrothorax at another, or in disease of the liver and general dropsy, or in some of the recognized affections connected with vascular congestion and obstruction.

But we are not left to conjecture in regard to the influence of gout in the production of inflammation of the membranes of the heart. I have seen at least two instances in which pericarditis has been allied with gout by ties as intimate and indubitable as I have ever known it to be connected with acute rheumatism; and feel satisfied that, when there is a more distinct recognition of the gouty diathesis, and a clearer diagnosis of its characters, it will be found to play a most important part in many of the cardiac inflammations which we are called to treat; and be acknowledged as the foundation of many of the morbid appearances which we discover after death.

CASE V.—I had for many years an opportunity of watching the development of the gouty diathesis in a young lady whose family history afforded more than one proof of the prevalence of the disorder. Her father had died of heart affection, which there is reason to believe was associated with gout. Her sister immediately senior to herself had for many years been affected with frequently recurring attacks of psoriasis, accompanied with gastric and hepatic derangement, which yielded only to remedies of an anti-arthritic character; and a maternal uncle had been for many years the victim of gout in its purest and most undisguised form, and to such excess, that the fits, in frequency and severity, baffled all attempts to control them. With so much of her predisposition known, Miss T. came under my care about two years previous to her death, at the age of twenty-five, with symptoms of obscure heart affection, alternating with those of gout in the small joints of the fingers, and the usual derangement of the stomach and disorder of the renal and biliary secretions. At this time there existed a slight endocardial murmur with the first sound, which after repeated examination I satisfied myself was not anemic; and it was persistent through life. She frequently complained of pain in the region of the heart, and of preternatural impulse, and these were always aggravated by exertion in walking, or by ascending a stair. Her general health was otherwise good, and remained undisturbed save by the occasional returns of the gouty pain and swelling of one or more of the metacarpal or finger-joints, and of the cardiac uneasiness and palpitation. The attacks yielded to the alkaline salts, with small doses of colchicum, and in the interval benefit appeared to be derived from the cod-liver oil. Two years had passed, with frequent recurring fits of illness, when at the close of January 1849, Miss T. was seized with severe pain in the præcordial region, with hurried breathing, and more than usual pulsation. The pain and swelling of the fingers were at the time of moderate character, and quickly disappeared. When seen on the second day of her illness (for long acquaintance with symptoms of a mitigated kind had made her too regardless of her present state) she was found lying on her back, with her shoulders raised in bed, her countenance pale and anxious, her breathing hurried, and performed with pain, her pulse rapid and small, her skin hot and moist. There was pain over the region of the heart, increased by pressure between the ribs, and upwards from the epigastric region. There was extended præcordial dulness, and distinct rubbing sound, in which was lost the ordinary endocardial murmur. The rubbing sound also gave way. The mind continued clear. No remedy availed. Repeated leeching, the rapid introduction of calomel and opium, together with colchicum and other adjuvants, appeared to produce no sensible effect on the still more rapid effusion, and with feeble and uncountable pulse, and tumultuous, trembling action of the heart, she sank on the third day. She died, I doubt not, of pericarditis with extensive effusion, caused by the specific poison of gout in the blood, but no *post-mortem* examination was permitted to reveal the full amount of the damage thus sustained.

CASE VI.—On a former occasion, when speaking of the disorder of the digestive and assimilating functions which precedes the paroxysm of rheumatic fever, I referred to the case of a young lady as one in point. Its applicability was doubted at the time; and farther insight into the family history leads me to believe that the disease was not rheumatism but gout. I give the case as recorded at the time referred to. The subject was a young lady of twenty-one, who had been ailing for some time before I was called in to see her. She was then pallid in countenance, and depressed in spirits. She owned no illness beyond a feeling of loss of strength, an occasional aching head, a want of appetite, and unrefreshing sleep. Her tongue was foul, her bowels disordered, and her urine scanty, and greatly loaded with lithates. The catamenia were suppressed. These symptoms went on from day to day. No local pain, no febrile movement, threw light upon their real nature, and no family predisposition pointed to their true character. By and by, a degree of fever was lighted up, and then a degree—a slight degree—of redness, pain, and swelling of the right ankle-joint, proclaimed itself, and revealed the latent disorder which lurked within, and which was now developed in great intensity. The fever, however, exceeded far the amount of articular complication; the endocardium by and by declared its participation in the constitutional derangement by the distinguishing *murmur*, and ere long the pericardium also, through the *to-and-fro* sound. No remedy availed to save life, though suffering was protracted—painfully protracted—for many weeks, when tumultuous palpitation, and laborious breathing, and dropsical swelling, closed the scene. No examination of the body was permitted, but the records of many such cases can declare the havoc that was wrought.

The only joint affected in this case was the right ankle. The swelling, pain, and redness were those of gout; but the age and sex of the patient, combined with the absence of hereditary predisposition, led me to regard the attack as one of rheumatism. Subsequent reflection, however, and better acquaintance with the family history, leave no doubt on my mind that the disease was gout.

In the fourth volume of the *Transactions of the College of Physicians of London*, Dr Haygarth details the case of Governor B., a gouty old gentleman of seventy-five, who had for six weeks been affected with pain in the wrists and knees, accompanied with swelling. These symptoms had considerably abated when he was seized with violent pain in the left side of the chest, exactly in the region of the heart, with great irregularity and rapidity of pulse, and such extreme anguish that it occasioned profuse sweats. After bleeding and blistering had been ineffectually employed, the application of sinapisms to the wrists produced speedy relief. The wrists became very painful and much swelled, and the disorder of the heart was soon and considerably relieved. Dr Haygarth and Dr Stewart regarded the case as one of carditis arthritica. In the absence of all record of the physical signs, many may be disposed to doubt the propriety of placing the symptoms under that name, and may regard them only as indications of functional disorder of the heart, in connection with what is called retrocedent gout.

The history of enteritis has sometimes been written in connection with gout.

CASE VII.—Many years ago I attended, along with the late Dr Abercrombie, one of the clergymen of the city, a gentleman of sixty years of age, of spare habit of body, and abstemious in food and drink, but notwithstanding a great sufferer from gout, which had descended to him by hereditary transmission, and visited him in the pure and regular form twice or thrice a-year. The usual times of attack, he had remarked for many years, were on the occasion of more than ordinary exertion, particularly at the communion seasons.[1] After a longer absence of the fit than he had been accustomed to experience, he was seized one afternoon with pain in the abdomen, augmenting in severity, with sickness, vomiting, and obstinate constipation. These symptoms increased in urgency, and were attended by fever, quick small pulse, great thirst, and tenderness over the belly increased on pressure. The usual remedies were had recourse to, but the symptoms did not yield. I suggested to Dr Abercrombie that the attack might be regarded as a form of gout, and might possibly be found to be influenced by the operation of colchicum, from which our patient had always obtained marked and speedy relief, when the malady centred in the foot. Full doses of the wine were accordingly administered at short intervals; copious diuresis resulted; the obstruction of the bowels gave way; and speedy relief to all the symptoms followed.

The alliance of gout with nephritis has often been noticed; and with nephralgia and stone the connection is close and frequent. Gout is sometimes the forerunner of Bright's disease or albuminuria.

---

[1] An eminent cabinet minister, a martyr to gout, informs me that the most severe fits of his malady usually follow his more laborious duties on the floor of the House of Commons, at a time when he is obliged to be particularly temperate in matters of diet.

CASE VIII.—I was requested by Mr Craig of Ratho, to visit with him a gentleman aged sixty, a member of a gouty family, who had been for years suffering from dyspeptic symptoms, which required great prudence in diet to mitigate and subdue. At the same time there were frequent attacks of pain and uneasiness in the feet, and neuralgia in the lower extremities, but a fit of gout had not developed itself. The urine at this time gave no trace of albumen, but was of acid reaction, and deposited the lithates. He then began to be affected with headache, and indistinct vision, and on examining the urine it was found slightly albuminous. The renal disorder advanced, the cerebral symptoms keeping pace with its development, and in the course of three months he died comatose.

CASE IX.—Some months ago I was requested by Mr Tait of Dunse to see a gentleman aged forty-five, suffering under a protracted attack of gout, which had affected the ankle and knee-joint as well as the tunics of the eyeball. Obscure indications of the disease had previously manifested themselves, and it was hereditary in the family. Mr Tait had found, on an early examination of the urine, that the oxalates and oxalurates existed together with the lithate of ammonia; but as the disease declined these disappeared, and were replaced by traces of albumen gradually becoming more and more distinct. The albuminuria has now been persistent for many months; the general health has, notwithstanding, improved, chiefly under the use of cod-liver oil, and there has been no return of gout. It would be interesting and instructive to find that a new fit of the gout suspended or removed the morbid action now going on in the kidney.

The gouty diathesis plays a most important part in some of the functional and organic diseases of the uterine system. Its connection with menorrhagia, leucorrhœa, and dysmenorrhœa is well known. The following case illustrates its operation in a manner to which I have not hitherto seen any reference.

CASE X.—Mrs M. married early in life, and became the mother of a large family. She enjoyed excellent health, with the exception of such deviations as arose from hysteria at one time, and frequent returns of hæmoptysis at another. The former affection, often severe and protracted, distressed her chiefly during the earlier years of her married life, and subsequently gave way to the hæmoptysis as a vicarious discharge. They alternated for years, and ultimately the nervous symptoms declined, and the attacks of spitting of blood became more frequent and more severe. The quantity of expectoration was never alarming; it was always florid, and mixed with thin mucous or serous fluid. Its appearance was not influenced by the state of the catamenia or by pregnancy. Its approach was always indicated by a sense of tightness and oppression, sometimes of pain, in the upper part of the right side of the chest. There was seldom any febrile disturbance. The blood was expectorated by an effort or slight cough, but no other symptom of pulmonary irritation was discovered beyond a mucous rale confined to the part indicated by the sense of tightness. The attacks of hæmoptysis recurred at uncertain periods, sometimes six or seven times a year, and lasted from three to seven or eight days at a time. They had recurred in this manner over a period of twenty years and upwards, when about the disappearance of the catamenia they also ceased, and were succeeded by symptoms which Dr Simpson, who now saw the case, regarded as those of spurious pregnancy. After a fruitless gestation, indications of irritation in the right ovarium presented themselves. Constitutional disturbance arose, and on examination pelvic cellulitis was discovered. While this newly-developed affection was under treatment, to which, however, it was not amenable, indications of gout presented themselves in the middle joints of the fingers, and the urine was found greatly loaded with lithates. Under a plan of treatment adapted to the prevailing constitutional derangement, including the neutral salts and colchicum, the general and the local symptoms gradually declined, and after four or five weeks spent at Buxton, renewed health has been acquired, and the long pent-up disorder of the system has disappeared along with the catamenia in a " fit of the gout,"—a disease in part acquired by generous living, and in part inherited from a parent, who had long manifested its possession in numerous nodosities of the fingers and hands—deformities which he has transmitted to his offspring.

In reference to this class of cases Dr Simpson has favoured me with the following communication:—

" I have seen several cases of inflammation of the uterus, or rather of the uterine region, of the nature of simple gout, or rheumatic gout; or, at all events I believed these inflammatory attacks to be of this special pathological nature, in consequence of their coexisting with, or following immediately upon the presence of undoubted gout in other parts of the system.

" In one instance which I had the pleasure of seeing repeatedly along with you last summer, the attack of uterine gout came on during the existence of a marked prolonged fit of the same disease in the fingers, &c. The principal organic effect, as ascertained by vaginal examination, was effusion into the cellular tissue of the broad ligaments (*pelvic cellulitis*), giving rise to tumefactions of considerable size around the sides of the uterus. This pelvic effusion was gradually absorbed under some local antiphlogistic means, and the use internally of colchicum and other medicines.

"About the same period I visited repeatedly, at some distance from town, a lady, hereditarily predisposed to gout, who, after some prolonged constitutional derangement, was seized with excruciating pain, followed by redness and swelling in and around the right ankle. The agony attendant on this attack was so great that she could not move the limb in the least for two or three weeks. There were large quantities of oxalates in the urine. When recovery, however, did begin, convalescence was very rapid for two or three weeks; when she travelled home to her own residence, a journey of nearly a hundred miles. Shortly after arriving, she was seized with severe pelvic pain; and two days subsequently there was a large inflammatory swelling in the right broad ligament, and at the same time much effusion of serum into the cellular tissue of the upper portion of the posterior wall of the vagina. This last formed a tumor like an orange, or larger, projecting backwards into the rectum. Under leeches and mercury these swellings subsided, but with a rapidity seldom or never seen in common cases of similar inflammatory swellings attendant upon common pelvic cellulitis.

"In the first case of uterine gout which I ever saw (now some years ago), the lady suffered repeatedly and metastatically in the uterus, simultaneously with, and sometimes after, the appearance of the disease in the extremities. The uterus was very large; was fixed as if there had been perimetritis; and in the attacks which I watched, it appeared to me that the wall of the organ and the peritoneal surface were usually the seats of the morbid action. In her there was no effusion into the neighbouring cellular tissue.

"In none of the instances of gout of the uterus which I have watched, have I seen the inflammatory action attack the neck of the uterus, or the mucous membrane of the organ. Rheumatic inflammation seems to do so."

These examples of diseases of a congestive or inflammatory nature sufficiently illustrate the influence of the specific poison of gout in generating or modifying the form of morbid action: and it is of great moment to keep in view the effects produced by such contamination, for though the lesions resulting in such cases may not differ pathologically from those of idiopathic origin, yet the specific character of the disorder being established, it will be found that the maladies connected with it are amenable to certain remedies, which otherwise might have been overlooked.

The gouty diathesis is frequently the origin of many painful affections of the nerves, and is known to give a character to their history, which being recognised, a mode of treatment is suggested which is often successful after other means had failed.

CASE XI.—I was called to see Capt. G. labouring under a severe attack of sciatica which had resisted many applications, and a great variety of internal remedies. I ascertained that he had a hereditary claim to gout, and had manifested some indications of its early visit. Accordingly the case was treated as one of gout, and colchicum and neutral salts were prescribed. Complete relief from suffering was quickly obtained, and similar results followed, on a subsequent occasion, the administration of the same remedies at the onset of the attack.

In cases where the neuralgia partakes more of the asthenic than the sthenic form, the combination of quinine or iron with colchicum has often a happy effect.

CASE XII.—I was requested by Dr Pagan to visit with him a gentleman, aged sixty, who had been for many days suffering a severe and intractable attack of sciatica. A great variety of treatment had been previously employed, but the result had not realized expectation. The gouty diathesis was more suspected than proved or acknowledged; and as the symptoms indicated neither active congestion nor inflammation, but on the contrary an atonic condition of the system, quinine with colchicum were recommended, and after a steady trial were found beneficial, the disease yielding to their united influence what had been refused to their action singly and unassisted.

That gout may affect the brain in a variety of ways has long been recognised; that it may also affect the spinal marrow is a truth which, though well established, is yet comparatively little known. We are chiefly indebted to Dr Graves for the information we possess on this interesting point; and though his views in regard to the manner in which the spinal marrow or its investments become the subject of degeneration and softening may be open to controversy, there cannot, I apprehend, be a doubt that gout or the gouty diathesis may involve these structures, and produce a state of complete paraplegia. Dr Graves has attempted to prove that gouty inflammation of the nerves and their neurilemma may, in process of time, extend to the spinal marrow and its investments, and give rise to the derangements of the latter, terminating in ramollissement and structural degeneration. The cases he has adduced in support of this are most interesting and instructive, and tend to shew that disease commencing in the nerves of some particular part or organ, may be gradually propagated to the spine, producing all the symptoms which are referable to an original affection of the nervous centres. Whether the disease originates in the periphery of the nervous system, and passing along the trunk of the affected nerve with a retrograde movement, finally reaches the nervous centre; or whether, originating in the latter, its first manifestations are perceived at the extremities of the nerves, there cannot be a question that a peculiar form of palsy of the lower limbs is intimately associated with the gouty diathesis.

CASE XIII.—Some years ago I attended a member of a gouty family, a gentleman of the age of forty-five, who had long laboured under the dyspeptic derangements which usually occur in the fit of gout. He had suffered much from the mental depression and hypochondriacal anxieties of the premonitory stage, and been familiar with the cardiac distress, and fugitive pains and twitches which foretold the more severe and lasting indications of his family inheritance. He had, moreover, indulged, though not to great excess, in the luxuries of the table. After exhibiting symptoms of latent gout in the extremities, he began to manifest an unsteadiness of gait, and want of command or power over his legs. Sensation was somewhat depraved, but not in proportion to the loss of power of motion. Considerable irritability of bladder accompanied this condition, and by and by the power of retention became affected. The urine was loaded with the lithates, and numerous crystals of the oxalate of lime were found from day to day. The usual symptoms of gout had disappeared, but those affecting the nervous system had continued for upwards of a month. He was placed on a simple, rather sparing diet, and took the nitro-muriatic acid, with alterative doses of arsenic, and subsequently cod-liver oil, under which he gradually recovered.*

CASE XIV.—I have lately been in attendance, along with Mr Smith of Lasswade, on a gentleman, aged fifty, inheriting a well-marked predisposition to gout, and who has for many years manifested its possession in the purest form, developed in frequent attacks in the toe, instep, and ankle, as well as in the knee, elbow, and small joints of the fingers. For many months before I saw him he had complained of weakness of the back and loins, and betrayed an unsteadiness in walking, and loss of power over the lower limbs. To these symptoms succeeded an irritability of bladder, and difficulty in commanding a steady flow of urine. The mind was dejected. All his symptoms became aggravated immediately before an attack of gout in the extremities. The urine was found to contain the oxalate of lime crystals in considerable numbers, along with the lithates and epithelium in small quantity. During the earlier part of his illness a variety of treatment had been adopted, but without apparent benefit. Latterly he derived advantage from the nitro-muriatic acid, and from small doses of arsenic continued over a long period as an alterative, and improved in walking, and in general health. The fits of gout, however, have again become more severe, and recurred more frequently; and it has been observed that the nervous symptoms on such occasions are of a graver character; that articulation becomes affected, and confusion of thought and other symptoms of an apoplectic tendency shew themselves, always subsiding, however, on the manifestation of gout in the extremities.

Case XV.—A gentleman, aged fifty, inheriting the gouty diathesis from both sides of his house, began, about the age of forty-five, to manifest in his own person the symptoms of the disease. The usual precursory signs were not clearly developed; but after suffering from dyspeptic derangement and cardiac uneasiness for some time, he began to complain of heat and tenderness in the soles of the feet, which were followed by a slight attack of gout in the ball of the great toe. He now suffered from hemorrhoids, and for many months the discharge was so copious as to weaken and blanch him. During this time the symptoms of gout were slight and evanescent; but he began to complain of loss of sensation in the feet, and of numbness gradually ascending the limbs. His gait became unsteady and insecure, and he felt the discomfort of not knowing that his feet reached the ground, or on what he was treading. Sensation gradually became impaired over both limbs, and upwards over the hips and loins to the height of the lower dorsal vertebræ. The hemorrhoidal discharge was arrested by the application of ligature; and after the employment of means directed to the removal of his anemic condition, it was hoped that the paralysed limbs might recover sensation and motion. No improvement, however, in this respect has taken place. The bladder has lost power. The urine, which at one time contained the oxalate of lime is now persistently phosphatic, and walking is performed with difficulty, and at the expense of numerous falls. On different parts of the body there are patches of psoriasis. Dr Keith, with whom I have lately seen the patient, had employed many remedies, suggested by varying notions of the true nature of the case, but with partial or doubtful benefit. Under the impression that it belongs to that class described by Dr Graves, and has gout for its origin, the patient has been subjected to an anti-arthritic treatment, including the cod-liver oil, but as yet with no better result.

There is reason to fear that in these two last cases some organic change may have taken place in the spinal chord or its sheath, the result of gouty inflammation there; though the favourable termination of the case immediately preceding may encourage the hope that, under the persistent employment of remedies directed to the removal of the gouty diathesis, the paraplegic condition may still be overcome.

I should now proceed to illustrate by examples the influence of the gouty diathesis in some forms of functional disorder and organic disease; but the limits assigned to such a paper as this have already been reached, and I must delay to another opportunity the exhibition of such sketches as I possess, along with any observations I may be able to offer on the nature and treatment of this obscure and interesting disorder.[1]

ART. II.—*Cases of Xerophthalmia, with Remarks.* By R. TAYLOR, Surgeon to the Central London Ophthalmic Hospital.

1. George Conway, aged twenty-five, was admitted an out-patient to the Central London Ophthalmic Hospital, Feb. 7, 1852.

Sixteen years ago he lost the left eye from smallpox, and has ever since suffered so much pain and irritation in the other that it has been of little use to him, though he could see sufficiently to walk about without assistance. Two years ago he was persuaded to put himself under the care of an empiric, who used some powerfully irritant application to the eye. This caused excessive pain, and the sight got rapidly worse, so that within a week he became almost totally blind.

---

[1] While this sheet has been passing through the press, I have for the first time perused chap. xxvi. of Dr Wardrop's recent work on Diseases of the Heart, and have been much struck and gratified by the similarity of view entertained by the author and myself, particularly in regard to "Gouty Inflammation of the Heart."

In the right eye the cilia, which were remarkably strong and bushy, were in contact with the globe along the whole extent of both lids, partly from trichiasis, and partly from slight entropium. The eye looked like that of a dead animal after exposure to the sun. The cornea was dry, devoid of lustre and transparency, and covered with particles of dust and dirt, especially in the line of junction of the lids; the situation of the pupil could with difficulty be made out, but the iris could not be seen. The conjunctiva was dry and parchment-like, and was traversed by several large tortuous vessels; round the outer margin of the cornea, it was thrown into several prominent wrinkles or plaits, which were flattened or deepened according as the eye was turned inwards or outwards. The palpebral sinuses were slightly diminished in depth, and contained a little moisture; their lining membrane was smooth, and covered with bluish marks like cicatrices. The situation of the lachrymal puncta was visible, but they were not pervious. The movements of the eye and of the lids were tolerably free, but were performed with a feeling of stiffness and restraint. The eye felt hot and dry, and he had frequent paroxysms of acute pain, which destroyed his rest, and rendered him anxious to submit to any treatment that would give him relief. The sensibility of the surface of the eye was much impaired; it could be freely touched without causing him any inconvenience. He could dimly discern the outline of a large object, such as the human figure, at the distance of a yard; but he could not distinguish a male from a female.

The left eye was shrunken and atrophied, and its conjunctiva dry and shrivelled, as in the right eye.

I performed Mr Walton's operation for entropium upon both lids, with the effect of removing the cilia from contact with the globe, with the exception of a few which were very much displaced; these were subsequently got rid of by inoculation with tartar emetic, as recommended by Dr Hunter (*Med. Chirurg. Review*, July 1841.) At the same time, he was directed to keep the eye constantly moist, by applying a drop of glycerine as often as necessary. With the removal of the entropium, the pain at once and permanently ceased, the sight also, was considerably improved within a few days after he commenced to use the glycerine, partly from the removal of the dirt with which the cornea had been covered, and partly from the degree of transparency which it communicated to the epithelium. It could now be ascertained that there was some slight superficial opacity of the cornea proper, induced, doubtless, by the friction of the inverted cilia; this gradually cleared away, until, at the end of three months, the colour and texture of the iris were distinctly visible, and his sight was so much improved, that he could distinguish a pen lying on the table at the distance of four feet. He now abruptly ceased his attendance, and discontinued the use of the glycerine; he was not seen for some months afterwards, when the eye had again become dry and opaque, and he could merely distinguish light from darkness.

2. John Scott, aged sixty-five, applied at the hospital, April 25, 1853.

About five years ago he had an attack of "cold in the eyes," from which, owing to neglect, he still suffered at the expiry of five months. He was then admitted as an out-patient at an hospital, where a strong solution of nitrate of silver was dropped into the eye three times a week; each application caused acute pain, which lasted the whole day, unless he could succeed in getting a few hours' sleep. This treatment was continued for several months, during which his sight got gradually worse, and eventually, when he ceased his attendance at the hospital, he was totally blind. He subsequently applied to several surgeons, some of whom again made use of stimulating applications without benefit.

In both eyes the palpebral sinuses are completely obliterated, and the eyeball and lids consolidated into one mass, the margin of the upper lid being on a level with the upper third of the cornea, that of the lower just reaching its lower border. The part of the cornea thus covered is not adherent to the lid, which can be pulled forwards from it by means of the cilia, so that a probe can be passed completely round its margin. The membrane is perfectly dry, and of a dirty white colour, resembling the dried cuticle which has been detached by a blister; it is not thrown into plaits around the cornea, as in the last case; on the contrary, it seems to be drawn tightly over the surface of the eye, diminishing the transverse fissure of the lids, and preventing motion, except to a very limited extent. The left cornea is dry, dead-looking, covered with adherent dust and dirt, and so opaque that the situation of the pupil cannot be made out, even when the light is concentrated upon it through a lens. The right cornea is of a uniform dull red colour from minute vascular injection, and has a pulpy villous appearance, speckled here and there with small ulcers. The situation of the puncta is visible, but they are not pervious. There is neither entropium nor trichiasis; the cilia, which are numerous and strong, as well as the margins of the lids, preserving a perfectly natural direction. By a strong muscular effort, the cutaneous surfaces of the lids can be brought into contact, but farther than this they are almost immoveable. The eyes feel dry, stiff, and uncomfortable, and occasionally he has pain of a burning character, but seldom very severe. The sensibility of the surface is not greater than that of the palm of the hand. He can merely distinguish between light and darkness.

The treatment consisted in keeping the eyes constantly moist with glycerine. The ulcerated cornea soon healed up, and became covered with a dense bluish cicatrix, which adhered to the lid. The left cornea has improved somewhat in transparency; the situation of the pupil can be made out without difficulty, and through one small spot near the margin, the colour of the iris is visible. In a good light he can see the outline of the human figure, but not distinctly. The eyes feel much more comfortable, and he has been entirely free from pain since the glycerine has been applied.

3. George Ling, aged seventy-three, was admitted as an out-patient, April 20, 1853.

Twenty years ago he suffered much from inflammation of the eyes, which he attributes to excessive weeping, having met with severe domestic misfortunes; he had constant pain, and after a time they began to feel dry and hot, and the sight became dim. He applied to a surgeon, by whom he was cupped and leeched, a strong solution of caustic was dropped into the eyes, and sulphate of copper was rubbed upon the lids. These applications, which were continued for some time, caused excessive pain. He asserts that the sight was completely destroyed by the first employment of the caustic solution; this may be an exaggeration, but it is certain that it disappeared very rapidly, leaving him merely the power of distinguishing light from darkness. Six years ago, after having had a great variety of advice, and having tried many remedies without relief, he was accosted in the streets by a surgeon, who told him that the cause of his blindness was inversion of the lids, and persuaded him to submit to an operation. This appears to have consisted in splitting up the upper lids, and, probably, removing a large portion of skin, as the lids were so much shortened that they could no longer be closed; the corneæ were thus permanently exposed to the air, and his sufferings were much increased.

Both upper lids are much shortened and disfigured by the operation, and their edges are studded with small, fine cilia, some of which are in contact with the globe. The palpebral sinuses are completely obliterated, not the slightest fold or indentation marking the spot where they formerly existed; the lids thus appear continuous with the surface of the eye, their edges are rounded off, and present no traces of Meibomian glands or puncta lachrymalia, and the transition between the skin and the altered mucous membrane is imperceptible. The surface of the eye is perfectly dry, and of a dirty-white colour, but there is no adherent dust. The conjunctiva appears to be converted into a thick, opaque cuticle, not thrown into folds at the margin of the cornea, but apparently prolonged over it, so that its boundary cannot be seen. Towards the centre of the cornea this covering is rather less dense, so that by concentrating the light upon it by means of a lens, one or two large vessels can be indistinctly seen deep in the substance of the cornea proper; on the other parts of the eye it has exactly the appearance of the cuticle detached by a blister on a part where the skin is thick, as on the palm of the hand. The lids do not adhere to the corneæ, the fissure made by the operation being attached round their upper borders, while the remainder of the lid is on a level with their upper thirds. The surface of the eyes is devoid of sensibility, and may be touched with as much freedom as any other part of the body. The movements of the eyes are very much limited, and are accompanied with a most distressing sensation of stiffness and restraint. He has frequent and severe paroxysms of burning pain, and is never free from uneasiness. Vision, except the power of distinguishing light from darkness, is extinct.

As the condition of the lids precludes the possibility of any operation for the removal of the trichiasis, and as the displaced cilia are few in number, and very weak, it has been deemed sufficient to extract them from time to time with the forceps. For the rest, the treatment has been limited to keeping the eyes constantly moist with glycerine, which has afforded him more comfort than he has enjoyed for many years. The pain, though it occasionally recurs, is much less severe, and the softening of the shrivelled conjunctiva permits greater freedom of motion, and diminishes the feeling of stiffness and restraint. Little improvement has taken place in the transparency of the cornea, nor is it probable, from the density of the cuticle with which it is covered, that much can ever be effected.

REMARKS.

It is very remarkable that a disease whose objective symptoms are of such a striking and peculiar nature as those of Xerophthalmia, should have altogether escaped the notice of the older writers on ophthalmic surgery, distinguished as they were for their powers of minute observation, and for their proneness to describe every trifling deviation from the more ordinary morbid appearances as a new disease. Such, however, is the fact; it remained undescribed until the commencement of the present century, and the few cases which have since then been accurately recorded are scattered widely through the medical journals of France and Germany, little attention having been paid to it in this country. This apparent neglect is perhaps to be explained by the rarity of its occurrence. The almost simultaneous appearance of three cases at the same hospital must be regarded as merely fortuitous, and not as affording any index of its frequency compared with other diseases; it is seldom witnessed even by those whose attention is specially directed to diseases of the eye, and to the majority of practitioners it is unknown except by name. It is interesting, therefore, more in a pathological than in a practical point of view; but as its phenomena are singular and little understood, the following brief account of its more striking peculiarities may be acceptable to those who have not leisure to investigate the subject for themselves.

Xerophthalmia was described for the first time by Schmidt, in a work on the diseases of the lachrymal organs (Ueber die Krankheiten des Thranenorgans), published in 1803. He was followed at considerable intervals by Benedict (Handbuch der Pract. Augenheilkunde) and Travers (Synopsis), who concurred with him in ascribing it to obliteration of the ducts of the lachrymal gland by chronic inflammation. Probably the best description which it has yet received is that by Professor Ammon in his Zeitschrift, f. d. Ophthalmologie, t. i., p. 65, and further, in t. ii., p. 381, which contains a review of a thesis by Klingsohr on the same subject. The same author, in his Klinische Darstellungen, Plate I., figs. 16 and 18–21, has given accurate delineations of the disease in several stages. Jæger and Chelius describe it briefly in their works on diseases of the eye, and it has received more extended notice from MM. Velpeau, Carron du Villards, Vidal de Cassis, Dupré, and Cade. Isolated cases are scattered through various Continental journals, and it is mentioned, more or less at length, in the principal standard works of this country on ophthalmic disease, especially in those of Mr Lawrence, Dr Mackenzie, and Mr Middlemore.

The earliest stage of Xerophthalmia is characterized, according to Professor Ammon, by injection and slight dryness of the sclerotic conjunctiva, and by a dead appearance and loss of polish of the cornea, "as if it had been smeared with grease."

In a more advanced stage the exposed portion of the conjunctiva is dry and shrivelled, resembling, as Mr Wardrop says, a piece of dried bladder; the palpebral sinuses, though less deep than natural, still exist, and may contain a little moisture, so that the motions of the eyeball and eyelids are not much impeded. In consequence of the hardness and loss of elasticity of the conjunctiva, it is thrown into deep wrinkles or plaits round the margin of the cornea, according as the eye is moved outwards or inwards. The cornea is dry and opaque, preventing a clear view of the iris or even of the pupil, and it is generally covered with particles of dust, swept more or less into a line by the action of the eyelids; frequently also, especially in cases complicated by entropium or trichiasis, the opacity is increased by exudation into the proper tissue of the cornea. This stage was well marked in the first case, that of Conway. As the disease proceeds, the conjunctiva contracts in every direction, flattening out the plaits around the cornea which are so characteristic in an earlier stage, and ultimately dragging out the reduplications which form the sinuses, so that the eyeball and eyelids are consolidated into one mass, and their motions are exceedingly limited. The cornea is partially covered by the upper lid, but as this, as well as the lower lid, is immoveably fixed to the surface of the globe, about two-thirds of it remain permanently exposed to the air, and its epithelium becomes so thickened and opaque, that even the situation of the pupil is invisible. The surface of the eye at the same time loses its sensibility; it may not only be freely touched, but will also bear the contact of irritating substances, as pepper, snuff, &c., without inconvenience to the patient. This stage is sometimes accompanied by pannus, or by ulceration of the cornea, as in the case of Scott, and in two cases reported by MM. Carron du Villards and Desmarres. What may be considered as the most advanced stage of the disease is seen in the third case, that of Ling, in whom, in addition to the symptoms already mentioned, the corneal epithelium has assumed the appearance of thick dead cuticle, and the line of demarcation between the sclerotica and the cornea is concealed by it. Professor Ammon is of opinion that the disease terminates in atrophy of the globe, which shrinks into the orbit, throwing the conjunctiva into radiating folds; but I would suggest that this condition, when present, may be due to penetrating ulceration of the cornea, with evacuation of the humours; as would probably have been the result in the case of Scott, had the ulceration not been checked, and as happened in the case of a lady witnessed by M. Desmarres. In Ling's case, although the follows—"In one case which fell under my observation, and which did not appear to be the result of inflammation, the conjunctiva cornea only was affected, and presented an appearance as if it had been a piece of silk paper laid upon the surface of the cornea." This is the only case, so far as I have been able to discover, which would at all appear to support M. Vidal's opinion that xerophthalmia can occur without previous inflammation, and we have only to refer to the later edition of Dr Mackenzie's work, to find that he himself attaches no importance to it, his first impressions having probably been hastily made; and to find him arguing at some length that "long-continued inflammation of the conjunctiva always precedes conjunctival xeroma." It is unnecessary, therefore, to refute at farther length the theory of its purely nervous origin, or to shew, as has been done by M. Velpeau, that it is inadequate to explain many of the most striking changes by which the disease is characterized, such as the obliteration of the palpebral sinuses, the adhesion of the eyelids to the globe, the destruction of the Meibomian glands, &c.

But however difficult it may be to explain these phenomena from the observation of a single case, or of one stage only of the disease, the difficulty seems to vanish in great measure if we trace the successive changes from their earliest commencement to their full development. We find invariably that there has been long-continued or often-repeated inflammation, which in some instances has been altogether disregarded, in others aggravated by entropium or trichiasis, or by the injudicious use of powerful escharotics or stimulants. After a certain time this is followed by unnatural dryness of the eye, which may be traced partly to closure of the lachrymal ducts, and partly to disorganization of the conjunctiva, rendered sufficiently manifest by its altered appearance. The next, and most remarkable feature of the disease, is the gradual shrinking and final disappearance of the palpebral sinuses; and the manner in which this is effected seems to afford a key to the explanation of its pathology. If we look at a case in its most advanced stage, we see nothing which would lead us to suppose that the adhesion of the eyelids to the globe had not taken place in the usual manner, by the contraction of new material poured out to repair loss of substance, as after a wound or burn, or by the union of opposite granulating surfaces, as might happen after extensive idiopathic ulceration. But if we watch the change from time to time during its progress, we see that no loss of substance, no breach of surface, has taken place. The sinuses slowly diminish in depth without union of their opposing surfaces, until they have totally disappeared, and the conjunctiva, instead of forming a deep reduplication, is continued directly from the margin of the eyelids over the surface of the eye, shrinking at the same time in every direction, flattening out the plaits which it had at first formed around the margin of the cornea, diminishing the transverse diameter of the palpebral fissure, and applying itself tightly over the surface, so as to bind the eyeball and eyelids into one solid and nearly immoveable mass. The cornea alone, though partially concealed by the upper eyelid, remains free to its margin, where the conjunctiva, with the exception of its epithelial covering, terminates; in those cases alone in which there has been ulceration of its surface, as in the case of Scott, does it adhere to the opposing surface of the eyelid. I think that we are warranted in inferring the nature of the process by which this destruction and gradual contraction of the conjunctiva is effected, from analogous changes in other organs, where they have been more carefully watched. In the kidney, for example, certain forms of chronic inflammation are attended with the exudation of lymph, by which, as well as by the impaired nutrition consequent upon its morbid condition, its proper structures are destroyed, and as the organization of the new material proceeds, the gland shrinks into a solid and structureless mass. Such, in all probability, is the process in this peculiar inflammation of the conjunctiva; its structures are infiltrated with inflammatory exudation, its secretory apparatus is destroyed, and the gradual contraction of the new material as its organization advances, is sufficient to account for the various changes by which the disease, in all its phases, is characterized.

With regard to the cornea, the changes which it undergoes are, so to speak, to a certain extent mechanical. To maintain its due transparency and lustre, it is essential that there should be a constant supply of moisture, and that the desquamation of the effete epithelium should be assisted by the friction of the eyelids. When, in the early stage of the disease, the supply of moisture is withheld, dimness is occasioned simply by the drying of the epithelium, exactly as occurs in a dead body if the eyes are exposed to the air; and a certain amount of transparency may be restored by wetting the eye with water, or as some patients are in the habit of doing, with saliva. So long as the eyelids retain their mobility, and assist, however imperfectly, in removing the dead epithelial particles, the dimness, except in so far as it is caused by exudation into the cornea proper, does not proceed to any considerable extent; it is not until desquamation is follows—" In one case which fell under my observation, and which did not appear to be the result of inflammation, the conjunctiva cornea only was affected, and presented an appearance as if it had been a piece of silk paper laid upon the surface of the cornea." This is the only case, so far as I have been able to discover, which would at all appear to support M. Vidal's opinion that xerophthalmia can occur without previous inflammation, and we have only to refer to the later edition of Dr Mackenzie's work, to find that he himself attaches no importance to it, his first impressions having probably been hastily made ; and to find him arguing at some length that " long-continued inflammation of the conjunctiva always precedes conjunctival xeroma." It is unnecessary, therefore, to refute at farther length the theory of its purely nervous origin, or to shew, as has been done by M. Velpeau, that it is inadequate to explain many of the most striking changes by which the disease is characterized, such as the obliteration of the palpebral sinuses, the adhesion of the eyelids to the globe, the destruction of the Meibomian glands, &c.

But however difficult it may be to explain these phenomena from the observation of a single case, or of one stage only of the disease, the difficulty seems to vanish in great measure if we trace the successive changes from their earliest commencement to their full development. We find invariably that there has been long-continued or often-repeated inflammation, which in some instances has been altogether disregarded, in others aggravated by entropium or trichiasis, or by the injudicious use of powerful escharotics or stimulants. After a certain time this is followed by unnatural dryness of the eye, which may be traced partly to closure of the lachrymal ducts, and partly to disorganization of the conjunctiva, rendered sufficiently manifest by its altered appearance. The next, and most remarkable feature of the disease, is the gradual shrinking and final disappearance of the palpebral sinuses; and the manner in which this is effected seems to afford a key to the explanation of its pathology. If we look at a case in its most advanced stage, we see nothing which would lead us to suppose that the adhesion of the eyelids to the globe had not taken place in the usual manner, by the contraction of new material poured out to repair loss of substance, as after a wound or burn, or by the union of opposite granulating surfaces, as might happen after extensive idiopathic ulceration. But if we watch the change from time to time during its progress, we see that no loss of substance, no breach of surface, has taken place. The sinuses slowly diminish in depth without union of their opposing surfaces, until they have totally disappeared, and the conjunctiva, instead of forming a deep reduplication, is continued directly from the margin of the eyelids over the surface of the eye, shrinking at the same time in every direction, flattening out the plaits which it had at first formed around the margin of the cornea, diminishing the transverse diameter of the palpebral fissure, and applying itself tightly over the surface, so as to bind the eyeball and eyelids into one solid and nearly immoveable mass. The cornea alone, though partially concealed by the upper eyelid, remains free to its margin, where the conjunctiva, with the exception of its epithelial covering, terminates; in those cases alone in which there has been ulceration of its surface, as in the case of Scott, does it adhere to the opposing surface of the eyelid. I think that we are warranted in inferring the nature of the process by which this destruction and gradual contraction of the conjunctiva is effected, from analogous changes in other organs, where they have been more carefully watched. In the kidney, for example, certain forms of chronic inflammation are attended with the exudation of lymph, by which, as well as by the impaired nutrition consequent upon its morbid condition, its proper structures are destroyed, and as the organization of the new material proceeds, the gland shrinks into a solid and structureless mass. Such, in all probability, is the process in this peculiar inflammation of the conjunctiva; its structures are infiltrated with inflammatory exudation, its secretory apparatus is destroyed, and the gradual contraction of the new material as its organization advances, is sufficient to account for the various changes by which the disease, in all its phases, is characterized.

With regard to the cornea, the changes which it undergoes are, so to speak, to a certain extent mechanical. To maintain its due transparency and lustre, it is essential that there should be a constant supply of moisture, and that the desquamation of the effete epithelium should be assisted by the friction of the eyelids. When, in the early stage of the disease, the supply of moisture is withheld, dimness is occasioned simply by the drying of the epithelium, exactly as occurs in a dead body if the eyes are exposed to the air; and a certain amount of transparency may be restored by wetting the eye with water, or as some patients are in the habit of doing, with saliva. So long as the eyelids retain their mobility, and assist, however imperfectly, in removing the dead epithelial particles, the dimness, except in so far as it is caused by exudation into the cornea proper, does not proceed to any considerable extent; it is not until desquamation is completely arrested by the fixing of the eyelids, and the cornea is at the same time constantly exposed to the air, that the epithelium accumulates and concretes into a dense and firm cuticle. The opacity of this covering renders it difficult to ascertain whether the cornea has undergone any structural alteration beyond the surface. It does not appear that any such change forms a necessary part of the disease, for, as far as we are able to judge by moistening the eye, the dimness in many cases, even of long standing, does not extend beyond the epithelium ; in others, again, by the assistance of powerful artificial light, we can detect, as in the third case, obscure appearances of morbid action deep in the substance of the cornea. At present it is impossible to say with any degree of certainty whether such changes form an essential or merely an accidental feature of xerophthalmia, but it is probable that long-continued impaired nutrition eventually produces more or less deep-seated injury in every case.

We have no information as to the immediate exciting causes of xerophthalmia. Dr Mackenzie is disposed to attribute it, in many instances, to the use of irritating applications ; "instead," he says, "of abating the inflammation of the conjunctiva by proper soothing and emollient applications, and by the local detraction of blood, it has unfortunately become a too common practice to use only stimulants and escharotics, and some of these so strong that they probably destroy the mucous texture of the membrane to which they are applied ; an effect followed, after some time, by the conversion of the conjunctiva into a mere cuticular covering." This opinion, coming from such a competent observer, and strengthened as it is by the history of the majority of the cases which have been recorded, is well worthy the attention of those who are in the habit of employing such agents.

The treatment of xerophthalmia, when fully formed, is merely palliative. If it were recognised in its earliest stage, while the lachrymal ducts were still pervious, and the conjunctiva to a considerable extent sound, it is not impossible that its progress might be permanently checked, and the eye restored to comparative health. As I have neither seen nor read of such a case, I can merely suggest the adoption of the same general line of treatment as has been found useful in other diseases of apparently similar nature. Such remedies as check the deposit and promote the absorption of inflammatory lymph are especially indicated ; and of these the most efficient would probably be a long-continued and very mild course of mercury, avoiding carefully its poisonous effects, as indicated by swelling and ulceration of the gums, and maintaining, as far as possible, a vigorous state of the general health by a liberal diet, and, if necessary, by medicinal tonics. It is not probable that much could be effected by local applications to the diseased membrane; but if they appeared necessary, the mildest astringents only would be admissible. The utility of counter-irritation to the forehead or temples, in chronic diseases of the eye, is disputed by some very competent observers; but from my own experience of its effects, I should anticipate favourable results from its steady employment in such a case as I am supposing.

When the conjunctiva has been converted into a dry and unsecreting cuticle, nothing short of its regeneration could effect a cure, and this is beyond the reach of surgery. It has been subjected to every variety of treatment; local applications, emollient, stimulant, and escharotic, have met with a fair trial; it has been excised by M. Sanson, and cauterized by M. Carron du Villards, who announces his intention of applying blisters in the next case which comes under his care. Mr Wardrop, under the idea that the cause of the disease, in the case which I have already alluded to, was congenital absence of the lachrymal ducts, attempted to form an artificial passage to the gland, by thrusting a bistoury in its direction, and keeping the wound open by a tent; an operation which is severely criticised by M. Vidal. All these measures, as well as others too numerous to mention, have proved totally ineffectual; the conjunctiva has irreparably lost its characters as a mucous membrane, and we can no more effect its renewal than we can that of a kidney which has undergone a similar process of degeneration. Still, much may be done to relieve the sufferings of the patient, and even, in some instances, to restore a certain amount of vision. The eye should be defended from dust and other foreign particles by spectacles of plain glass, and entropium or trichiasis, when present, should be removed by appropriate operations; great care being taken to avoid any unnecessary shortening of the eyelids, which would aggravate the evil by still further impeding their motions and exposing the cornea. Further, some substitute must be found to supply the place of the natural secretion, to soften and relax the hardened conjunctiva, and to moisten the dry epithelium of the cornea. Patients sometimes effect this for themselves by applying the saliva on the point of the finger; mucilaginous fluids of various kinds have been recommended, but pure oil has been generally preferred. From its unirritating qualities, and power of retaining its moisture, I was induced to make a trial of glycerine, and have found it to answer the purpose so effectually, that I can strongly recommend it to the notice of those who may have similar cases under their care. Whatever application be preferred, it must be very frequently used, as the small quantity which can be made to adhere to the surface of the eye, especially when the sinuses are gone, very speedily dries; the patient should therefore carry a supply with him, and apply it from time to time, as required, with the point of the finger, or with a camel's-hair brush. By attending to these precautions, much relief and comfort may be obtained; in advanced cases, the distressing sensations of heat and dryness are removed, and the motions of the eye and of the eyelids are facilitated by the softening of the hard and unyielding conjunctiva; while in less advanced cases, as in that of Conway, where the corneal epithelium has not yet undergone the cuticular transformation, its transparency may be so far restored as to allow of a very fair amount of vision, so long as the remedy is persevered in.

The following references may be useful to those who may be desirous of investigating the subject further; others might be given, but I restrict myself to those alone which I have had an opportunity of consulting.

Schmidt—Ueber die Krankheiten des Thranenorgans, p. 55.

Benedict—Handbuch der Pract. Augenheilkunde, vol. iii., p. 157.

Ammon—Zeitschrift f. d. Ophthalmologie, vol. i., p. 65; and vol. ii., p. 381.

Ammon—Klinische Darstellungen, Plate I., figs. 16 and 18–21.

Vidal de Cassis—Pathologie Externe, vol. iii., p. 413.

Carron du Villards, vol. ii., p. 114.

Bibliothèque du Médécin Praticien, vol. x., p. 283.

Dupré—Thèses de Paris, 1836.

Cade—Gazette Médécale de Paris, 1836, p. 316.

Rau—Annales d'Oculistique, Supplement, vol. i., p. 105.

Wardrop—Lancet, 1834, p. 344.

The works of Desmarres, Lawrence, Mackenzie, Middlemore, and Travers.

ART. III.—*Two Cases of Partial Chorea in Pregnancy; with Remarks.* By J. MATTHEWS DUNCAN, A.M., M.D., F.R.C.P.E.; Lecturer on Midwifery; Physician-Accoucheur to the Royal Dispensary, Edinburgh, &c., &c.

Of the nervous affections which complicate pregnancy, chorea is one of the most interesting. It consists in combined movements of one or more groups of muscles, independent of cerebral influence, increasing in violence when predetermined movements are attempted, and more or less interfering with the latter.[1]

Chorea is peculiarly a disease of childhood, but is occasionally observed also in the adult. When it does thus occur in the adult female, it is frequently found in connection with pregnancy. Numerous cases of this description are recorded by Lever, Meissner, Romberg, and others.

In some the disease is found to have continued from childhood, or from the period of commencing menstruation on till the occurrence of pregnancy. In others it has commenced with or during pregnancy, and in another class of cases it has a less distinct individuality, being found merely as the precursor or follower of paralysis of the limbs. The cases specially under consideration are peculiarly interesting, *firstly*, because the attacks although severe, were partial, the lower limbs being the parts affected with the involuntary movements; and, *secondly*, because the attacks were periodical in their character, the movements in one case occurring only in the evening and early part of the night, and especially on awaking after the first sleep of an hour or less; in the other case, the motions being troublesome also only during the night.

This affection is one of a class of nervous disorders now better known than formerly, although their pathology is still confessedly obscure. In his work on Pregnancy (p. 4), Dr Montgomery takes notice of them after speaking of the anatomical changes in the hypogastrium consequent on pregnancy, and their more direct results. He says that "from the stretching of the round ligaments of the uterus, as well as from the increased sensibility of the nerves which they contain, considerable uneasiness is felt in the direction of these cords, and about their termination at the sides of the pubes. This uneasiness extends also along the nerves of the thigh, producing numbness, cramps, and even considerable pain along the limb; which latter symptoms are often observed among the earliest indications of uterine irritation, whether arising from functional derangement, organic disease, or the healthy excitement of pregnancy. It is not unusual, under such circumstances, to find the power of one or both of the lower limbs considerably impaired; and in some few rare instances they have become partially or completely paralytic, and even hemiplegia has been observed, but to what degree the mere enlargement of the uterus is the agent in the production of such a state, seems very doubtful."

---

[1] Romberg on the Diseases of the Nervous System, vol. ii., p. 54.

The observations, now sufficiently numerous, of similar nervous disorders during pregnancy seizing various regions of the upper half of the body instead of the lower, demonstrate that the mere mechanical enlargement and consequent pressure of the uterus on the neighbouring parts cannot be in all cases their direct cause. As examples of these affections, we may refer to the cramps or paralyses of one or both arms or hands, or even of single fingers,—to cases of amaurosis or other derangements of vision, also of deafness, as well as the more ordinary disturbance of the function of taste and of the purely cerebral functions.

Moreover, cases are constantly met with where extreme distension of the abdomen and consequent pressure on the hypogastric regions do not induce disturbance of the nervous functions of the lower limbs, although the circulation may be seriously obstructed. Of this, large fibrous or ovarian tumours are familiar examples. I lately met with a case of pregnancy aptly illustrative of the effects of the pressure of the gravid uterus upon the circulation, without disturbing the innervation of the lower limbs. A young woman, unmarried, employed as a housemaid, came to consult me in regard to swelling of the lower limbs. On examination the veins were found in the highest varicose condition, large, convoluted, and prominent from the ankle upwards. She had not menstruated for seven months, and, on inquiry, she admitted the existence of an abdominal tumour. In it a fœtal heart's pulsations were easily detected. The uterus was developed transversely, the head of the child lying in one iliac fossa, and the body directed across to the corresponding part on the other side. It appeared that she had been using her stays so as violently to compress her figure, and hide the abdominal swelling. The result of this was the abnormal position of the child *in utero*, and its powerful pressure against the parts about the brim of the pelvis, causing obstruction of the returning current of blood. There were no cramps in the limbs, nor did she complain of any want of strength in them.

Although the weight and pressure of the gravid uterus affords no sufficient explanation of these affections, there can be no doubt of the fact that the lower limbs are more liable to be implicated than any other part. The cases of chorea related in this paper, affecting the lower limbs, afford striking illustrations of this; for this disease is scarcely known to attack the inferior extremities alone under any other circumstances. Romberg, indeed, says that "the inferior extremities are never attacked alone."[1] But this restricted statement is proved incorrect by the cases occurring during pregnacy, and by other observations.[2]

Mrs H. was first affected with chorea when she was sixteen years of age. Menstruation did not commence at this time, but was delayed till she was eighteen. The motions began in a very slight form, affecting first the ankles only, but soon the whole legs and also the hands and forearms. They were always much aggravated during summer, and continued more or less severe till about the beginning of 1850, when they were either entirely absent or so little troublesome as not to attract her attention. In the course of 1850 she was married, at the age of thirty-two years. After this she continued in very good health till about the middle period of her first pregnancy, which ended favourably in the birth of a male child in July 1851. She had passed through the first half of the pregnancy in good health, but after that she began to be troubled with the choreic motions of the entire lower limbs, and chiefly, as on former occasions, of that of the left side. The movements were described as very annoying, only at night, and chiefly on lying down in bed, when they continued so long as to deprive her of much sleep. When the motions were worst in the limbs, there were occasionally also some affecting the hands, the forearms being still. The movments occurred only in the evening and at night, unless she sat in one position for a long time, as in church, when they sometimes commenced.

The motions had been very troublesome for about six weeks before I was consulted. There was no other complaint. The morning sickness was only occasional. Her appearance was that of a delicate dyspeptic female; the skin was pallid, dry, and harsh; the tongue whitish, and deeply fissured in all directions; the bowels were regular and the urine natural. I ordered the use of large doses of carbonate of iron (ten grains) thrice daily, and a moderate dose of laudanum to be taken at bed-time; also the use of a full diet, with good beer. After this plan of treatment had been continued for some days, she was generally able to get to sleep in a short time after going to bed, without the laudanum. The iron and diet were persevered in till within a few days of her confinement; but for nearly a month before this, the motions had entirely disappeared. Her confinement was natural; only, during the inhalation of chloroform, the choreic movements were reproduced in a troublesome way. The recovery was not unfavourable, except that an abcess formed in one mamma. She nursed her child. In the end of 1852 she was again confined under favourable circumstances; and since the attack in the first pregnancy she has not had any motions worth mentioning.

---

[1] Diseases of the Nervous System. Sydenham. Transl. vol. ii., p. 54.

[2] Nous l'avons vue une fois limitée aux membres inférieurs, mais plus marquée du coté gauche. Grisolle, Pathol. Int., tome ii., p. 611.

Mrs R., a lady of middle age, was travelling in Scotland during this summer. She was pregnant, and was gradually becoming worse and worse in general health, from symptoms which she connected with her being in that condition. In consequence she came to Edinburgh on her way home to England. Mrs R. has generally enjoyed good health, and never has had chorea,—is the mother of several children, the last only of which was stillborn. She is now in the sixth month of pregnancy,—has the look of a woman of originally good constitution, but is at present thin and of unhealthy appearance. Her pulse varies in frequency, is never rapid, but weak; the skin dry and scurfy; she complains of burning heat in hands and feet,—tongue not foul,—suffers greatly from vomiting after every meal,—no derangement of the heart's action, or of the function of the kidneys, can be discovered. There is a copious muco-purulent discharge from the vagina, which is much softened and dilated, and the *cervix uteri* is in a swollen and abraded condition. Late in the evening involuntary movements of the lower limbs come on, causing great annoyance, and continuing till she goes to bed. Generally she falls into a short sleep, which is, however, soon interrupted, and then the motions recommence and continue for hours till she again falls asleep. On awaking, the choreic movements are again entirely absent till near evening.

The treatment pursued in this case was the same as that described in the former, only in addition the *cervix uteri* was twice cauterized with nitrate of silver, with an interval of three days between the operations, and an astringent lotion of decoction of oak bark with borax was injected to the amount of three ounces morning and evening. By these means the leucorrhœa was entirely arrested. For the irritability of stomach a belladonna plaster was applied over the epigastrium, and was followed by great improvement. In about a fortnight the choreic movements disappeared, and my patient left Edinburgh greatly improved in health.

The state of albuminuria, which has been shewn to be intimately connected with some of the most important nervous affections of pregnancy and parturition, did not exist in the two preceding cases. Neither presented any of the signs of disease of the heart, nor had either of them ever suffered from rheumatism. They were both in the better classes of society, and were therefore well fed and cared for, and both were subject only to favourable psychical influences. But in both there was a distinct asthenic state of body, characterized by pallor of the surface, weakness of pulse, dryness of skin, and the other unmistakeable signs of general bad health; and in one the previous occurrence of chorea formed a strong predisposition.

The only condition with which this affection is at all liable to be confounded is fidgets. This latter is not unfrequently observed in pregnant females, existing in the lower limbs to a degree so intense as to assume the importance of a disease. It is generally brought on by either voluntary or constrained abstinence from exercise, and is aggravated by the engorged state of the vessels of the lower limbs, induced partly by the anatomical conditions of pregnancy, and partly by the absence of the contractions of the muscles of the limb. It may be regarded as a muscular affection, and is removed by exercise of the limbs, or by rubbing and kneading the affected parts.

In the two cases just related, the treatment pursued was simply that of symptoms. The numerous peculiar plans of treatment of chorea were disregarded. It was my object, by the use of iron, to remedy the asthenic condition generally, and also to improve the state of the blood indicated by the pallor of the lips and general surface. But I am inclined to regard the use of opiates to procure sleep as having been the chief agent in improving the condition of these females. Both were suffering from want of sleep, of which also they complained. Small doses of laudanum were placed at their bedsides, and if they found the movements continue so as to prevent sleep, they were enjoined to have recourse to the opiate. After a time they improved so as to procure sleep without resorting to this means.

In addition to iron and opiates, various other remedies were used intermediately to regulate the bowels, to palliate sickness, and to meet other passing indications. In one of the cases an aggravated leucorrhœa demanded special attention. The ulcerated *os uteri* was twice cauterized with nitrate of silver, and by this, along with the use of an astringent lotion twice daily in small quantity, and gently injected, the discharge was arrested, a result manifestly favourable to recovery.

It has frequently been remarked by authors, that cases of chorea in pregnancy do not recover till after confinement. Both of these patients, however, got completely rid of the motions after a few weeks of treatment.

ART. IV.—*Tropical Fever and Dysentery; being the result of an experiment by order of Government, in the General Hospital, Calcutta, and Garrison Hospital, Fort-William, in the years* 1849–50, *with a Sketch of the Theory resulting.*

By E. HARE, Surgeon 1st Bengal Fusiliers.

I. TROPICAL FEVER.

The treatment for tropical fever has undergone one of the most singular and sudden revolutions in the history of medicine. Before the year 1638, the complete failure of all methods must have been generally acknowledged, from the immediate and joyful reception which the discovery of Peruvian bark about that time obtained throughout Europe. The Jesuit missionaries undoubtedly owed much of their success in all parts of the East to the cures which they effected by this invaluable medicine, "*the* bark," as it was called, being beyond all others the most precious. Its value in those days may be estimated by Bruce, the best informed man of his day, selecting bark as the only essential medicine for his Abyssinian expedition; and his wise choice was quickly proved; for he owed the whole of his favour with the Abyssinian court, to his cure of the king's and minister's son of severe[1] remittent fever by it; and his own faith in its virtues is rather ludicrously shewn by his instantly cramming himself with it the first moment he felt unwell.[2] The mode of administering bark in those days is well described by Lind, Hunter, and Clarke. Dr Hunter, speaking of bark and remittent fever, says, p. 86, "No disease requires more speedy assistance; for the efficacy of the medicine employed depends in a great measure on its being given early. The disease gains strength by repeated attacks, and is fatal or materially injures the constitution. To wait for any crisis would be time irrecoverably lost. After a purgative,—it is of no great consequence what kind of purgative be given,—a little Epsom salts,

[1] Vol. ii., p. 179.

[2] After shooting in some marshes.

jalap, or rhubarb will do. After a few stools have been procured, to make it sit easy on the stomach, commence immediately to give bark; clearing out the bowels quiets vomiting, and moreover causes a quicker absorption of the bark into the system."

He commenced with a drachm dose every two hours, and his great anxiety was to give as much bark to his patient as possible without causing vomiting. "The dose," he says, "may be increased to two drachms every hour, but few stomachs will bear as much."

Forty-eight tea-spoonfuls of bitter sawdust daily, in a stomach sickened with fever, without doubt few could bear. If Dr Hunter had had quinine, what vomiting he might have saved his patients. He did, however, his best to make the bark "sit easy." He tried infusions, decoctions, and mixtures with milk, coffee, wine, &c., all of bark, for bark must be given in some shape.

His ingenuity was taxed to the utmost to get bark down, and keep it there, and when he could not, he says he always lost his patient. If the bark purged, he tried to stop it that road too by giving laudanum. Dr Lind (p. 332), when the stomach positively would not receive more bark, injected it up the rectum, and cured bad cases in that way. Children who could not swallow much of the bitter wood, he always treated by clysters of bark, and actually dipped them in baths made of the bark, and applied large poultices of it to the stomach; so determined was he to saturate his patients with the antidote by any means, and all means. Dr Lind (p. 94) objects strongly to emetics—why? Because he says they often make the stomach irritable, and not able to retain his favourite bark. "By emetics," he says (p. 121), "the stomach is rendered incapable of receiving the bark, the only medicine we are yet acquainted with that possesses power to stop the progress of the fever. In all cases, a vomit ruffled and fatigued the patient more than a purge, without procuring equal relief, and if a vomit were given during the paroxysm of the fever, it was generally necessary afterwards to give a purge before the bark could be administered, by which a whole remission was often lost." Again (p. 94), "If there be sickness and vomiting at the beginning of the disease, chamomile tea or warm water are sufficient to cleanse the stomach."

"The stomach is also relieved by a purgative glyster. In this way the vomiting is often quieted, and the stomach enabled to retain the bark." He thus gently and carefully labours to prepare the stomach to bear his bark without delay.

Again (p. 90), "In severe cases of the fever, in which it is absolutely necessary to wait for the remission, whenever the pulse becomes a little slower, and the heat begins to abate, a dose or two of the infusion of bark may be given, and the powder added afterwards, as soon as the stomach will bear it." But it is only in severe cases, and from absolute necessity, that he stops his bark during even the paroxysm; and this necessity was no other than the vomiting which the large quantity of bitter sawdust always caused during the height of the paroxysm; for (p. 123) he says, "Bark generally excites sickness and vomiting, if given during a paroxysm of the fever. A desire of administering early this great specific sometimes led to this practice, but nothing was gained by it, as it was almost always rejected by the stomach,—nay, there was some danger of raising a disgust in the patient to the medicine, which might continue during the remission."

It is evident, therefore, that it was not from fear of increasing the violence of the paroxysm, or causing inflammation, that he stopped giving bark, but simply because it was vomited up again. But when the stomach settles a little, he watches his opportunity, and happy and right skilful does he think himself, if by any manœuvring he can gain an hour or two on the enemy, and enable his patient to take the bark at the first possible moment. Thus he disguises his bark with all sorts of nice things, milk, coffee, wine, &c., that it "may sit easy (he says) on the stomach, and no time be lost. By these expedients the stomach is reconciled to the medicine, and enabled to receive a larger quantity and retain it better. When the method of cure laid down above is carefully put in practice from the beginning, it will in many cases prevent a return of the fever. In general, however, a sufficient quantity of bark cannot be given in the first remission, nor is there time for it to produce its effects on the body, so as to prevent a second paroxysm."

When this paroxysm is a little cooled, he sets to work again, and says, "The second remission, as well as the first, is to be employed in administering the bark freely. In this way above two ounces may in general be got down before the fever returns, and this will in most cases be sufficient either to prevent entirely the next accession, or so far break the force of it as to render it void of danger."

He speaks (p. 122) thus positively of the demonstrated evidence in favour of bark—"The prejudices that formerly existed against bark are no longer in being. They were founded in idle speculations of the learned, but even with the vulgar they are now extinct. Any attempt, therefore, to prove that the obstructions of the viscera are the effects of the disease and not of the medicine, would at this time be deemed superfluous. The great, and indeed the only evil arising from the bark that has fallen under my observation, has been to excite sickness and vomiting during a paroxysm."

To this truth, Lind, Clarke, Hunter, Balfour, Bruce, and a host of others, for nearly 150 years till 1808, gave their united testimony. From the West and East Indies, Africa, and America, the universal assent was,—Bark is the antidote to tropical fevers; he that gives most of it, and soonest, will save most patients. The purgative effect of jalap does not rest on better evidence.

The scene, however, now changes. Dr James Johnson arrives at Calcutta, as surgeon of a ship, and fresh from the schools. He anchors in Diamond Harbour, and severe fever from those notorious swamps soon attacks the crew. He then says (p. 36), "I thought myself fortunate in possessing two celebrated authors, Clarke and Lind. I read them by day, and I studied them by night; in short, was quite anxious to grapple with this Hydra disease."

Let us see what use he makes of this study in his first case (p. 37).

"I first gave an emetic, and a remission follows. I now, with an air of confidence began to throw in the bark, quite sanguine in my expectations of soon checking this formidable disease; but alas, my triumph was of very short duration, for in a few hours the fever returned with increased violence, and attended with such obstinate vomiting, that although I tried to push on the bark through the paroxysm, it was all in vain, for every dose was rejected the moment it was swallowed, and I was forced to abandon the only means with which I had hoped to curb the fury of the disease. The other methods which I used need not be enumerated, they were mere temporizing shifts, calculated to obviate occasional symptoms. The plain truth was, that I knew not what to do, for the sudden and unexpected failure of that medicine on which I was taught to depend, completely embarrassed me, and before I could make up my mind to any feasible plan of treatment, my patient died."

This case scarcely requires comment. The two points which Dr Johnson's study of Lind ought to have warned him from, viz., emetics, and pushing bark *through* the paroxysm, lest the stomach be so nauseated that it will not retain the medicine afterwards, are the very points neglected by Dr Johnson; and yet on the result of this badly treated case, he dashes away bark, and the experience of 150 years, and strikes out a new treatment of his own, viz., salivation by scruple doses of calomel, with copious bleeding.

"Henceforth (he says) I carried the evacuating plan with a high hand. If I gave a purgative, I always added to the calomel, mercurial frictions, to prevent a halt, in the pursuit of my ulterior and principal object ptyalism;" and in p. 51 he exultingly declares, "that there *may* be cases wherein the use of wine, and even bark is indispensable, I shall not attempt to deny, but the latter in particular I NEVER had occasion to employ;" and he ridicules Dr Balfour (p. 55), for trying to combine the bark and mercurial parties as he calls them.

Dr Johnson wrote his classical book on tropical diseases soon after this date, and by its universal circulation, and his talented Review, accomplished a most wonderful revolution in the treatment of tropical fevers. Bark, the proved antidote, was henceforth discarded, and these fevers treated solely as if they were inflammatory diseases, by large depletion and salivation. This system of treatment continued till 1833. About this time the miserable effects of repeated salivations had been observed, both in India and England. A man may be salivated and cured once of a severe fever without much injury to his constitution, but relapses are frequent, and the same patient becomes year after year the subject of fever, and requires repeated salivations, which must eventually destroy his life or health. The wretched cachetic condition of retired Indians in England became a proverb. Moreover, in the bad forms of fever it is often very difficult to salivate. An enormous quantity of calomel was given, which remained inactive in the stomach and bowels, but when reaction commenced, this was rapidly absorbed, and often produced fearful ptyalism, and instances were not uncommon — I have known them even during my service in India—where large portions of the jaw exfoliated.

In 1816 Dr Halliday published, with severe remarks, numerous cases shewing the horrible effects of the mercurial system, and states that 13,237[1] grains of calomel were given in the General Hospital in one month. The controversy became warm, and the language so violent, that it was silenced by order of government, and the scruple doses of calomel continued till Dr Twining's time, 1833. He recommends less calomel, but copious and repeated bleedings, with drastic purgatives. His book still advocates the inflammatory theory of fever, and in several passages he shews his belief to have been, that inflammation of internal organs,[1] the liver, spleen, &c., is the *cause* of the paroxysms of fever, and that till it was subdued it was highly dangerous to give quinine. I have his MS. cases from the diaries of the General Hospital now before me.

---

[1] The average admissions for fever and dysentery into the General Hospital have been 35 patients monthly for the last twenty years.

In them also Dr Adams, a contemporary of Twining, shews his belief in this doctrine, and remarks on a *post-mortem* of remittent fever, that " it was connected with, or perhaps *depending on!* severe determination to the lung." It was, in fact, the common theory of those days; Broussais asserting that fever depended on inflammation in the bowel, Dr Clutterbuck on that of the spine, &c. It is most interesting in these diaries of the early practice of Dr Twining, to trace the gradual forming of his treatment, as, decreasing the doses of calomel, he substitutes for it bleeding and purging. Quinine too had now been discovered, and was used by him in small doses as a tonic, in the latter stages. I need but refer to Dr Twining's published cases to shew to what excess depletion was carried by him. Many even of his published cases, and still more in his MS. diaries, shew strong evidence of Marshall Hall's reaction from excessive bloodletting having been treated by Dr T. as relapses of fever, by more purgatives and leeches.

Twining is still our standard authority, and so is Annesley, who came next, but with little change of treatment, except that his bleedings were more moderate. Bark is used by him also as a tonic in the latter stages, and he says (p. 502), " At this period bark may be ventured on, and its effects carefully watched, to preserve the energies of the frame against the depressing agents which surround it, and the exhaustion consequent upon previous excitement. As long, however, as the tongue continues dry and the skin hot, the local determinations and inflammations are still unsubdued, and the exhibition of bark contra-indicated."

Dr Corbyn's *Medical Journal*, published throughout this period, contains ample proof of a continuance of the antiphlogistic treatment for fevers in the East Indies by all our practitioners, and contains numerous hints at the folly of the old bark system, particularly the No. for September 1839, where Dr Grant condemns even Twining's small and late doses as too large; and for other tropical countries[2] we may consult Dr James Johnson's book, and his *Med. Chirurg. Review* throughout, to the date of his death in 1849.

---

[1] See the note to the commencement of his chapter on Fever.

[2] Except a few writers in America, where, moreover, the early quinine practice was by no means general.

A most instructive account of the effect of the calomel treatment of malarious fever, may be found in Dr Oldfield's history of the Niger expedition under Laird in the Quorra and Alburkah steamers, published in 1837. He says (p. 121), " The cause of this fever, in my opinion, was the want of sea-breeze, and the prevalence of the deadly miasma to which we were nightly exposed from the surrounding swamps." Again (p. 308), " The patients alluded to were cupped and blistered, their heads shaved and cold lotions applied, purgatives administered, and afterwards five to ten grains of calomel given, every four to six hours, till ptyalism was slightly produced. Smith, being a robust man, was bled to thirty ounces." (P. 345)—" I treated Captain Mitchell in the usual way, but the calomel took no effect on the system. I felt assured he would not recover; for in several cases where it had failed, death had most surely taken place in a few days."

In the year 1845 appeared our last authority for East Indian fevers, viz.,—Johnson and Martin *on Tropical Diseases*. This contained a new edition of Dr James Johnson's work, reiterating his continued confidence in his former opinions, after, he says, fifty years' experience; and the book was rendered especially authoritative to us in Bengal, from its having also the sanction of the name of Dr Martin, who was long in extensive practice in Calcutta. Dr Martin writes thus (p. 123)—" Early and copious bleeding at the very onset, is very generally necessary in the severer forms of Bengal remittent fever; then full doses of calomel, with sudorifics, short of producing salivation, with saline purgatives in the intervals. If the disease does not now yield, but, on the contrary, if the secretions become watery or suppressed, and the paroxysms recur at shorter intervals, or with increased severity, leaving but imperfect remissions, then there is imminent danger—and inflammation or acute congestion, in some important abdominal or other organ, may be more than suspected. For this, in addition to abdominal bleeding, mercury, in small repeated doses, with antimonials, must be given mildly, to affect the system. It is the only known means of saving the patient, by anticipating the destruction of some organ essential to life. It here becomes—in the apt words of Dr Jackson—a remedy of necessity. Where the remissions, on the other hand, are well marked, quinine in full doses (gr. iij. to gr. v., three or four times a-day) should be given without waiting for *every* thing. Some practitioners recommend that, before this drug is used, we obtain previously a clean tongue, natural secretions, and the absence of all heat of skin or local affection. I believe this to be a very dangerous practice. If we are to wait for everything, we shall often wait too long, or till it is too late. I have always administered quinine in the more favourable cases now stated, in disregard of certain abdominal complications (those of the head should generally exclude it), believing that if I arrest the paroxysm, I do greatly more towards the cure at large than quinine can possibly do of harm to the local affection, the treatment of which, by local depletion and counter-irritants, is not interfered with by this means. Again, all tenderness on pressure or local affection does not, in the case here stated, necessarily constitute inflammation."

My experiment in Calcutta was to prove, that "mercury" is not the only known remedy, or a remedy of necessity, when the remissions are imperfect, and there is acute congestion of some important abdominal or other organ, but that quinine ought to be given in such cases in enormous doses of from ʒi to ʒii daily; and the more acute the symptoms, and the less the remissions, the larger and earlier are the doses of it required. That head symptoms should not generally exclude quinine, but demand its fullest and largest quantity, and the earlier the better; that it is most fatal waste of time "to wait till the remissions, on the other hand, are well marked" before quinine is given; and that so far from not waiting for "*every* thing," we ought to wait for *nothing*, but if the fever be undoubtedly malarious, and with bad symptoms, proceed at once to saturate the patient with the antidote; and that a grain even of calomel is in no case required. By following this system, in a public experiment of twelve months' duration, in the General Hospital at Calcutta, my mortality by fever proved to be one-twelfth only of the average mortality there for the last twenty years. The history of the experiment is thus:—

In April 1843, after my return from the Cabul campaign, I was sent with my regiment to the borders of the Nepaul Terai, which is a belt of forest in breadth about sixty miles, and extending along the foot of the Himalayas. This jungle is healthy and safe during the cold months, but during the rest of the year is so deadly that even the birds and animals desert it. A dead silence prevails through the extensive forest, and no European or native of the plains can pass even one night in it without almost certain death.

Fever became here my favourite study, I procured all the books I could hear of on tropical fevers, and among the rest, Hunter, Lind, and Johnson. The sudden change of practice introduced by Dr Johnson astonished me, and appeared, on careful consideration, founded on no evidence at all, when compared with the mass of witnesses against it; and I thought I could trace the reason why this one case made so deep an impression on this talented physician, as to induce him to give up all further trial of bark, and attempt at once bleeding and salivation.

The first edition of Dr Baillie's *Morbid Anatomy* was published not much more than ten years before Dr Johnson's arrival in Calcutta. It is well known what excitement and fresh energy this book gave to our profession, and set every one to search for the causes of disease in the dead body[1] and *post-mortem* appearances. Before his time physicians never dirtied their fingers with dissection; and I have no doubt that Johnson, just arrived from the schools, had largely imbibed the notions of the day, which tended, in discarding the humoral pathology, to attribute all diseases, and even fevers, to local lesions. In those days, too, the signs of inflammation were very imperfectly known, and the difference between congestion and inflammation had not been recognised. Where blood was found in quantity, there also was supposed to have been inflammation, or if not inflammation, yet that the congestions themselves required free bloodletting. It was not known then that congestions may be produced by depletion, chlorosis, and the mixture of poisons with the blood, &c.

That this is a correct view of Dr Johnson's reasoning, I think must appear evident to any one who carefully considers his description of the *post-mortem* in the above case, and his immediately resulting practice.

He says (p. 39) "He found the liver so gorged with blood, that it actually fell to pieces on handling it, indeed it appeared as if the greater number of the vessels had been broken down, and almost the whole of the interior structure converted into a mass of extravasation. The gall bladder contained a quantity of bile like tar. In the brain marks of turgescence, particularly in the venous system, were found." He bleeds and salivates his next patient. The origin of his scruple doses of calomel is evident by his own cure by them of dysentery, which he also believed to be purely an inflammatory disease. See his description of his own case, p. 186.

But theory alone, even supported by Dr Johnson's talents, would not have been able to supplant the established practice, had there not been in it evident deficiencies. Bark, in its bulky form, could not cure the worst forms of fever. The already sickened stomach was not able to digest or endure such quantities of the woody powder. Well assured as I am now of the certainty of cure from quinine, I doubt much if I would trust a bad case of fever to bark alone. It must cause inconceivable distress and vomiting to the patient, without the slightest benefit, for it cannot be retained. I can therefore readily imagine the delight with which the elegant and tasteless medicine, calomel, was resorted to, with depletion, which coincided so well with the theory of the day. And the result, I know from my own experience, must have been successful, quite as much so, perhaps, as the treatment by bark in its crude state. There cannot be a doubt, that if not calomel, yet certainly salivation is an antidote to malarious fever. The instant a patient's mouth is sore the fever leaves him. The mercury produces not the slightest effect till then, but from that moment the disease vanishes, as if charmed. The change is from death to life, from extremity of suffering to calm and comfort.

---

[1] It destroyed for the time the humoral pathology, now again reviving.

We all in India have had multiplied experience of this, but one should read Dr Johnson's own case of malarious fever with dysentery (p. 186). "As my night closed in, the exacerbation was great, and I was again delirious. On the third day my fever ran higher, with hot dry skin; and as night approached my apprehension of the usual exacerbation brought on extreme mental agitation. The surgeon endeavoured to cheer me with the hope of ptyalism, which he assured me would alleviate my sufferings [all this time he was taking calomel in scruple doses.] As the night advanced my symptoms became aggravated, and I was convinced a fatal termination must ensue. Suddenly at four o'clock of this same night salivation appeared, succeeded with such agreeable sensations, that I ejaculated aloud my heartfelt gratitude to Heaven for my deliverance." Numerous instances, too, of the safety which salivation gives from the effects of the malarious poison, may be found in Dr Johnson's book, viz., patients salivated for syphilis sleeping with impunity in places which were fatal to every one of their companions; and also many cases are on record of officers in India passing in a state of salivation by Dâk, unharmed through the most deadly jungles.

Dr Johnson's new system was therefore offered to the world with proofs of its efficacy not often attained in medicine. He had in fact, without knowing it, discovered a new antidote to malarious diseases, and which, though used on wrong inflammatory theories, must have been nevertheless decidedly successful. But in more recent days we have been to blame.

It has been for some time known, that many of the worst forms of fever cannot be salivated with any quantity of mercury, and these all die. Bark has been given to us in the elegant form of quinine, yet, except in agues, it has been scarcely used by us in India, except as a common tonic in small doses at the end of the disease. The ill effects, too, of the large doses of calomel were observed; its real efficacy as an antidote by salivating, and not as an antiphlogistic, had not been inculcated, and our treatment has gradually degenerated into the mild antiphlogistic system for smallpox, typhus, and other diseases, for which we have no antidotes.

Most of my service, when I first came to India, openly professed their adoption of the expectant treatment for all malarious diseases. Some few, however, of the more enlightened did, as Dr Martin quoted above, urge the more free use of quinine, after a remission had been procured, and without waiting for the removal of all organic congestions. But since the discovery of the invaluable quinine, we have wanted a bold return[1] to the practice of our fathers; for in quinine we have all the qualities required. It is not a bulky medicine, and does not cause vomiting. It is a perfect antidote, and will cure all cases of malarious fever not otherwise hopeless, and it has no bad effect like mercury on the constitution. The gradual introduction of quinine in place of calomel has left our mortality by fever much the same as in former days; but by the expectant treatment for malarious dysentery, where quinine has not been substituted for salivation, the statistics of the General Hospital for 25 years shew that the mortality is actually double now what it was under the salivating treatment, viz., from 1826–1833 there was only one death in six admissions, whereas from 1846–1850 there was one death in every three admissions.[2]

The diaries, too, of this Hospital from 1820 to 1850, give the deaths of more than 600 cases of malarious fever, and in only three[1] of the *post-mortem* dissections is any trace of inflammation mentioned. They are all such as Dr Johnson's case quoted above, viz., extreme venous congestion of the liver, spleen, mesentery, and brain. In the two last ecchymosis is often described, and in the brain constantly effusion of limpid serum.

---

[1] I refer here only to our practice in the East Indies. A far better treatment has often been advocated in America, on principles almost the same as mine. But in the East Indies, before the publication of my pamphlet in November 1846, quinine was never thus used. The Medical Board who receive the cases and returns of our army, must have been well aware of what the general practice was, when they officially recommended to Government my treatment as new, for an expensive experiment. The general feeling indeed was, that it would cause great mortality, especially in cases with head affections. The discovery of the treatment must be credited to the seventeenth century. Dr Goodeve (p. 13, 1844) says that quinine should not be given till at least the third remission. Dr Hutchison, Secretary to the Medical Board (p. 133, 1845), recommends it only "when the fever has *disappeared*," and so on all our writers.

[2] See Table in the next Number at the end of Part II.

Now it would seem *a priori* probable that such cases of congestion as Dr Johnson describes, and which are very common, viz., "the liver so gorged with blood, that it seemed broken down in its interior structure;" it would seem probable, that in some such cases less severe, an effort for repair might commence in some instances, and effusion of lymph be found after death, or pus, the result of inflammation following the injury of the textures. Yet nevertheless, only three such cases are described in the records of the General Hospital, by a large number of observers, from 1820 through the long period of thirty years: and who have always been the best men of our service. The conclusion, therefore, may safely be relied on, that malarious fever is very rarely indeed accompanied with inflammation of any internal organ, and that, dropping all theory of the cause of the disease, we find from inspection that it destroys life, simply by an accumulation of blood in the veins of the brain and all the abdominal viscera. Now, quinine has the power of stopping these accumulations, and though we may occasionally relieve the present tension of the viscera by local or general bloodletting, yet the congestions will again occur, and eventually destroy life, except we administer their antidote as early and in as large doses as possible; and the experience of those who used bark, and my own experiment in the General Hospital, shew not only the safety, but the immense advantage of this treatment. I am myself convinced that malarious fever is caused by a blood poison of a most depressing kind, and destructive of all vital action, and that the excitement which we see in these fevers is far from the stimulus of the poison, but is the last effort of the vital power, not quite overcome, struggling to relieve itself from the Lethean influence. The most dangerous form of this fever is not when this struggle is strong, which is a most favourable symptom, but when vitality is so depressed that no distinct reaction takes place, the skin changing from hot to cold twenty times and more in the day, with partial sweats here and there in parts only of the body, or where, as in the algid fever described by Dr Haspel,[1] in Africa, "the skin is icy to the touch; a subtle poison appears to be preying upon the vitality of the economy, the patient resembling a breathing corpse; the tongue and breath are icy, the voice extinct," and, as he adds in the next page, "the surface generally blue," from the accumulation of the whole blood of the body in the veins. I have seen very many of these cases, and anxiously watched over them while I administered enormous doses of quinine in solution. I have had patients lie in this state more than forty-eight hours, and the first symptom of recovery was violent reaction, which I hailed with delight; for then a little cold water to the head, and the continuance of quinine, rapidly restored the patient to health. I am convinced by such cases that we are wrong in considering quinine merely as an antiperiodic.

---

[1] The three cases of fever referred to in the text are described thus in the diaries:—

1. *John Abbot, Fever, died 13th October* 1834.

Dr Twining's case. Brain gorged with blood in its veins. Effusion of coagulable lymph between dura mater and pia mater in small quantity. Serous effusion into and softening of the brain, which was very vascular. Liver large and soft; much vascularity of the mesentery.

2. *W. M'Intyre, Fever, died 8th July* 1844.

Dr Rankin's case. Great cerebral congestion, with coagulating lymph on the surface of the brain, and limpid serum in the ventricles, and at its base. Other viscera perfectly healthy.

3. *Owen Carolan, Fever, died 11th July* 1844.

Dr Rankin's case. Died delirious, having been tranquil in the early part of the day; cerebral vessels extremely turgid. Blood oozing from the surface, and limpid serum in the ventricles, and at the base of the brain.

It is indeed a most valuable and remarkable fact, that during 30 years from 1820, in the largest hospital for malarious fever in the world, only these three cases of inflammatory results are found, and that the liver, spleen, and viscera have *never* shewn marks of inflammation or abscess from the effects of malarious fever.

These were the arguments which, after careful trial for three and a half years, I published in a small pamphlet, together with a treatise on Dysentery at Delhi, in November 1846. It excited attention, and numerous successful cases by others were published in our Indian periodical, then edited by Dr Edlin. But notwithstanding its favourable reception, and, so far as its treatment had been tried, successful result, the seniors of our service especially, and many of my friends who had been greatly persuaded of its safety, yet confessed freely their fear of giving quinine in fever, when the head was affected. They said, and justly, that the treatment by calomel for these cases had been always considered by every one so imperative, and the action of quinine on the brain so like a stimulant, that they were afraid to risk the lives of their patients by giving them quinine in the large doses required, till it had first been extensively tried. At last the discussion on this, and my plans for dysentery, were so prominently brought to the notice of the Medical Board, that they wrote to the Governer-General, urging a full trial of them at the General Hospital. The Punjaub war broke out, and all was in confusion; but when it ceased, the Board applied again to Lord Dalhousie, who immediately assented, and arranged himself the general plan of the experiment, and I was ordered to proceed to Calcutta. A large ward in the General Hospital was set apart for my patients, and I was placed under the orders of Sir James Thomson and the surgeon of the hospital, Dr Nicolson, by whom my patients were inspected; and my cases and reports, with every dose of medicine, were sent through them monthly for the information of the Board. Twelve months, to comprise all the seasons of the year, were fixed as the term of the trial, and my results compared with the average mortality for fever and dysentery at the same hospital for the twenty previous years. All the fever patients, on admission to the hospital, were sent to my ward for treatment, which was without exception simply Dr Lind and Hunter's system, substituting only quinine for bark. I ordered my apothecary to give ℈j of quinine the first moment of admission, before I could be called to see the patient, and one dose of castor-oil, or jalap and cream of tartar, if the bowels were not loose. If they were, this was omitted. The patients afterwards took three, four, or six scruples of quinine during the twenty-four hours. The more severe the fever, the more frequently was the ℈j dose administered, till complete cinchonism was produced, viz., singing in the ears and deafness. General bleeding and even leeches were rarely required, and only for plethoric sailors, just arrived in the country. For these one bloodletting (it was never repeated) often gave great temporary relief, and I always used it strictly on Dr Mackintosh's principles, just before the accession, to break the first paroxysm, and give time for the quinine to act. Bleeding after the paroxysm, even where the fever is decidedly continued, is often highly dangerous. I believe, too, that moderate bloodletting assists greatly in procuring the rapid absorption of quinine into the blood. Leeches were very rarely used. Purging I greatly object to, for my experiment on malarious dysentery convinced me that it prevents absorption of the quinine.

---

[1] Brit. and For. Review, Oct., p. 358, 1852.

The bowels, however, require to be freed from their foul contents, caused by the morbid secretions of the congested liver and intestines. For these, if allowed to remain, cause sickness of stomach and vomiting of the quinine. I prefer much large enemata to purgatives, in all cases of fever; but my syringes were in such constant employment for my dysentery cases, that I could not conveniently use them for fever also. I would in fact treat with perfect confidence any case of malarious fever, however severe, with quinine and cold water to wash out the bowels and apply to the head, liver, or other congested organ. I by no means, however, deny the extreme relief that a moderate bleeding at the proper time often gives to a plethoric man—leeches I have found of less value.

I treated in this way 129 patients with one death, the average in the General Hospital for the last twenty years having been one death in every 11¼ fever cases. I had therefore less than one-twelfth part of the average mortality.[1]

Among the long-neglected cases of debauched sailors from the streets and shipping of Calcutta, who are the majority of the patients of the General Hospital, and are often sent there after they have been lying sick many days on board their ship, or wandering about the bazaars of Calcutta, till they are picked up insensible by the police, and deposited in the hospital, I had, of course, most numerous patients in all stages of delirium and congestion of the brain, and many with coma, and black sordes on the teeth and tongue; some also with acute and ardent symptoms; but all had the same unvaried treatment, the only distinction made being more frequent doses. For in these dangerous symptoms I always pushed the quinine most freely, yet the result was as above.

But the Government were not content with this one trial. I was placed likewise under the orders of the surgeon of Her Majesty's regiment then in garrison at Fort-William. The dysentery and fever cases of half, viz., five companies of the regiment, were sent to me, and the remaining cases from the other five companies to the surgeon. The surgeon had the ward in the upper story, I had the lower,—a difference which all who have treated malarious diseases in Bengal, know to be very considerably against me. My cases were kept in a book, with the medicine prescribed, and recorded symptoms, open for the inspection of the surgeon, who daily visited my patients. The result for fever was 292 cases and two deaths. The surgeon had 279 cases and four deaths. That is more than double my mortality, though his cases had quinine in the usual doses after a remission had been procured. The saving of life in my ward was therefore procured by my scruple doses to saturate the system at once. The Board also acknowledge in their report (p. 22), that the average period my patients were under treatment was very much smaller than that shewn by all the other returns received by them from the whole Presidency during the same period.

---

[1] My figures will be taken throughout from the Report of the Medical Board, published by order of Government, and circulated to all medical officers in the three Presidencies, and a copy of which is herewith sent to the editor of this Journal.

I thus treated in all 421 cases of Bengal fever, and during the experiment some remarkable facts were observed.

My orders to my apothecaries in both wards were, to give scruple doses of quinine to every patient with symptoms of fever, from the very first moment of admission; and they often thus got 40 grains of quinine before I saw them. During part of the year, viz., March, April, and May, smallpox and measles raged like an epidemic in Calcutta. Numbers of these patients, in their early stages, before the appearance of any eruption, were sent to my ward as fever cases, and were treated as the rest with large doses of quinine, sometimes for thirty-six hours before I could detect their disease. Almost all these cases terminated fatally. Latterly, however, I was able to avoid these errors, by watching the effect of the first dose of quinine. For in cases not malarious it invariably caused great uneasiness, without any benefit to the general symptoms. Moreover, deafness and singing in the ears were very quickly induced, whereas in malarious fever, with the same ardent symptoms, the quantity of quinine taken, without producing any cinchonism, was often extraordinary, and so far from uneasiness, it seemed always to give the greatest relief, and the febrile symptoms yielded rapidly under its use. One scruple dose of quinine will cause most evident feelings of cinchonism in a healthy man, and yet a bad fever case will take four to six scruples daily for often three days, without any symptom but recovery. The same injurious effect of quinine was remarked also in delirium tremens cases, and the febrile symptoms consequent on a debauch. Should this curious fact be ascertained also by others, it seems to me that we have in it a direct test for the malarious poison; as decided a test, in fact, as can be given by any in chemistry, the effects caused by the poison, and their change by the antidote, being as visible to the senses as the change of colour or smell caused by the tests for arsenic. But lest I be considered a theorist, I will quote the authority of Dr Holland,[1] who says—

---

[1] Medical Notes and Reflections, p. 150.

" And as bark, by curing, associates together many intermittent affections, under the presumption of a common cause, so may colchicum furnish similar inference regarding affections seemingly remote in situation and symptoms. This mode of inference, hitherto very limited, may hereafter be extended much further. It is one of singular value, as closely connecting the treatment with the theory of disease, and rendering them mutually corrective of each other. It at once enlarges and simplifies our views, in every part of pathology; each fact well ascertained is fruitful of results far beyond those which directly appear to the view."

Again (p. 348), he enumerates these as proofs of a disease having a common origin with ague, viz., malaria.

1st, By their intermissions.

2d, By their succession to, or alternation with, intermitting fever.

3d, By the influence of the *same* specific remedy in removing or relieving such disorders.

He says the application of these proofs to particular cases might be largely made. Again (p. 354) :—

" The influence of certain medicines, and particularly of bark, in curing even the most anomalous varieties of these intermittent disorders, is a fact of great interest. Like the use of mercury in obscure syphilitic affections, or colchicum in the most irregular forms of gout, it enables us to denote and class together symptoms apparently the most remote in kind, but which presumably could not thus be relieved unless depending on some common cause. We have therefore, in the specific nature of the remedy, a sort of practical test of the character of the disease, of great importance to the consistency and success of our treatment, and related, through the principle of the inquiry, to some of the most interesting questions in all pathology. This it is, as was noticed in speaking elsewhere of colchicum, which gives peculiar value to all that illustrates the action of these remedies; they are the interpreters of facts far beyond their momentary effect, and of connections between morbid states which are in no way so definitely made known to us."

The success of this method in the diagnosis of various forms of gouty dyspepsia, headache, rheumatism, and gouty diseases of the eye, joints, and heart, are well known, and they are now arranged as the effects of the poison of gout, because they are cured by colchicum. So also many most anomalous intermittent affections, first remarked by that excellent observer Dr Maculloch, have since been acknowledged as malarious diseases, because they are cured by bark;

and thus, again, are doubtful syphilitic eruptions, &c., tested by mercury. Now, what Maculloch has done in a cold climate for the chronic forms of malarious disease, I believe may be accomplished also for the acute forms in the tropics; and my Calcutta experiment goes far, I think, towards fixing them.

On my first arrival in Calcutta, having seen the diseases only of the very different climates up-country, I felt much anxiety lest the success of my treatment, which had been great, should fail me there, from the different character of the diseases of Calcutta, and especially of dysentery. For fevers I felt less doubt, for I knew that except in the colder climates of the hills of Cabool, typhus and typhoid fever from animal contagion were not found. Nevertheless, even for fever I felt some misgivings, when I read in Dr Twining's work numerous varieties of fever distinguished, viz., ardent, continued, remittent, congestive, intermittent, and a peculiar fever he calls a walking fever, from the patient continuing to transact his business without knowing his danger.

Dr Twining practised himself in the same General Hospital, and was an accurate observer. These forms of fever are indeed all found there, and are beautifully and correctly described by him. A few days' study, however, of his manuscript case-books in the record office of the Hospital, soon convinced me that all these fevers, differently as they are named, are but varieties of one and the same disease,—a patient admitted with intermittent fever falling into a continued form, and *vice versâ*; again, a man admitted with congestive fever, suddenly rouses into the reaction of an acute burning paroxysm, which may remain in a continued form for days, and which last frequently terminates in common ague.

I found, therefore, here two of Dr Holland's proofs of identity, viz., 1*st*, by their intermissions; 2*d*, by their succession to, or alternation with, intermitting fever; and I proceeded with confidence to establish his third proof, viz., the influence of the *same* specific remedy in removing or relieving such disorders.

This last was most completely established. In 421 cases,—some of them admitted in almost a hopeless state, being comatose, with sordes on the tongue, &c.,—there were but three deaths, and one of these three was plainly caused by delirium tremens and excessive spirit-drinking; but the surgeon thought otherwise, and reported it as a death by fever. However, the difference is nothing; and taking the figures as the Board's official report gives them, the conclusion to me is irresistible, that all these were malarious fevers (and they were cases during all the seasons of the year).

It would have been very different had the single medicine which I used been an evacuant. For multitudes of diseases arising from very various causes are benefited by increasing the secretions; and it is thought by some few, that colchicum cures by its evacuations, and not by its specific properties. But in my experiment a medicine was given which produces no increased excretions, and in such enormous doses and indiscriminate way, that it must, if wrongly applied, have caused great mortality; and it actually did so in those cases of smallpox and measles where mistakes were made.

It is known also, in fever especially, what fatal results always follow a wrong system of treatment, pertinaciously continued; and yet quinine reduced the deaths from these various forms of fever, given largely and universally in all of them, to $\frac{1}{12}$th the average mortality.

In reasoning thus on the simplicity and oneness of the natural agents of disease, I could not fail applying the same tests to dysentery also, the other subject of my experiment.

RANGOON, *August* 29, 1853.

(Part II. in next Number.)

ART. V.—*On the Nature of Inflammation.* By JAMES CAPPIE, M.D., Edinburgh.

It seems now to be generally admitted among pathologists that inflammation is essentially a form of abnormal nutrition, and in this general view of its nature we entirely agree. That the vital operations in the tissue inflamed are in the first instance affected, and that the phenomena which manifest themselves during the progress of the morbid action, are to be more or less directly referred to this derangement, we think there can be little doubt. Such a statement, however, gives no explanation of the phenomena themselves. The question as to the exact manner in which nutrition is modified when inflammatory action is set up still remains. Before the theory of inflammation can be considered as complete, it must be determined what those intimate changes are which occasion the phenomena by which its presence is usually recognised, or which are certainly known to result from its occurrence. Considerable diversity of opinion still exists in regard to these details, and in their investigation there is still ample room for inquiry and speculation.

A theory on the nature of inflammation must then be based on the assumed correctness of particular views on the nature of nutrition. The latter process must be carefully analysed before any satisfactory progress can be made in unfolding the nature of its abnormal forms. Nutrition itself is not a simple action; it involves the co-operation of a variety of agents, and is constituted by a comprehensive combination and succession of phenomena. These phenomena must be minutely studied individually; we must become acquainted with their conditions and healthy modifications, and know the qualities and particular offices of the various structures and agents concerned in their production. The more extended and precise our knowledge is in these respects, the better qualified shall we be to judge of the nature of deviations from the healthy standard. Where obscurity rests on any point of importance, the theory of inflammation must remain, to a certain extent, imperfect; but as physiology and pathology are inseparably related, and the facts and principles of the one are always illustrated by those of the other, the study of the abnormal forms of nutrition may assist in confirming or refuting particular views on those points of the healthy process on which there is still some difference of opinion. It is moreover of the utmost importance, in regard both to the healthy and abnormal forms of nutrition, that we be looking for further explanations in the proper direction. For this purpose it is useful frequently to review the nature and extent of our present actual knowledge; and the formation of theories, even in the absence of sufficiently conclusive data, assists in arriving at a knowledge of the truth, by stimulating the mind to the discovery of such facts as may test their correctness, and thus relations may be recognised which might otherwise have remained unobserved.

Nutrition has for its object the attainment and preservation of such a constitution in a part as fits it for efficiently discharging the duties it is destined to perform in the animal economy. We at present restrict the application of the term to that process by which the integrity of the texture is preserved, notwithstanding the infinitude of changes which are constantly going on in its ultimate structure. We will not at present dwell minutely upon all that is known of its constituent phenomena, but will simply glance at those points which may afford assistance in forming correct conclusions in regard to the nature of inflammation.

For this purpose nutrition may conveniently be regarded as constituted by three series of phenomena. These are, in the first place, the changes in the ultimate structure of the tissue; secondly, the movement of blood in the capillaries; and lastly, the interchange of fluids between these vessels and the intervascular spaces. Something positively certain, or highly probable, is known respecting the conditions and healthy modifications of each of these sets of phenomena. Having specified some particulars regarding each of them, we must make it our object to discover in what manner they are affected individually, or in what respect their combination or succession differs, in the state of inflammation, from what occurs in the progress of healthy nutrition.

The changes presented in the ultimate structure of the texture are all more or less immediately connected with the growth, transformation, decay, and reproduction of cells. At any particular moment these are to be found in all stages of development. For the evolution of that progressive series of changes which constitutes their life as individuals, the principal condition is the presence of a fluid capable of furnishing them with the requisite materials of growth, and this is fulfilled by the blastema in which they are bathed, and which has been derived from the capillary bloodvessels. On this fluid important changes are effected by any progress in the development of the cells. Some of its elements are removed by those in a state of growth; while other cells, having arrived at the term of their existence, break up and restore to it, in a different form of combination, the elements they had previously separated. Circumstances are thus occasioned unfavourable to the growth of new germs. These, with a constitution similar to that which belonged to the parent cells, will be unable to separate the requisite materials of their growth, unless they are to grow differently, and have a different destination.

Looking at the texture as a whole, an important change is soon effected on its constitution by the simple evolution of these changes. As the former constitution, however, was the essential source of its functional power, some means must be provided by which every change may be remedied, and the integrity of the texture preserved. The worn-out particles must be replaced by others capable of being affected by the same influences, and of producing similar results. What is immediately required for attaining this end, is the removal of those products which result from decay, and the supply of fresh nutritive material.

The removal of effete matters, and the introduction of the necessary plastic elements, are made mutually subservient to one another. The attainment of these two objects is accomplished by one process. The fluids containing the respective elements being only separated from one another by the thin wall of the capillaries, endosmosis and exosmosis must immediately take place. The albuminous fluid within the capillaries will thus pass outwards into the intervascular spaces, while the fluid in the latter, loaded with the products of decay, passes into the vessels. Simultaneous interchange takes place among the gaseous ingredients, so that oxygen and carbonic acid mutually replace one another.

This interchange having been effected, the inherent tendencies of the growing cells are permitted to develop themselves. The new supply of plastic material is at once the stimulus and condition of growth, and the least progress in this again immediately involves the occurrence of the former change in the blastema. A necessity hence arises for also constantly renewing the fluid in the capillaries, and this is accomplished by the unceasing current of the circulation.

The principal circumstance in connection with the capillary circulation which we would at present call to mind is its intimate relation to, and dependence upon, the vital operations in the ultimate structures of the texture. The rapidity of its current, or the quantity of blood made to flow through the vessels of any part, is not altogether dependent upon the rate or vigour of the heart's action. The occurrence of certain changes in the parenchyma, and of a mutual action between the fluids within and external to the vessels, is a most necessary condition to the free transmission of blood through them. When nutrition is depressed, or its operations cease to be evolved, the current of the circulation is also interrupted, or it becomes completely stagnant. It is also well known, that when the nutritive changes are being carried on, a force is exerted at the capillaries by which the movement of blood is occasioned independently of any force acting on it from behind.

The final cause of this circumstance is sufficiently obvious. The object of the circulation being to act as a stimulus in various vital processes, and to afford materials for the formation or regeneration of the textures, there must be some means of so regulating its supply, that while this shall be sufficient for the wants of the texture, it will not be so great as, by its bulk or otherwise, to interfere with the proper exercise of its function. While the general current of the circulation, therefore, is sustained by the action of the heart, its local distribution must be regulated by other influences. Those changes which are the occasion of the demand in particular organs must involve the means of modifying the supply. The more actively the operations of nutrition are being carried on, the greater is the necessity for a free supply of nutritive material being provided. Whatever causes the growth of the cellular elements to take place more rapidly, will have the effect of sooner exhausting the supply, or changing the constitution, of the surrounding blastema. The activity with which the function of a part is being exercised exerts considerable influence on the occurrence of this change. Certain of the organic particles of which its structure is ultimately composed, may be regarded as instruments by the co-operation of which its function is discharged. The use of these instruments involves their wasting, and consequent unfitness for longer performing the duties devolving upon the tissue. The greater the energy with which they are exercised, the shorter will be their term of existence, and the more rapid therefore will be the change effected on the constitution of the fluid in which they are bathed, and into which they are at last dissolved. In these circumstances increased facility must be afforded for providing means by which they may be replaced,—the materials of growth must be supplied in greater abundance, and the products of decay more rapidly removed.

The immediate agency by which this result is attained is very complex, and our knowledge of its operation obscure; but as the local vascular disturbance is one of the most remarkable phenomena characteristic of the earlier stage of inflammation, it would be of importance, in order to account for that disturbance, to form an approximate notion of the mode in which the operations of nutrition can at all affect the movement of blood in the capillaries.

As it is necessary for this purpose to have recourse, for the sake of illustration, to those physical phenomena which present the nearest analogy to the one under consideration, it must be remembered that no mechanical ingenuity can construct a combination of circumstances at all approaching in intricacy the ultimate structure of any of the textures; nor, assisted by the most intimate knowledge of chemistry, can produce a combination and succession of changes such as are there presented. The minuteness of the structures, and the great variety and delicate nature of the changes, as well as the inconceivable rapidity with which they are effected, render it impossible for the mind to follow minutely all the steps and combinations of the process, or to trace with precision what relation any particular effect bears to some preceding step or combination. Our knowledge must be very general, and even in giving expression to this, a lengthened narration is sometimes necessary to describe what, in its actual occurrence, would occupy an almost inconceivably short space of time.

In physical science, the phenomena of capillary attraction, and those of combustion, afford instances of a current of fluid being produced independently of the influence of gravitation, or any obvious mechanical arrangement. The conditions on which nutrition is carried on may be regarded as involving a combination of the causes which operate in these two instances. Among the phenomena of capillary attraction, the particular fact that one fluid having a strong attraction for the walls of a capillary tube, will drive before it another fluid having a weaker attraction,[1] must especially be kept in mind when we would attempt to explain those of the capillary circulation. In the latter case, however, the capillaries themselves may be regarded as almost perfectly neutral, and as taking no active share in causing the movement of the blood. The attraction of their walls may be regarded as equally strong for arterial or venous blood. They may therefore be left out of view, and the relation of their contents to the fluids and solids external to their walls alone attended to. We have already seen how the constitution of the blastema in the intervascular spaces is soon altered from the action of the growing and decaying particles. It would constantly tend, in consequence, to become more widely different in constitution from the fluids within the vessels, if these were completely separated from one another. Being only separated, however, by a thin membrane, the physical phenomenon of interchange must, according to certain laws, immediately take place. Each gives up to the other certain of its constituents, so that they again come more nearly to resemble one another in composition. This will continue so long as the vital operations go on in the intervascular spaces, and as far as endosmosis and exosmosis will permit. If the supply within the vessels were limited, the vital changes would soon become suspended, just as combustion will become extinguished if the supply of oxygen be exhausted. But as in the latter instance the physical changes occasioned by chemical union serve, in ordinary circumstances, to bring a fresh supply of oxygen within the sphere of action, so the operations of nutrition involve in their performance the means of renewing the supply of blood in the capillaries. In combustion, the expansive repelling force of the heat acts equally on the constituents of the air and the carbonic acid which results from chemical union. But while the latter is simply repelled, the atoms of the burning body in immediate contiguity with those in the act of chemical union, attract the oxygen with greater force than the heat repels it. A perpetual agitation is thus kept up in the surrounding medium, and if oxygen be obtained with greater facility from one quarter, a current will be occasioned by which the gaseous products of combustion will be carried away in the opposite direction. In like manner, in nutrition the growing particles may be represented as keeping up a constant agitation in the medium by which they are surrounded, which extends itself, so to speak, into the capillary vessels. Within these, however, it is only from one direction that a fluid can be obtained with which the phenomenon of interchange can be readily effected. If now we suppose this tendency to interchange to be equivalent to the simple force of attraction in rigid capillary tubes, we may obtain a tolerably precise notion as to the manner in which a constant current may be sustained in the vessels. So soon as interchange has been effected to the smallest extent at any point, the tendency to its further continuance must immediately become diminished, but in regard to the fluid at an insensible distance behind it, in the direction of the arteries, this tendency is increased. To use a convenient form of expression, but not strictly precise, we may say, that when interchange has taken place, the attraction of the fluid in the intervascular space for the blood within the capillary, becomes at that point weaker, while it is comparatively stronger for the blood at an insensible distance behind it. The latter must therefore drive the other onwards before it, and the same series of phenomena will continue to be repeated so long as conditions continue favourable. The vital operations constitute a constantly operating cause of change in the intervascular spaces, and the arterial blood presents a fluid capable of affording ready interchange, and of sustaining these vital operations. A constantly operating force is thus brought to bear upon the movement of the blood.

---

[1] Draper "On the Forces which produce the Organisation of Plants."

If the above representation be correct, we may conveniently look upon the *occurrence of interchange* as an immediate cause of the movement of blood in the capillaries. Whatever favours the occurrence of this phenomenon, will therefore facilitate the movement of blood, or increase the force of its current. If nutrition is languid, and the blastema is in consequence slowly affected, the mutual action between it and the blood will be less vigorous, and the circulation will be retarded. If, on the other hand, nutrition be excited, the conditions of interchange being more actively presented, it will take place with greater energy, and its influence on the circulation will be correspondingly increased—the blood will be more rapidly transmitted through the vessels.

Such, then, is an outline of the circle of phenomena by which the process of nutrition is constituted. Their order of succession and mutual dependence and reaction must be kept steadily in view when we would attempt to determine the nature of inflammation. As no theory of the latter process can be considered complete or satisfactory which does not involve an explanation of its most characteristic phenomena, we will begin by attempting to interpret some of these individually. It will be admitted that that theory is most likely to be correct which, while perfectly consistent with all established facts, serves in the best manner to account for the production of observed effects. If several of the latter may be traced to one modification of the ordinary process of nutrition, we may be warranted in regarding that modification to constitute the proximate cause of inflammation.

Of the various symptoms or effects occasioned by the presence of inflammation, we select two which are most invariably presented—1*st*, increased vascularity; and, 2*nd*, the increase locally, and in the general current of the circulation of fibrine. If the occurrence of these be explained in a satisfactory manner, the others—as heat, pain, swelling, &c. —may be easily accounted for.

When a part which has become the seat of inflammatory action can be made the subject of observation, the greatly increased and permanent redness, in consequence of a larger amount of blood in its vessels, is one of the most prominent physical alterations presented. The blood is moreover transmitted through its vessels with increased velocity. It is true that when the inflammation has arrived at a certain height, the current begins to move more slowly, and at last complete stagnation of the circulation takes place; and this we will afterwards attempt to explain. At first, however, the circulation is invariably excited in all the vessels of the inflamed texture; the amount of blood transmitted through them in a given time is increased. On the proof of this we need not dwell, as we believe these statements will not be called in question. "The whole quantity of blood," says Dr Alison, "returned by the veins from an inflamed part (as in the case of the hand), is found to be greater, and, when the inflammation is severe, to be three or four times greater, than that returned within the same time by the veins of the opposite sound organ." Moreover, "in parts which have no blood-vessels of their own, and yet which manifest certain of the signs or effects of inflammation, these generally, if not always, involve (as Mr Paget remarks) enlargement of the adjacent vessels, and especially of those from which the diseased structure derives its natural supply of nutritive material. Thus, in inflammation of the cornea, the vessels of the sclerotica and conjunctiva are enlarged; and in ulceration of the articular cartilages the same enlargement is seen in the vessels of the surrounding synovial membrane or subjacent bone."[1]

Here, then, is a phenomenon, obvious and constant, and standing in the relation of effect to the morbid action whose nature we would endeavour to discover. When inflammation is at all acute in any texture, the blood is attracted more powerfully towards it, and is transmitted through the vessels more rapidly than in health. When we remember the intimate relation of the capillary circulation to the vital changes in the ultimate structures of a texture, and that excitement of the circulation is one of the earliest obvious results occasioned by inflammation, it seems probable that to account for its occurrence would at once determine the earliest step of the process,—the first deviation from the healthy order of succession in the phenomena of nutrition.

In having recourse, for the purpose of illustration, to those instances of alteration in the local distribution of blood which take place as a condition of health, we must confine our attention to those instances where the cause is also local,—where changes occurring in the parenchyma of the part itself so influence the current of blood in the capillaries as to accelerate its movement. Now, in all instances where this is observed, it is associated with activity in the ultimate structures of the texture affected. This activity may, of course, manifest itself in many various ways. For example, if it be gradual and simultaneous in all the structures which enter into the constitution of the texture or organ, increase of bulk or hypertrophy will be occasioned. If the organ be one of secretion, the amount of matters separated by it from the blood will be increased. The question therefore immediately suggests itself—Are the phenomena of inflammation the evidence of a peculiar or unusual excitement in the operations of nutrition? We think they are, and it will become our object to determine more precisely the mode in which this takes place.

As we have just seen, one of the modes in which excitement of the nutritive process (strictly so called) can manifest itself, is in hypertrophy. But what will be the result if the excitement be more confined to the cellular elements, and the destructive as well as constructive operations are carried on with greater vigour than usual? Here, we think, the excitement will manifest itself in the phenomena of inflammation. But to explain more fully how this can take place, we will take a glance at the other characteristic phenomenon we have mentioned—the increased production of fibrine.

---

[1] Brit. & For. Medico-Chirurgical Review, Jan. 1851.

We may remark, that this relation of the vascular disturbance to a previous excitement in the vital operations has already been insisted on by various authors, and especially by Dr Alison. But in regard to the nature of the excitement—in tracing what results it directly tends to develop, and in interpreting the other characteristic phenomena of inflammation—we believe our views will widely differ from those of other writers.

In blood of a healthy constitution, fibrine is present in the proportion of about 2 or 3 parts in the 1000; but when an acute inflammation is well established in any part of the system, the proportion rises to between 6 and 8 in the 1000, or even higher. It also accumulates sometimes to a considerable extent at the seat of inflammatory action. The intervascular spaces become filled, or the inflamed surface becomes furred over with it. There is, therefore, an absolute increase in the amount of fibrine in the system.

Before this increase can be accounted for, the nature of fibrine itself must be determined. How is it produced in the economy, or from what source is it derived? On this point, however, very opposing opinions are entertained by physiologists. While some regard it as one of the constituents of the blood, at the expense of which the solid textures are principally nourished; others suppose (and we think correctly) that "it is more connected with the decaying than with the formative stage of life, and is destined to be removed from the system by the organs of secretion."—(*Bennett.*) We cannot here enter into an examination of all the arguments which have been advanced in favour of or against either of these views. The opinion of Zimmerman, that it results from the disintegration of the tissues, has in this country been advocated by Dr Bennett and Mr Simon; while Dr Carpenter, as is well known, strongly insists on the correctness of the other view. The latter eminent physiologist, however, seems to have very considerably modified his views as to the destination of fibrine. He would now "limit its *histogenetic* value to a certain class of tissues, namely the gelatinous." Now if albumen be sufficient for the nutrition of the muscular and nervous textures, it is difficult to see what necessity there can be for its becoming more highly vitalized in order to afford nutritive material for the gelatinous tissues. The very minuteness of the proportion in which fibrine exists in the blood appears strongly to militate against the correctness of such a view. It is also a remarkable fact, that in those individuals whose blood contains a larger proportion of fibrine than usual the textures are most poorly nourished.

We will only add one argument to those which we have seen advanced by other writers in favour of the view that fibrine is a product of disintegration. Both comparative physiology and pathological facts seem to point out that the proportion of fibrine and red corpuscles in the blood bear an inverse ratio to each other. When one of these principles is present in greater quantity than usual, we may infer with tolerable certainty that the proportion of the other is diminished. This may not be invariably true, but it occurs with sufficient frequency to indicate that an important relation exists between these two constituents. In individuals in very robust health, and in carnivorous animals, the proportion of the red corpuscles is greater than the average in the human subject, and that of the fibrine is diminished; while in weak, scrofulous, chlorotic individuals, in leucocythemia, and in herbivorous animals, the proportion of the corpuscles is diminished and that of the fibrine increased. Now, on the supposition that fibrine is a product of decay, we think it may be shewn how its amount is in part related to that of the corpuscles as effect to cause. It is a substance destined to be excreted from the blood; but before this can take place its chemical constitution must be altered, especially by its combination with oxygen. As the red corpuscles, therefore, are the "oxygen carriers," the greater their proportion is in the blood the more readily will the fibrine be oxidized, and so made to assume a form in which it can with the greatest facility be removed from the system.

Assuming, then, that the principal source of fibrine is the disintegration of the textures, we may proceed to discuss the question how its increase as the result of inflammation is to be accounted for.

In regard to its accumulation at the seat of inflammatory action, it has, so far as we are aware, been universally taken for granted that it has been exuded from the capillary vessels. All writers on the subject, however much they may differ in interpreting the other phenomena of the process, seem to agree in regarding this as a matter of fact. According to Dr Bennett, " exudation is the essential phenomenon, the *sine qua non* of inflammation." Now, if exudation be really a *fact*, it will, of course, with the usual stubbornness of its genus, remain unaffected by any arguments of ours to prove the contrary; and it may seem not a little presumptuous to question what is so generally admitted. Still we cannot help thinking that this explanation of its local accumulation is erroneous. Various objections may be urged against it, and some arguments may be advanced in favour of the idea that it is directly elaborated by the structures external to the vessels, and that its increase in the general current of the circulation is secondary to the occurrence of its local increase.

If liquor sanguinis were exuded in inflammation, we should expect the proportion of fibrine in the blood to become diminished instead of being increased, as it actually is, to at least twice or three times its usual amount. Soon after the local signs or general symptoms have presented themselves, the proportion of fibrine in the blood is increased, and gradually becomes greater in proportion to the intensity of the process or the amount of tissue involved in it. If we rightly understand Professor Bennett, however, he regards this increase to take place only after inflammatory action has subsided. He says, "during the disintegration of simple cancerous and tubercular exudations, the animal matter broken down is again rendered fluid, repasses into the blood, and then constitutes that excess of fibrine detected by chemists."[1]

Now this view is surely inconsistent with fact. Other writers have always supposed that the increase in the general current of the circulation is almost simultaneous with the progress of inflammation, and we are not aware that any proofs have been advanced to shew this opinion to be erroneous. If the buffed and cupped clot be any indication of the presence of an increased amount of fibrine, then it is certainly presented when inflammation is acutely going on. "The augmentation of fibrine," says Dr Watson, "begins with the inflammatory process, increases with its increasing intensity, and diminishes as it abates. That it depends upon the inflammation and not upon the general febrile condition, is apparent from the interesting fact, that in idiopathic fever the proportion of fibrine in the blood is diminished."

Moreover, the rational explanation of the fact of its local accumulation is not in the least promoted by assuming the occurrence of exudation. The phenomenon would simply have to remain an ultimate fact; for it is impossible to form any conception of an efficient cause for its occurrence; or rather, our present knowledge is opposed to its probability. If in the healthy evolution of the nutritive process fibrine passes out of the intervascular spaces into the capillary vessels, in accordance with the laws of endosmosis, then neither simple excitement nor depression of the vital operations will be able to cause it to pass in the opposite direction. Neither can we conceive that any alteration in the physical condition of the capillaries can have any material influence on its occurrence. Vital attraction cannot be the cause, for fibrine is an effete matter. Or, if we are to adopt the views of Dr Carpenter and Mr Paget on the nature of fibrine and on the nature of inflammation,—that the former is "the most highly organized material of the blood," and that in the latter, "so far as the proper substance of the inflamed part is concerned, there appears to be decreased action, that is, decreased formation," —it must be explained how a larger proportion of the fibrine than usual can pass into the intervascular spaces, when there is a less degree of attraction for it than in health.

---

[1] Monthly Journal of Medical Science, Feb. 1850.

Even assuming the occurrence of exudation as a fact, what does it explain? It does not in the least assist us in accounting for any of the other phenomena characteristic of inflammation. It cannot be the proximate cause of any of the observed effects, except swelling. Neither the vascular excitement, the heat, nor the tenderness manifested or experienced at the part, can be referred to it.

As there is an increase in the absolute amount of fibrine in the system, we must look for the cause of that increase to the source from which fibrine is in the first instance derived. We may naturally expect some excitement to exist in the process by which it is produced.

Its principal primary source we have already considered to be the dissolution or disintegration of cells or fibres. Of the various elements into which these are resolved in their decay, fibrine is one; and it is therefore being constantly elaborated in all the textures. Let us for a moment suppose that its production is the final cause of nutrition,—that it is a secretion which, instead of being carried away by a channel of its own, must be received into the blood; if we were wishing to cause a circumscribed portion of any structure to produce fibrine in greater quantity than usual, we would simply require to adopt such means as would cause the changes of growth and decay to go on faster than usual. The instruments by which it is elaborated being thus, as it were, more vigorously exercised, they must be more rapidly worn out and reproduced. This will involve,—as in all other instances where an organ of secretion is stimulated,—not only as a condition for the continuance of the excitement, but, in the manner we have already attempted to explain, as a direct consequence,—the supply of a larger amount of nutritive material. The conditions of interchange being more rapidly presented, the mutual action between the blood and the tissues will take place with greater vigour; and, in consequence, the blood will be more strongly attracted to the part, and transmitted through the vessels with increased rapidity.

Now, we are inclined to think that this is actually what takes place in the first stage of inflammation; and we thus see how its two most characteristic phenomena are the result of one action. To account for the increase of fibrine, we have arrived at exactly the same modification of nutrition which we assumed to be the immediate cause of the vascular excitement. If the manner in which we have attempted to trace these causes individually be a legitimate one, the coincidence affords strong presumption in favour of the correctness of our conclusions.

We need not here do more than allude to the satisfactory manner in which the increased heat and tenderness of the part may also be explained by the above view. If the vital operations are being carried on with greater rapidity, chemical changes must also be taking place with increased activity, and a greater amount of heat must in consequence be evolved. If the nutrition of the nerves be involved in the excitement, this will, with the consequent augmented vascularity, exalt their function, and render them more susceptible of impressions.

"Dilatation of the vessels" has been regarded as one of the most constant phenomena of inflammation. It seems to have been studied, however, more in relation to the effects it involves, than in regard to its immediate cause. The influence of greater or less *friction*, in consequence of diminished or increased calibre of the vessels, in affecting the current of blood, has been fully discussed.[1] But it must be remembered that it is the mutual action between the blood and the tissues which regulates the amount of nutritive fluid transmitted through a part; and therefore the changes in the physical condition of the vessels must be the effect, rather than the cause, of any alteration in the movement of blood. If the forces affecting that movement are acting with greater vigour,—if the current of the circulation be more rapid than conception of an efficient cause for its occurrence; or rather, our present knowledge is opposed to its probability. If in the healthy evolution of the nutritive process fibrine passes out of the intervascular spaces into the capillary vessels, in accordance with the laws of endosmosis, then neither simple excitement nor depression of the vital operations will be able to cause it to pass in the opposite direction. Neither can we conceive that any alteration in the physical condition of the capillaries can have any material influence on its occurrence. Vital attraction cannot be the cause, for fibrine is an effete matter. Or, if we are to adopt the views of Dr Carpenter and Mr Paget on the nature of fibrine and on the nature of inflammation,—that the former is "the most highly organized material of the blood," and that in the latter, "so far as the proper substance of the inflamed part is concerned, there appears to be decreased action, that is, decreased formation,"—it must be explained how a larger proportion of the fibrine than usual can pass into the intervascular spaces, when there is a less degree of attraction for it than in health.

---

[1] See Jones "On the State of the Blood and the Bloodvessels in Inflammation," and Brit. and For. Med.-Chirurg. Rev., Jan. 1850.

Even assuming the occurrence of exudation as a fact, what does it explain? It does not in the least assist us in accounting for any of the other phenomena characteristic of inflammation. It cannot be the proximate cause of any of the observed effects, except swelling. Neither the vascular excitement, the heat, nor the tenderness manifested or experienced at the part, can be referred to it.

As there is an increase in the absolute amount of fibrine in the system, we must look for the cause of that increase to the source from which fibrine is in the first instance derived. We may naturally expect some excitement to exist in the process by which it is produced.

Its principal primary source we have already considered to be the dissolution or disintegration of cells or fibres. Of the various elements into which these are resolved in their decay, fibrine is one; and it is therefore being constantly elaborated in all the textures. Let us for a moment suppose that its production is the final cause of nutrition,—that it is a secretion which, instead of being carried away by a channel of its own, must be received into the blood; if we were wishing to cause a circumscribed portion of any structure to produce fibrine in greater quantity than usual, we would simply require to adopt such means as would cause the changes of growth and decay to go on faster than usual. The instruments by which it is elaborated being thus, as it were, more vigorously exercised, they must be more rapidly worn out and reproduced. This will involve,—as in all other instances where an organ of secretion is stimulated,—not only as a condition for the continuance of the excitement, but, in the manner we have already attempted to explain, as a direct consequence,—the supply of a larger amount of nutritive material. The conditions of interchange being more rapidly presented, the mutual action between the blood and the tissues will take place with greater vigour; and, in consequence, the blood will be more strongly attracted to the part, and transmitted through the vessels with increased rapidity.

Now, we are inclined to think that this is actually what takes place in the first stage of inflammation; and we thus see how its two most characteristic phenomena are the result of one action. To account for the increase of fibrine, we have arrived at exactly the same modification of nutrition which we assumed to be the immediate cause of the vascular excitement. If the manner in which we have attempted to trace these causes individually be a legitimate one, the coincidence affords strong presumption in favour of the correctness of our conclusions.

We need not here do more than allude to the satisfactory manner in which the increased heat and tenderness of the part may also be explained by the above view. If the vital operations are being carried on with greater rapidity, chemical changes must also be taking place with increased activity, and a greater amount of heat must in consequence be evolved. If the nutrition of the nerves be involved in the excitement, this will, with the consequent augmented vascularity, exalt their function, and render them more susceptible of impressions.

"Dilatation of the vessels" has been regarded as one of the most constant phenomena of inflammation. It seems to have been studied, however, more in relation to the effects it involves, than in regard to its immediate cause. The influence of greater or less *friction*, in consequence of diminished or increased calibre of the vessels, in affecting the current of blood, has been fully discussed.[1] But it must be remembered that it is the mutual action between the blood and the tissues which regulates the amount of nutritive fluid transmitted through a part; and therefore the changes in the physical condition of the vessels must be the effect, rather than the cause, of any alteration in the movement of blood. If the forces affecting that movement are acting with greater vigour,—if the current of the circulation be more rapid than natural,—then increased *pressure* must be exerted on the vessels, and this, of necessity, must tend to enlarge their calibre. A natural explanation of the occurrence of dilatation in the capillaries of, and arteries leading to, inflamed parts, is thus afforded on the supposition that excitement of nutrition exists in the latter.

---

[1] See Jones "On the State of the Blood and the Bloodvessels in Inflammation," and Brit. and For. Med.-Chirurg. Rev., Jan. 1850.

The preternatural excitement in the operations of nutrition cannot long continue without involving most important consequences. The general rule, that the greater the rapidity with which any texture is formed, it is the more liable to degenerate, soon becomes well illustrated. The evidences of vitality soon become less marked, and at length, if the inflammation be severe, nutrition becomes entirely suspended. The current of the circulation first becomes languid, then oscillates, and at last becomes completely stagnant. These evidences of decay and death are, however, the natural and inevitable result of the excitement which characterized the early stage.

To account for the stagnation of the circulation, various theories have been entertained, and much discussion occasioned. No efficient physical impediment can be detected. There is no obstruction in the vessels themselves, and yet the cause must be a local one. We must here again consider the influence of the structures external to the capillaries. As the increased vascularity of the early stage of inflammation was referred to an excitement in the operations of nutrition, so stagnation of the circulation,—so invariably presented during its later stage,—may result from these being in the first instance suspended. The current will thus be interrupted, not so much from the operation of any active cause, as from the mere absence of the force which occasioned the movement of the blood during the healthy state of nutrition.

It is difficult to give a very minute detail of the various steps by which this ultimate result is occasioned; but the following statement, which we give as a summary of our views on the nature of inflammation, may exhibit their possible order of sequence.

The primary effect of such a stimulus as excites inflammation is to disturb the balance of nutrition in the part affected. *The whole series of operations by which the simple integrity of the texture is preserved is evolved with greater rapidity than natural.* Growth, dissolution, and reproduction all take place with greater vigour than in health, or in comparison with their occurrence in the texture around the seat of inflammation. The attractive force for nutritive materials is rendered stronger than natural, and these are effused from the vessels in greater abundance. The constitution of the blastema is rapidly altered, and the interchange or mutual action between that fluid and the blood is carried on with great activity. The force by which the latter fluid is attracted to and transmitted through the vessels is thus acting more powerfully than usual. The current of the blood being more rapid, its pressure against the walls of the vessels must be greater, and thus dilatation of the capillaries, and of the arteries by which they are fed, is occasioned. Various circumstances, however, soon interfere with the occurrence of interchange, and the circulation in consequence begins to flag. Fibrine is one of the substances which are being produced in greater quantity by the excitement in the vital operations. In order that it may be able to enter the bloodvessels, it must be kept dissolved by a sufficient quantity of watery fluid. The effused serum, after it has given up its albumen to the growing cells, assisted by the more inorganic products of decay, supplies this desideratum in healthy nutrition. But *in inflammation the fibrine is elaborated with greater rapidity than it is being admitted into the vessels.* Its amount thus increases in the intervascular spaces, and in consequence it must soon interfere with the nutritive process—the vital operations will be depressed; the more inorganic compounds will pass more readily into the vessels than fibrine, and thus the exosmotic passage from these of serum will still be sustained. From this, in consequence of the momentum which nutrition has received, the germs and cellular elements will still strive to be developed, and thus the proportion of fibrine will continue to increase in the interstices of the tissue. As interchange becomes less vigorous, the current of the circulation will be transmitted more slowly. The globular constituents of the blood will accumulate in the capillaries, partly because the mutual action between these and the textures is less energetic, and partly because the fluid portion of the blood will be more readily affected by the action of the heart. When the vital operations are completely suspended, the blood will, as a necessary consequence, become stagnant in the capillaries. An attraction of simple cohesion, rather than a tendency to interchange, will be exerted between the fluids on either side of their walls.

One of the principal points, but a very important one, in which the above theory differs from those advanced by other writers on the subject, is in regarding the fibrine which accumulates at the seat of inflammatory action to be the result of direct elaboration, instead of exudation. If this view be correct, our notions on the nature of the healing process, and on the manner in which hemorrhage is checked, must be very considerably modified. We think, however, that the manner in which it may be applied in these instances affords corroborative evidence of its correctness.

When a solution of continuity has occurred in any texture, the circumstances in which nutrition takes place are greatly altered. They are brought more nearly to resemble those in which secretion is carried on. The conditions for effete matters being received into the vessels, are much less favourable than those presented in the ordinary circumstances of nutrition. The molecular elements will, from their own inherent endowments, and from the stimulus given by the mechanical violence, still strive to be developed, and this will occasion effusion of the serous part of the blood. But whether the raw surface be superficial, or two cut surfaces are approximated to one another, the products which result from the evolution of the nutritive changes cannot be received into the circulation with the facility with which this is effected in the healthy condition of the tissues. The more inorganic compounds will, however, enter the vessels, or be carried away in the discharge more easily than the fibrine, and thus the latter, accumulating at the surface, will afford it protection, or form a bond of union between two parts; it will prevent hemorrhage, limit effusion, and permit nutrition beneath it to be carried on as usual.

If from the previously depraved character of nutrition, or from the nature or intensity of the stimulus, the nutritive operations are evolved in a manner different from natural, the formation of pus may be the consequence. But if nutrition has been healthy, and conditions are favourable, the growing molecules will be developed in much the usual manner, and thus union with the first intention may take place without any of the other phenomena of inflammation manifesting themselves. In the latter instance, the nutritive operations at the cut surface may be regarded as naturally tending to their own extinction. In the treatment of ulceration, it is our object to produce this effect, and the means employed must be adapted to the nature of the ulcer. While the healthy sore simply requires protection against the operation of injurious influences, the indolent ulcer requires the application of such substances as have an irritating influence, that is, which tend to the production of inflammation, and consequently to the formation of an increased amount of fibrine, or fibrinous structure. We need only allude, in confirmation of this view, to the remarkable effect of a blister in changing the action of the worst forms of indolent ulcer.

In regard to the pathology of hemorrhage, if extravasation of blood be checked by the coagulation of fibrine as it is escaping from the ruptured vessels, we should expect that in those instances when the proportion of that constituent of the blood is above the average, hemorrhage would be checked with the greatest facility. So far is this from being the case, however, that in some states of the system where this condition is present, the strong tendency to hemorrhage is one of the most remarkable characteristics attending it. In some cases of scurvy the proportion of fibrine in the blood, if we may judge from the "buffed and cupped clot" which is formed, is increased. But in leucocythemia this connection between the hemorrhagic diathesis and such a condition of the blood is most marked. In this affection the proportion of fibrine is almost invariably much above the average, and in twenty-five undoubted cases of it published by Professor Bennett "extravasation of blood occurred in fourteen." In one case where it is recorded that there was "no epistaxis at any time, no hemoptysis, a little hematemesis, no melæna, hematuria, nor menorrhagia," some leeches having been applied to remove a local pain, "the bleeding was profuse and not very easily arrested." Some other influence must therefore be in operation besides the mere coagulation of the fibrine of the blood itself, and this may be found in the vigour with which nutrition is being performed. If this process be languid, little hindrance will be afforded to the occurrence of extravasation; but if its operations be carried on with vigour, the elaboration of fibrine at the part will speedily be effected, and the mouths of the vessels be thereby closed.

We will not in the present essay give any account of the mode in which development or degeneration of the products of inflammation may take place. Neither will we enter into any examination of the constitutional symptoms, nor attempt to trace their relation to the local changes. It is sufficient for our purpose, in the meantime, if the theory we have advanced be not inconsistent with facts established in regard to these. We will therefore conclude for the present with a few remarks on the principles which must guide us in the treatment of inflammation.

A theory on any morbid process is important according as it affords a rational explanation of the mode in which remedial agents operate in checking its progress and removing its effects. We are inclined to think that the views which we have attempted to establish on the nature of inflammation will, to say the least, admit of as wide application in this repect as any of the more prevalent theories on the subject.

A slight review of the nature and relations of its various phenomena will serve to shew how every means which prevents the free determination of blood to the seat of inflammation, will tend to lessen its intensity and favour resolution. The local vascular excitement is, as we have already attempted to shew, both the consequence and condition of the process. The supply of blood is simply in proportion to the demand for it; therefore, a diminution in the amount of the former must inevitably interfere with the operations which occasion the latter. The development of the particles cannot be carried on with the same degree of vigour if the nutritive materials are less readily supplied, and thus an opportunity is afforded for the balance of nutrition being restored. If the excitement still tends to continue for a short time in the ultimate structures, the conditions of free and immediate interchange not being present, the products of decay will accumulate in the interstices of the tissue, and the vital operations being thus interfered with, the natural termination of the process is more speedily effected.

Although in their action on inflammation, some substances, such as opium or acetate of lead, may possibly have a directly sedative influence on the operations of nutrition, the majority of the means employed to arrest its progress seem rather to act by moderating the supply of blood to the parts affected. The influence of bloodletting must in this way be frequently very powerful in "disposing to a favourable termination," or even in "cutting short" the morbid action. The same end, however, may often be obtained by any means which simply cause a derivation of nutritive fluid from the inflamed textures. For example, tartar emetic, by acting on the skin and mucous membranes of the body, and causing in consequence a determination of blood towards these, will affect the general distribution of that fluid, and permit a less amount of it to be sent to the seat of inflammation. In like manner such means as act on particular organs of secretion, mercury, purging, blistering, &c., will often be useful. Without dwelling on these instances, however, we would rather attempt an explanation of the tonic plan of treatment.

"There are none," says Dr Alison, "who resist the exciting causes of inflammation so well as those in whom the blood is abundant, and the vascular system vigorous. But the tendency to inflammation is remarkably given by all permanent causes of debility; by imperfect nourishment, impure air, long-continued heat or cold, excessive evacuations, mental depression, excessive exertions, and intemperance." Now, in these instances we think the influence exerted does not so much increase the tendency of individual textures to inflame, as by diminishing the power of nutrition to preserve the equilibrium of the circulation, and therefore favouring the more free determination of blood to the seat of inflammatory action.

A constant antagonism is exerted between all parts of the body in their relation to the circulation. If A and B are supplied with nutritive material from one vessel or system of vessels, the amount which A will receive will not altogether depend upon the degree of its own attraction for it, but also on the intensity of the force operating at B, and on the facility, therefore, with which the latter will permit itself to be deprived of a portion of its supply. It is therefore not simply the amount of stimulus applied to an organ or texture which determines the amount of blood it will receive, and consequently the effect which will be produced on nutrition, but also on the degree of force required to derive it from other parts of the system. The readiness with which the latter can be deprived of their natural proportion of blood will have an important influence in aggravating or repressing the tendency to local excitement,—in favouring or counteracting the influence of the stimulus. If the operations of nutrition in the system generally be vigorous, the textures will with difficulty be deprived of their usual supply of nutritive fluid; but if nutrition be languid, and the attraction for blood in consequence feeble, the latter fluid will, with comparative facility, be determined in greater quantity towards any point at which operations causing a stronger attraction for it have been excited. This must be at least one of the reasons why in a debilitated constitution inflammation takes place with greater certainty than usual, on exposure to its exciting causes.

The object of treatment, then, must be to restore at once the balance of the circulation and that of nutrition. It comes to be a question, therefore, whether this is to be attempted by directly moderating the activity at the seat of inflammation, or by exalting nutrition in other parts of the body. It is obvious that bloodletting cannot often be trusted to for restoring the balance of nutrition. The loss of blood will have a depressing influence on this process in the system generally, as well as at the seat of morbid action. It may happen that at the latter part the operations are still *relatively* more active than in other parts, and thus inflammation may go on unchecked, or may even be aggravated.

In the majority of instances our object will be more efficiently accomplished by stimulating nutrition in other portions of the system. For this purpose it may be useful, as we have just seen, to act on particular parts; but if the same end can be attained by affecting nutrition generally—by rendering its operations more vigorous, and so increasing the attraction for nutritive fluid, then, not only will inflammatory action be depressed by the parts affected receiving a diminished supply of blood, but an important step will be taken in securing more speedy convalescence. In all cases where it is suitable, therefore, the latter course is certainly to be preferred. In erysipelas, pneumonia, or in almost any inflammation whatever, when the nutrition of the system has been depraved, the administration of tonics, as the preparations of quinine or iron, must in this way have frequently a most beneficial influence.

We think there exists considerable analogy between the *modus operandi* of tonics in the treatment of inflammation, and of quinine in the cure of intermittent fever. In the latter disease, the diagnostic phenomena are associated with, if not occasioned by, general disturbance in the balance of the circulation, and this, again, is connected with a weakened state of nutrition in the system. At the commencement of a fit of ague, a less amount of blood than usual is determined to the surface and extremities of the body, while congestion takes place in the internal viscera. Now, as there is no obvious physical or physiological cause for the occurrence of this phenomenon, and we are consequently ignorant of any action to subdue, or physical influence to counteract, the rational mode of treatment must be to adopt such means as will tend to preserve the equilibrium of the circulation. If, by the introduction into the blood of any substances having an irritating effect on nutrition, we cause that process to be carried on more vigorously, then, on the one hand, the attraction for nutritive fluid is rendered stronger, and the textures are deprived of their supply of blood with less facility; and, on the other, the tendency to passive congestion is counteracted. If in this way the simple equilibrium of the circulation be preserved, the characteristic phenomena of the fit of ague will be prevented from manifesting themselves. And such we believe to be the *modus operandi* of quinine and arsenic in its cure. They act on nutrition generally, and cause its operations to take place with increased vigour. They exert, we would suppose, an irritating, rather than a stimulating influence; that is, their influence is, in the first instance, directed on the ultimate structures of the texture, rather than directly on the circulation itself.

We need hardly remark, that if the exudation theory, when applied to the phenomena of inflammation, be inefficient and unsatisfactory, so neither can it afford an explanation of the formation of tubercle. This pathological product we regard to result from a combination of circumstances almost diametrically opposed to those which occasion the phenomena of inflammation. It is also a product of abnormal nutrition, but the process, instead of being excited, is, in comparison with that of the texture around where it is forming, materially depressed. The balance of nutrition is still disturbed, but here it is because that of the diseased tissue fails to come up to the healthy standard. From the deficient nutritive or stimulating qualities of the effused blastema, the operations fail to be fully evolved. Into this subject, however, it is not our intention to enter at present.

ART. VI.—*Case of Death occurring forty hours after the administration of Chloroform; with Remarks on the Causes of Death from the Use of Anæsthetic Agents.* By KELBURNE KING, M.D.

On the 24th August 1853, Mr J. T., solicitor, aged forty-nine, called on me for advice under the following circumstances.

He said that for some time he had had a feeling as of a severe cold in the head, obstructing his breathing, rendering his voice thick, and his sense of smell deficient; that for the last few weeks these symptoms had increased in severity so much as to prevent him from attending properly to business. He had a continual sensation as of something plugging up the centre of his head, which he might dislodge by a forcible effort of expiration, but which rendered him "stupid," and incapable of long directing his attention to any one subject. This gentleman held the office of coroner of the burgh of Hull, and he complained that the obstruction in his nose interfered much with the duties of this office, rendering his speech thick, so as sometimes to be scarcely understood by the jury. On my asking whether he supposed that all these symptoms arose simply from the cold in his head, he remarked that he felt precisely as one might do whose nose was continually held forcibly closed, only that his obstruction was in the posterior nostrils, about the middle of his head. I asked him to close one nostril and breathe through the other, when I found that no expiratory effort could force any air through the right, and only a very little through the left. I then examined minutely the throat and the nostrils. In the throat I saw nothing worthy of remark, but on looking up the nostrils, I found that though the nose was externally large and prominent, the apertures of the nostrils were uncommonly small, and were closed up by small polypous growths, hardly discernible on the left side, but distinctly visible on the right. On explaining this to him, he said he distinctly remembered, some years previously, that, after a violent fit of sneezing, a body such as I described came away. His office of coroner often bringing him into connection with medical subjects, he took some interest in diseases and their treatment, and evinced considerable curiosity as to the nature of polypi, and the means used for their cure. Having had a little information on these points, he readily consented to permit me to extract them; and having sat down for the purpose, I readily seized and brought away the most projecting tumour on the right side. He then insisted that I should give him chloroform before proceeding further; and on my refusing, from want of assistants, he positively declined to have anything more done on that day, saying that he would rather fix a future period, when I might administer chloroform and do what was requisite. I remonstrated with him on this subject, assuring him that the administration of an anæsthetic agent was altogether unnecessary; and that although there might be nothing in his case to contraindicate its use, I did not consider the operation of sufficient severity to call for its administration. His mind, however, was made up, and having examined his heart and lungs, I agreed to give him chloroform and relieve him from the excrescences at some future day.

I may here mention that Mr J. T. was a large well-formed man, rather inclining to be corpulent. He had lived generously, much as an English gentleman in easy circumstances ordinarily does. He enjoyed good general health, and had, to all outward appearance, the prospect of many years of active and useful life. I had known him intimately for the space of two years, and had never observed any predisposition to disease, nor in fact heard of any complaint which he had ever laboured under, except occasionally a slight attack of gout; and once or twice before he had asked advice regarding this stuffing of his head, which, supposing to be simply the effect of cold, I had not considered of any importance. Although, on the whole, I would have preferred that he should have submitted to the operation without the use of chloroform, I saw no reason why his strongly expressed wish and determination should not be gratified.

Accordingly, on the 1st September, at eleven A.M., I called at his house with one of the students of my class, and administered chloroform. When he reached the point of insensibility to pain, he became very violent, so much so, that I had to give up the inhalation until I got another assistant. About fourteen minutes thus elapsed, during which he quite recovered from the effects of the chloroform, talked freely of his sensations, and remembered my trying his sensibility a few seconds previously to his becoming outrageous, of which, however, he had no recollection. Assistance having arrived, he was again subjected to the influence of chloroform, which was poured in small quantities on a handkerchief and held at a short distance from his face. In a few minutes, the same symptoms of violence were repeated, but were readily controlled, after which the state of anæsthesia shortly became complete. Up to this time, my finger had not been removed from his pulse, which beat regularly, and with its ordinary vigour, becoming slightly quickened at first, and continuing so throughout. His respiration all along was deep and regular, and seemed neither quickened nor the opposite. On account of his violence it was absolutely necessary to push the effects of the chloroform to its most advanced stage of complete insensibility. I then seized the polypi and cleared the right nostril as rapidly as I could. There was a good deal of bleeding, which retarded me; and it was necessary to administer a little more chloroform during the operation. After the right nostril was completely cleared, I passed the forceps into the left, but found that sensibility was returning; and as I ascertained by my watch that he had been kept eight minutes in a state of complete insensibility, I thought it better to rest contented with what had been done, and leave the extraction of the polypi from the left side to a subsequent period. Immediately after, he began to talk—at first a little at random, but in a very few minutes quite sensibly; he recounted his sensations, and asserted that he had all along been conscious of everything that was said and done, but had felt no pain. This statement—though he repeated it afterwards—was, I have no hesitation in saying, erroneous, as the state of insensibility was complete. The eight minutes during which it lasted were, I believe, completely blotted out from his memory; leaving the last act of perception or thought before and the first after the anæsthetic state in apparent proximity. In whatever way this impression may be caused, it is frequently noticed; and my patient confidently believed that, though deprived of the capacity of feeling pain, he retained during the whole time the use of his mental faculties. He breathed freely through the nostril which had been operated on, spoke with feelings of satisfaction of the result of the process to which he had been subjected, and only regretted that I had not taken the opportunity of his inconscious condition to clear both nostrils. The amount of chloroform administered in all was six drachms by weight. These details are precisely similar to many other cases of the administration of chloroform; and I have given them so minutely only because some little occurrences otherwise of no moment are rendered interesting by the sequel.

Late in the evening of the same day, about 10 P.M., happening to be in the neighbourhood, I called upon this gentleman, found that he had had a strange confused feeling for some time after I left him; that in accordance with my advice he tried to sleep, but unsuccessfully, and in a few hours the feeling wore off. He expressed himself as greatly relieved by having one side of his nose to blow through, and said that after a good night's rest he would be all right. One of the members of his family being at the time under my care, I sat and talked with him for about a quarter of an hour, during which time he again recounted the sensations he had experienced, and expressed his conviction that he knew what was going on all the time, though he experienced no pain.

He said he felt a slight inclination to shiver, and asked whether, as he had had no wine or stimulant all day, a little hot brandy and water would do him any harm; to which I replied in the negative, and saw him take a glass of brandy and hot water and retire to bed.

Next morning (2d September) I called at his house at 11 A.M., and was requested to see him, as he had been restless all night, moaned much in the morning, and had shewn no disposition to get up. I found his pulse quick, 100, and rather full; his head hot, face flushed, and temples throbbing; he seemed dozy and sleepy, was rather unwilling to answer questions, but had no pain either in the head or nose, or indeed in any part. He had passed water freely both the night before and that morning; seemed rather surprised at being asked so many questions, and said there was nothing the matter with him, except that he had not slept well during the night, and felt a little drowsy. I prescribed one drop of croton oil, with ten grains of calomel, to be taken immediately, and followed after three hours by a senna draught.

At 4 P.M. I again visited him, when I was informed that the bowels had been freely acted on, but that he had with great difficulty been got to bed afterwards, and was evidently unconscious of what was passing around him. On examination I found that he had indeed become almost completely comatose, it being impossible to arouse him except for a single instant, and then very imperfectly; his respiration was quick and laboured, his pupils sluggish, and his pulse had diminished alarmingly in strength. Blistering fluid was applied to the nape of the neck and to the head; calomel, turpentine enemata, mustard plasters to the calves of the legs, and every remedy which appeared likely to assist in rousing the system, were tried under my own superintendence and that of Dr Cooper of this town, to whom, for his kind assistance throughout the latter stages of this unfortunate case, I feel myself deeply indebted. Nothing produced the least effect; the coma became deeper, the snoring louder, and the pupils more and more insensible, until three o'clock in the morning of the 3d, when death finally supervened, forty hours after the administration of chloroform.

At 2 P.M. on the same day I made a *post-mortem* examination of the head in the presence of Dr Cooper. The features had a more livid appearance than usual—particularly the ears and back parts of the face. This may have been partly owing to the free application of vesicating agents.

On cutting through the scalp, there was a considerable oozing of dark fluid blood; and on sawing through the bones, the membranes being wounded in one or two places, a considerable quantity of bloody serum flowed out into a vessel placed beneath. The skull-cap having been removed, the dura mater presented no unusual appearance. On dividing it, we found that the surface of the greater part of the summit of both cerebral lobes was covered by a thin layer of pus adherent to the visceral layer of the arachnoid. On removing this with a sponge, the cerebral vessels were seen congested, but there was no redness of the arachnoid, nor did there seem to be any particular effusion in the subarachnoid cellular substance. On examining the base of the brain, a considerable quantity of bloody serum was found between the layers of the arachnoid. The cerebral vessels also were considerably congested; but there was no appearance of inflammation of the dura mater or parietal layer of the arachnoid. The state of the olfactory nerves, and of the cerebral substance and meninges in their neighbourhood, was minutely examined, but no trace of inflammatory action was discerned. The subarachnoid spaces anterior and posterior were filled, but not distended, by bloody serum; and the ventricles contained more than their normal quantity of the same fluid. In other respects the cerebral substance was firm and perfectly healthy.

The structure of the cerebellum, pons, and medulla oblongata was carefully examined, but nothing beyond a congested state of the larger vessels was found.

We did not think it necessary to examine the other cavities, as I had only a few days before ascertained that there was no organic disease, and death had evidently begun from the brain. I have since regretted that the condition of the kidneys was not inquired into; but I do not suppose that they had any connection with the result, as there was no suppression of urine, which indeed was passed involuntarily a few hours before death.

Such are the facts connected with this unfortunate case. What are the inferences? Was this a death resulting from, or in any way connected with, the administration of chloroform? Did the two events, closely connected in point of time, stand in the more intimate relation of cause and effect?

The subject of anæsthetic agents is one comparatively new to the profession; and although its rapid adoption and speedy admission as one of the recognised steps of every very important surgical operation, seem to disprove that slowness to leave the beaten track which has often been objected to against medical practitioners, it must be admitted that there still is a division of opinion as to the extent of their applicability, and the amount of danger which attends their use. In the opinion of some, whenever pain is to be inflicted, these agents may be employed, unless special circumstances, as diseased heart, &c., contraindicate their use. Others again would limit their application to the severest surgical operations, considering that a certain amount of danger always attends their administration; and that therefore they are inapplicable in all cases which are usually painful, but unattended with fatal results. It is admitted by all, that deaths have not unfrequently occurred after the use of anæsthetic agents; but according to some, the deaths actually caused by these agents are very rare, some of them being caused by accidental circumstances, and others by a neglect of those means which, if applied in time, would have obviated the fatal result. On inquiring into the mode in which death is produced, we find a remarkable discrepancy of opinion. In a very excellent paper in the September number of the *Edinburgh Monthly Journal* by Mr Bickersteth, several cases and experiments are adduced to prove that death takes place usually if not universally by asphyxia. He gives as the result of his observations, that "the respiratory movements cease before the cardiac;" and that, "if artificial respiration be resorted to before the cardiac contractions are seriously affected, and be properly maintained for a sufficient period, the respiratory functions may be re-established." He states it as his opinion that "the pulse should not be taken as any guide during the administration of chloroform. It should be wholly disregarded, except under certain circumstances." The attention should be mainly directed to the respiration; and, without denying the possibility of exceptions owing to peculiar idiosyncrasies, he considers that while the respiratory act is regularly performed, the state of anæsthesia may be produced without incurring any danger; but if the exhibition of the agent be persevered in while that state continues, syncope and asphyxia may occur simultaneously and cause sudden and irreparable death. But the fact of the pulse becoming less frequent is of no consequence—indeed is "so constant, that by some it is supposed, and correctly supposed, that anæsthesia is not sufficiently complete for the performance of surgical operations, unless the pulse be below its standard." Such at least is the general impression which his very admirable paper conveys to my mind; and if we can subscribe to his opinion, the induction of anæsthesia loses almost all its terrors, for death is preceded by a period of warning when by prompt and simple measures the fatal termination may be averted. The patient is in the condition of one seemingly drowned, but still capable of resuscitation. But unfortunately we find a different theory and different practice as decidedly and as ably recommended. In the *Lancet* of November 19, 1853, there is an abstract of a paper by M. Jobert De Lamballe, in which he says that "the administration of chloroform should cease immediately when the beats of the heart decrease in number and force;" and again, "the action of the heart should always be our guide;—it is the best means of ascertaining the saturation of the nervous system by chloroform, and of judging how far this system has been influenced by the anæsthetic agent." As to practice, M. Jobert "prefers acting principally on the nervous system, rather than lose time by exciting the functions of respiration and circulation."

Without going at any length into this matter, I believe that there is much greater probability of asphyxia than of syncope supervening from the use of chloroform; because the muscles of respiration (and consequently the respiratory act) are governed by nerves connected with the cerebro-spinal system, which is much more readily affected by this agent than are the sympathetic nerve and its branches, which govern the cardiac movements, and which, we have no reason to suppose are affected except secondarily. It is therefore natural to conclude that in most cases the failure of the respiratory act will take place before that of the heart; and that on its being noticed and the proper means adopted, time will be given to arrest the circulation of venous blood in the arteries, the continuance of which would necessarily prove fatal. At the same time, I fear we must consider it as proved that syncope may supervene without this premonitory notice. In a case recorded by Dr Dunsmure in the November number of the *Monthly Journal*, the breathing was not observed to fail before the pulse stopped. Those "who had an opportunity of observing the respiration, positively assert that the breathing did not cease before the pulse." It is stated that after "artificial respiration was had recourse to, in a few moments he made a long inspiration, which was followed by four others at gradually lengthening intervals," while "no pulsation could be felt in the radial arteries."

An anonymous correspondent of the *Medical Times* (October 15) concludes from these facts that this was a case of "mere syncope," and that therefore, as a rule, "the state of the pulse should be attended to, as betraying the first symptom of impending danger." But it must be remembered that in such cases every inspiratory act is necessarily observed, while we have no clear evidence of the last contraction of the heart: the mere fact of the pulse at the wrist being imperceptible is no evidence that the heart had not contracted; and I have some difficulty in believing that five acts of inspiration took place without a single contraction of the heart! I believe, in this instance, that a failure of the heart's action (syncope) and a paralysis of the respiratory muscles (leading to asphyxia), occurred at the same instant—that a partial reaction took place, when most probably the observed acts of inspiration were accompanied by unobserved contractions of the heart,—but that the efforts of nature were incapable of setting the machinery in motion, and that from the last natural inspiration the man's death is to be dated. The anonymous writer before referred to seems to think that this event is to be dated from the last perceived pulsation; but it would be monstrous to apply the term death to a man who was still capable of making natural respiratory efforts; and, if it were as capable of demonstration, I believe that in this case contractions of the heart would have been found taking place after the last act of inspiration. But I do not deny that deaths have occurred in which failure of the heart's action has been the prominent symptom, and that syncope has been attended by its usual symptoms of pallor of the countenance, &c. (In Dr Dunsmure's case the face and heart are said to have been a good deal congested.) Two cases are mentioned in the Dublin *Medical Press* (April 20, 1853), in which syncope having supervened, M. Nelaton restored suspended animation by inverting the body, after which the face recovered its colour, and the cases terminated favourably. And in a case of M. de Vallet, in the Hotel Dieu d'Orleans, the patient became suddenly pale, respiration ceased, and tracheotomy, artificial respiration, and galvanism were of no avail to prevent the fatal result. I conceive, then, that it is clearly established—1. That a continuance of the use of anæsthetics would in all cases, if sufficiently long employed, paralyse all the muscles supplied by the cerebro-spinal axis, including the muscles of respiration; and, secondarily, those dependent on the sympathetic system: that in all cases an overdose of chloroform may be expected to produce asphyxia, which, however, may, in most cases at least, be arrested, if observed before the cessation of the heart's action, by the methods usually employed. 2. That in some cases, altogether independently of organic disease, the heart's action may suddenly cease, even without an undue quantity of the drug having been employed; when, if the heart can be stimulated to contract, respiration (which cannot in the living body go on independently of the heart's action) may be restored and life preserved. The treatment here would be that usually adopted in syncope; for if the heart can be stimulated to contract, the respiratory muscles are capable of performing their functions; the patient is simply in the condition of one who has fainted. This class of cases must be rare, because anæsthetics (as before remarked) act on the cerebro-spinal much more readily than on the sympathetic system. 3. That in another class of cases death is produced by the simultaneous failure of the respiratory and cardiac movements; that these are the most dangerous of all; and that the treatment employed ought to combine the methods used in the first two cases.

At one time I thought that death always resulted from asphyxia; but the reported cases before alluded to, and the observations both of myself and others, convince me that we must be prepared to meet with other and even more formidable dangers. However much this is to be regretted, I think it must now be admitted, and ought to be generally known; for if the profession and the public are to have the full benefit of anæsthetic agents, it is necessary to look the dangers and difficulties attending them fairly in the face, when, by a proper discrimination of the causes likely to lead to a fatal result, we may be prepared *promptly* to apply those remedies more particularly suited to the case in question. We must be ready to lay down preconceived views and prejudices; and no man need be ashamed to change his opinion on a subject still new to the profession, and regarding which every day is accumulating experience and elucidating new facts.

But to return to my case, from which I have wandered further than I intended: it is quite clear that by none of these methods was death produced. The administration of chloroform presented no peculiarities; he recovered from its effects in the usual way. When I saw him ten hours after, there was nothing about him to induce the slightest apprehension. Are we therefore to exonerate the chloroform from any share in the fatal result? Is the fact of a perfect recovery from the immediate effects of the anæsthetic agent to be received as proof that the system had entirely thrown it off, and that the condition of the individual is precisely what it was before its administration? The symptoms which on the 2d September manifested themselves, bore too strong a resemblance to those artificially induced on the 1st, not to suggest the idea that the two events were in some way connected. There was the loss of the intellectual powers,—diminished sensibility, the strongest sinapisms produced no appearance of uneasiness,—the respiration was impeded and stertorous,—in a word, the condition in which he was on the 2d, seemed only an exaggeration of that produced on the 1st. Indeed chloroform acts by suspending the powers of the cerebro-spinal axis; its influence being directed especially to the sensitive nerves. Could we find an agent whose actions are limited to this, we would have reached the acmé of anæsthetic discovery. In addition to the fatal results of asphyxia and syncope, are we to add to the dangers of administering chloroform those which may arise from a return of coma? Is it possible that the comatose state induced by chloroform may outlast the period of its administration, or even return after some hours of apparent health? This question is one which has often forced itself on my mind, and to which the mere fact of no such occurrence having ever taken place seemed hardly a sufficient answer. Convulsions or epileptic fits seem not uncommon results. Dr Dunsmure, in speaking of his fatal case, remarks, that "he seemed to take a slight convulsion like an *epileptic fit*, and *such as I have seen on several occasions* in people who have led an intemperate life." Dr Murphy, in the *Association Journal*, September 2, 1853, details two cases in which chloroform was administered during parturition; in the first of which "distinct dyspnœa, excessive lividity of the face, and all the signs of extensive engorgement of the lungs and head," came on an hour and a half after labour; and though temporarily relieved, half an hour after "she suddenly awoke with a return of the distressing dyspnœa, that was soon followed by *convulsions* and almost immediate death." In the second case, two hours after the termination of labour, the patient was seized with slight cough and some difficulty of breathing; in the evening, dyspnœa had so much increased, that she was cupped to 10 ounces. She slept for two hours, "but when she awoke the dyspnœa returned with still greater distress, and continued to increase during the night. On the following morning she became asphyxiated, and died about 2 o'clock P.M.," 24 hours after the termination of labour. In remarking on these cases Dr Murphy asks the very appropriate question, Is it probable, that when no dangerous symptoms presented themselves during the administration of chloroform, fatal effects could result from it "at a time when the vapour was dissipated, or, if any remained, its force greatly weakened, and less likely to cause such effects?" We know that the chloroform acts on the nervous centres, and that its action is usually evanescent; but is it necessarily so? After the cause has been removed, may not the effect remain? The second of Dr Murphy's cases I have sometimes thought to have a shade of resemblance to the one I have just detailed, as far as symptoms go. The account is too scanty to enable us to form a decided opinion on this subject. Dr Murphy states that she died asphyxiated. Now my patient died asphyxiated, *i.e.*, the heart beat rapidly and convulsively after the respiration had become almost impeded; but I do not consider asphyxia to have been the *cause* of death, because, though its immediate precursor, it came on merely in the train of events. Death commenced from the brain (coma), and extended to the nerves of respiration (asphyxia). Had there not been *post-mortem* appearances clearly pointing to different causes of death, one might have thought that in both cases the fatal supervention of paralysis of the nervous centres might have been prepared by its first artificial induction. Dr Murphy states that it is improbable this should occur, seeing that he cannot "find the report of a single case in which the patient who inhaled chloroform without an unfavourable symptom was attacked two hours after with dyspnœa, still later became asphyxiated and died." And again he states that "he cannot find a case of the kind in the whole range of surgical practice."—"The danger—the only danger—which the surgeon dreads, is sudden death during the operation." I do not mean to say, that in Dr Murphy's cases death was caused by chloroform; but I do think that we have a right, derived from *à priori* reasoning, to fear that all danger is not past when the patient emerges unharmed from the anæsthetic condition; and that it is only more extended observation which can prove whether those fears are wholly chimerical. My own impression is, that in Mr T.'s case death was not caused by the administration of chloroform; but I ground that opinion, not on the fact that no other case of the sort has been recorded, nor on the supposition that, in the nature of things, such an event might not occur, but on the appearances presented by the *post-mortem* examination, coupled with the symptoms for which he first sought my advice.

A comatose condition was induced by chloroform, which was as evanescent as that state so induced commonly is,—twenty-four hours afterwards true coma supervened, and proved fatal. But the *post-mortem* examination of the head shewed that this latter condition was connected with effusion of bloody serum—a common cause of coma, but not (as far as we know) ever observed as a result of chloroform. In cases where death has resulted from the employment of this agent, effusion has never been found, and in many it is distinctly mentioned that the brain was not even congested; so that the comatose state is produced without the intervention of vascular excitement, and therefore by a process different from that which was found to have occurred in the case I have related. I admit, however, that it is possible that an influence may be exerted on the brain, capable of superinducing, after a longer or shorter period, effusion of serum or other secondary effects; as we know that a person may recover from concussion, and yet sink under other results of the same cause which produced the concussion. Nor will this appear a far-fetched comparison, if we suppose, as I think we must do, that chloroform acts directly on the nervous centres. But there is a feature of the *post-mortem* examination which I now would call attention to, and that is the pus found between the parietal and visceral layers of the arachnoid. This is a rare phenomenon, and therefore not so well understood as other lesions of the brain and its membranes, which have been more frequently observed. It was found as a thin layer spread over the summits of both lobes, and adherent to the visceral portion. It did not extend to the basilar portions of the brain, and therefore can hardly be supposed to have been caused by any extension of inflammation from the mucous membrane of the nose. Indeed, neither during life nor after death was there anything to indicate that inflammation of the Schneiderian membrane had taken place. It could not have caused compression, and therefore is of importance only as proving the existence of previous arachnoid inflammation. But there was no redness or visible sign of inflammation of the arachnoid. There was much congestion of the cerebral vessels, but the products of their inflammation would have been thrown out in the subarachnoid tissues. Now I cannot help thinking that had there been so intense inflammation of the arachnoid as to have caused purulent effusion in the space of time between my visit at ten o'clock P.M. and his death twenty-seven hours after, there would necessarily have been some *post-mortem* evidence. I conceive, therefore, that the condition which led to the formation of pus was pre-existent to the administration of chloroform. The feeling of stupidity, difficulty of articulation, loss of sense of smell, &c., may all have resulted, not from the closure of the nares by polypi, but from this internal disease. There was no heat of head,—no apparent increased vascular action of any sort,—nothing to call attention to any other cause than the mechanical obstruction. But after calmly weighing the whole facts of the case, I firmly believe that a low obscure form of arachnoid inflammation produced those symptoms which seemed fairly and naturally to be referred to the presence of polypi. What effect the administration of chloroform may have had in such circumstances it is difficult to say, but I feel convinced that it at all events did not set up the action which terminated so fatally.

The *post-mortem* appearances in this case were very peculiar. The position of the pus on the free surface of the arachnoid must be held conclusive as to inflammation having existed in that membrane. Dr Watson mentions the fact of such effusion as being rare (vol. i., p. 380), and quotes from Dr Abercrombie a case in which it was observed in a child who had been three weeks ill; and says, that "if simple arachnitis of an acute kind ever happen, it has not been my fortune to see or to recognise it." While pus was found in the summit of the brain, the effusion of serum at the base was into the arachnoid cavity, and not in the subarachnoid space; and though bloody (*i.e.* red-coloured) serum was found in the ventricles in more than usual quantity, it was not excessive, nor by itself at all sufficient to cause death. There was also congestion of the bloodvessels of the brain; not inflammation of its substance. But that and the effusion of serum might, and probably did, take place within the last twenty-four hours of life—the purulent deposit I think hardly could.

On these grounds I maintain that in the case I have related death did not result from chloroform. At the same time, I give it as my decided opinion, that until we find an agent which is capable of producing anæsthesia—inaction of the nerves of sensation—without implicating the functions of the cerebrum and motor nerves, we must be prepared to encounter occasional dangerous cases of asphyxia and syncope, and fatal ones in which these states occur simultaneously; and further, I would be inclined to expect, that cases might yet occur to prove that with the cessation of the immediate effect of the anæsthetic agent danger has not entirely ceased. In the meantime, I think it the duty of every surgeon to communicate to the profession any fact occurring in his practice in relation to this subject which presents novel and interesting features. It is in this way only that we shall learn the true merits of the case, and decide the important question of the compatibility of the anæsthetic agents at present known with all constitutions free from actual organic disease.

HULL, *December* 20, 1853.

ART. VII.—*Report of Surgical Cases occurring in Hospital Practice.* By JAMES DUNSMURE, M.D., F.R.C.S., Surgeon to the Royal Infirmary, Edinburgh.

Case of Lithotomy. Nucleus of the Stone—a portion of the Patient's trousers or drawers.

William Howell, aged fifty-three, bookbinder,—admitted 18th October 1853. He states that he enjoyed excellent health till the commencement of his present illness in September 1842. Having slipped his foot while stepping from the Bass Rock into a fishing-boat, he fell backwards on the "thowl-pin" which entered the perinæum immediately at the posterior margin of right side of anus; although much hurt, he was able to raise himself and take his seat in the boat without acquainting his companions of the accident. Very little hemorrhage occurred, and although faint, he was still able, when the boat landed, to walk home, a distance of a quarter of a mile, without assistance. Severe febrile symptoms, however soon appeared, and he was confined to bed for three weeks with inflammation "of the lower part of the bowels." After being confined for eight to ten days, a large abscess formed in the perinæum, and burst externally at the seat of injury, a quantity of dark fetid pus escaping, with great relief to his sufferings. Three days afterwards he observed while at stool that part of his urine escaped through the opening in the perinæum, and that air was mixed with the urine which passed along the urethra.

The discharge of pus and urine through the perineal wound continued when he emptied the bladder, till the following March (five months), when the opening ultimately closed. By this time he had recovered his general health, and was free from all suffering except after considerable exercise, when he had frequent calls to make water, accompanied with hot burning pains in the region of the bladder; at these times he observed that the urine was mixed with blood. A few weeks after this, and about six months from the date of the accident, he for the first time observed the stream of urine suddenly interrupted, as if by some body "plugging up the passage." This having occurred frequently, and being troubled at the same time with lancinating pains in the perinæum shooting along the penis, he consulted his medical attendant, who sent him to the Royal Infirmary. His bladder was examined by Mr Syme, but no stone could then be detected. When he applied at the hospital his urine appears to have been free from blood, but to have deposited, after standing, a copious sediment of ropy mucus or pus. On several occasions he passed by the urethra one or two elongated friable bodies like splinters of wood, white, rough on the surface, and easily breaking down under the finger.

He continued in much the same state till about two-and-a-half years before the period of his admission, when, being confined to bed for seven weeks with fracture of the tibia, he felt greatly relieved, being free from all pain, and able to pass his urine without any difficulty or uneasiness. On resuming his employment his former symptoms returned with great severity; he had frequent desire to empty the bladder, which was always accompanied with pain; the stream was frequently and completely obstructed, and the urine was mixed with pus and blood after the slightest exertion. About six months elapsed before he was sounded; a calculus was then detected; but having refused to submit to an operation, palliative treatment was had recourse to: his sufferings continued unabated, and lately became so much aggravated that he applied (to Dr Dunsmure) to have the operation performed.

On admission.—He has a pale sallow appearance and somewhat anxious expression, but appears otherwise healthy. He complains chiefly of lancinating pains in the perinæum, shooting along the penis and up to the loins; these are much aggravated during micturition, especially when the bladder has been nearly emptied. Micturition is frequent, the urine flows slowly at first; however, more from fear of pain than from actual obstruction,—any attempt at forcibly emptying the bladder being accompanied with excruciating pain. No sudden stoppage to the flow of urine has been experienced for some weeks. He has never had any distinct pain in the thighs or feet, but occasionally a feeling of slight cramp.

On examination the cicatrix of the wound can be traced in the perinæum at the verge of the anus. Nothing abnormal can be detected in the rectum. The prostate gland is somewhat enlarged. A sound is readily passed along the urethra, which appears perfectly normal; and the presence of a stone in the bladder can be detected both by touch and sound. Its surface, as far as can be made out, is smooth, and it is not of unusually large size.

His general health is at present out of order, from his having indulged a little too much before coming into the hospital. Tongue white, appetite bad, bowels open, pulse sixty-eight. Urine passed frequently and in considerable quantity, of a pale muddy colour, depositing, on standing, a copious gelatinous sediment, shewing under the microscope numerous pus corpuscles, blood globules, and a few crystals of oxalate of lime. Specific gravity of urine 1034, slightly albuminous, and becoming of the colour of dark "sherry" when boiled with liquor potassæ.

For two weeks after admission attention was directed entirely to his general health. The albumen in the urine gradually diminished, and in a short time all appearance of sugar disappeared. In a few days the urine, which on admission contained oxalate of lime, became intensely phosphatic; but this condition was easily removed by treatment, and for eight to ten days before the operation was performed the specific gravity was 1018; a mere trace only of albumen remained, not more than might result from the quantity of pus which was still present, mixed with a few blood corpuscles. His general health having much improved, and being anxious for the operation, lithotomy was performed by Dr Dunsmure on the 4th of November, and a stone about 2 inches long, 1½ broad, and ¾ thick, was removed without difficulty. Part of the external crust, of a reddish-brown colour, gave way under the pressure of the forceps, but all the debris was included, along with the stone, in the blades of the instrument. On examination afterwards the external brown crust was found to be very thin, and below it the stone was of a white colour, and of a loosely laminated structure, portions of the laminæ breaking off under slight pressure. No hemorrhage followed, and in about three hours after the operation the urine began to come away by the tube; he slept for some hours in the evening, and on awakening passed several ounces of urine by the urethra. The tube was examined immediately after and found to be perfectly free, with a small quantity of bloody urine dribbling through it. During the night after the operation he slept for six hours continuously, and in the morning the urine was found escaping much more freely by the tube; but as he had again passed some ounces by the urethra, the tube was removed at the visit, twenty-four hours after the operation, to afford more free outlet by the wound. From this time the urine escaped by both passages with very little suffering, the quantity coming by the urethra gradually increasing till the 18th, being the fourteenth day after the operation, when it ceased to flow by the perinæum altogether. The wound gradually closed, and with the exception of slight irritation of the left testicle, the patient never presented an untoward symptom, and on the 6th of December was dismissed cured.

The above case has been extracted from the hospital journal by the kindness of my house-surgeon, Dr Alexander Struthers, and is chiefly interesting from the nature of the accident which caused the production of the calculus. When I first saw the patient, he mentioned to me that he thought the thowl-pin of the boat had penetrated his bladder, and had carried along with it a portion of his drawers and trousers. I was not inclined to believe this statement, as I conceived the contused and lacerated wound of the bladder, caused by such a large body as a thowl-pin, would have produced even more severe symptoms than those from which he suffered immediately after the accident. I ascribed the flow of urine through the wound to the abscess which formed having eat its way by ulcerative absorption into the membranous portion of the urethra, and that extravasation of urine was prevented by consolidation of the tissues from inflammatory exudation. But a few days after the operation, when the stone was kindly cut for me by my colleague Dr Mackenzie, it was found that its nucleus consisted of a mass of woollen texture, of the size of a filbert nut. This was lying in a cavity considerably larger than itself, which was formed evidently by the cloth shrinking and giving off the urine which it contained, by exosmosis, after the phosphates had been deposited upon it. If the coats of the bladder had not been torn at the time of the accident, I can hardly conceive it possible that such a large mass of cloth could have afterwards entered its cavity by an opening taking place by ulceration either in the urethra or bladder; and no extravasation occurred at first, from the urine being prevented escaping by the wound being plugged up by the cloth. The case also is another example of a most severe wound having been recovered from by the mere *vis medicatrix naturæ*, the patient having applied for no medical assistance until more than six months after the accident.

We can also readily understand how no calculus was detected when Mr Syme examined his bladder, the soft foreign body not communicating to the sound that peculiar click so indicative of stone in the bladder.

Dislocation of the Tibia and Fibula backwards.

John Boyle, aged forty-four, a labourer—admitted on the 2d of August 1853. States that when working upon the junction branch of the Caledonian and Edinburgh and Glasgow railway, a large quantity of earth fell upon him, pushing his body forwards while his right leg was in a fixed position. He was knocked down by the weight, and was unable to rise. He was immediately brought to the Infirmary along with one of his companions, who was likewise injured by the same slip of earth.

Upon examination of the limb, the heads of the tibia and fibula were found partially displaced backwards. The condyles of the femur were prominent and projecting. The ligament of the patella was not ruptured. The patient was quite unable to flex or extend the knee. Having been put to bed, Dr Broadbent, my house-surgeon, assisted by one of his fellow clerks, gently extended the leg, when, without any difficulty being experienced, the bones resumed their natural position. The limb was kept steady by being put upon a straight M'Intyre's splint; considerable effusion of blood had already taken place into the knee, and the farther flow was attempted to be arrested by having cold wet cloths assiduously applied to the joint. The patient was in a state of collapse. When I saw him at visit the following day the knee was swollen and exceedingly painful to the touch; the state of depression had passed off, and that of reaction had commenced. Two dozen of leeches were ordered to the knee, and were repeated the next morning with great relief to the pain. The strictest antiphlogistic regimen was enjoined. From this time the severity of the symptoms seemed to be subdued; leeches, however, were had recourse to on several occasions, and these, with warm evaporating lotions, low diet, a gentle course of mercury, and complete rest to the joint, had so entirely removed all pain, that by the end of August I considered him out of danger; so much so, that the limb was removed from M'Intyre's splint, and a wire one placed under the articulation, as being lighter and not so irksome to the patient. About this time, feeling himself so well, he was in the habit (unknown to me) of getting out of bed and walking in the ward; which imprudent conduct, as was to be anticipated, soon brought on a renewed attack of inflammation of the joint. By leeching and antiphlogistic treatment this was again subdued, and the knee brought into a quiescent state As he could not be convinced of the necessity of giving his joint complete rest, and as he was tired of the restraint put upon him in the hospital, he was discharged at his own request, on the 4th of October. Before the relapse of inflammation occurred the swelling of the joint had entirely subsided; but upon his dismissal from the infirmary, although there was no pain, the knee was still somewhat enlarged.

This case is instructive, as being an example of a very rare form of accident; the complete dislocation of the leg backwards is still more rare, and can only occur after rupture of the ligamentum patellæ, or of the tendinous expansion of the quadriceps muscle to the patella. The case shews also how, by severe antiphlogistic treatment, a joint such as the knee, and which is so liable to become disorganized when attacked with acute synovitis, may ultimately be saved. I heard of this man on the 6th of December, when he had so entirely recovered as to be able to walk to Portobello, a distance of three-and-a-half miles from his own house. His knee, however, was still somewhat stiff.

Rupture of Internal Lateral Ligament of the Knee-joint.—Recovery.

Francis M'Kay, aged forty, smith,—admitted 21st June 1853. He states, that, having leaped over a stone while running down Arthur's Seat, a few hours before admission, his left foot slipped outwards, his knee at the same time bending inwards, bringing him to the ground with great violence. He says that he distinctly felt something give way with a snap at the inner side of the knee, while it was half bent under him. He was unable to rise, and his companions, believing that the knee was out of joint, immediately employed extension by violently pulling the leg, with the view to set it again. As he was still, however, unable to walk, and suffering great pain, he was assisted home, and then brought to the hospital. On careful examination of the limb, while the patient was under the influence of chloroform, no signs of fracture can be detected; but there is unnatural mobility at the knee in a lateral direction, allowing the leg, when fully extended, to be partially bent on the thigh in an outward direction. No such motion is admitted of inwards; the leg can be made to resume its position in a straight line with the thigh, but cannot be carried further inwards.

The limb below the knee is much swollen, being almost double the size of the opposite one, the integuments tense, and partially discoloured; the superficial veins are very much distended, and there is considerable effusion into the joint; but above this the parts are natural. The pain in the knee is very severe, but most complained of at the inner side of head of tibia, corresponding to the part which he felt gave way. The popliteal and tibial arteries are felt beating with the same force as on the opposite side.

The limb was placed on a M'Intyre splint, sixteen leeches were applied to the knee, and an evaporating lotion subsequently laid over the joint. He was ordered an antimonial saline purge. On the following day, the pain was much relieved, the pulse of moderate strength, and the swelling of joint and limb considerably diminished. Bowels freely opened by medicine.

24th.—Pain in the knee almost gone, but still considerable effusion into the joint. The swelling of leg has much diminished, and the integuments are assuming a greenish-yellow colour.

July 1*st.*—Pain quite gone from knee; the swelling of the leg is rapidly going down, and the veins have resumed their natural appearance; but the surface has still a dingy yellow colour. From this date the patient rapidly recovered, and was dismissed cured on the 22d of July, one month after the accident.

This case was evidently rupture of the internal lateral ligament, and, in connection with the previous one, is interesting, as illustrating the rapidity with which a large joint recovers from a severe accident, without such an amount of inflammation being produced as to terminate in destruction of the articulation.

Case of Tetanus following Lacerated Wound.—Death.

Alexander Peddie, aged fifty-five, horse-dealer,—admitted 12th November 1852, on account of a wound of the knee, having been thrown from his horse a few hours previously. A little above the left patella there is a transverse wound of the soft parts about 2 inches in length, but not communicating with the joint, or dividing the fascia lata. It was filled with mud, and the edges were gaping, but he did not appear much hurt, or to suffer much pain. The wound was carefully washed, the edges brought together by sutures, and the limb placed in a raised position; on the following day the sutures were removed, strips of adhesive plaster substituted, and the tepid water dressing applied. On the evening of the 14th he complained of hoarseness, accompanied by difficulty of speech, which he attributed to exposure to cold before admission, also of pain and constriction in the chest. On the 15th the pain in the chest assumed a sharp shooting character, and he now suffered from a feeling of stiffness about the neck. Nothing abnormal could be detected by the stethoscope beyond a few bronchial râles. On the 16th the pain in the chest had yielded to counter-irritation and Dover's powder; the voice was less husky, but the rigidity of the muscles of the neck continued. On the 18th this rigidity had much increased, and was also felt in the muscles of the jaws; he had severe pain behind the ear. At 4 P.M. of the same day he could not open the mouth, but was still able to speak; the muscles of expression had become contracted, and he had great difficulty in swallowing, and in moving the tongue. On the 19th the muscles of the neck were quite rigid and hard, those of the face were much contracted, and there was complete trismus; the difficulty of swallowing was very great. On the 20th he was found to have had four hours' sleep from the effects of morphia, but was no better; the abdominal walls had become stiff and hard, and the features were more pinched and expressive of suffering. The arms and legs were occasionally convulsed with spasmodic jerking. In the afternoon the muscles of the neck became relaxed so as to permit of nourishment being introduced by the stomach-pump, but those of the back became affected shortly after; and when he was slightly raised, the whole body was found perfectly stiff, no flexion occurring at any of the joints. The muscles of respiration were in the same condition, scarcely any movement being perceptible in the walls of the thorax; the abdominal parietes being in the same spasmodic state, the breathing was obviously very much impaired. Coma gradually supervened, and he died on the 21st, at 5 A.M.

The treatment for the first day consisted of counter-irritation to the chest, with Dover's powders, as tetanus was not suspected; but when the true nature of the case was evident, smart doses of turpentine and castor-oil were administered, followed up by the solution of muriate of morphia in ʒi doses every hour until some ounces were taken. When no relief was obtained, the tincture of the Cannabis Indica was tried in ʒ ß and ʒi doses. Bladders of ice were kept along the spine, and when the power of swallowing was nearly lost, his strength was attempted to be maintained by nourishing enemata. Nothing that was used seemed to be of any benefit.

No *post-mortem* examination was allowed.

Osteo-Sarcoma of Upper Jaw—Excision of Superior Maxilla.—Recovery.

James Melrose, aged forty-three, farmer, admitted 21st April 1852. States that he enjoyed excellent health till about two years ago, when he observed a small hard swelling on the right side of the hard palate, which he attributes to an injury of the jaw received twelve years previously, while having two teeth extracted, several small pieces of the jaw having subsequently exfoliated. Shortly after his attention was attracted by the tumour in the mouth; another of the same kind made its appearance on the outer surface of the maxillary bone of same side. That on the palate continued gradually to increase in size from the time he first discovered it, but the other remained almost stationary till within the last two months, during which its growth has been very rapid.

On admission, his general health appears unaffected, the only complaint he has being the inconvenience and anxiety from the tumour in the mouth. On examination, nearly the whole surface of the hard palate of right side is involved in a hard, irregularly-flattened tumour. It is perfectly circumscribed, of a hard, bony consistence, and covered by the soft parts of the roof of the mouth, its under surface projecting below the level of the front teeth. The swelling of the outside is not so large, being about the size of a walnut, but presents the same circumscribed appearance and bony consistence as the other. It occupies the canine fossa of the right superior maxilla, situated midway between the edge of naris and the molar bone. As the tumour was evidently of a benign nature, and presented the characters of an osteosarcomatous growth, it was resolved to remove it by excising the superior maxilla. This was accordingly done on the 5th of May. An incision was first made from the angle of the roof of the mouth to the anterior root of the zygoma, the integuments were dissected back, and the bony connections of the maxilla separated with the cutting pliers, The soft palate was then divided by the knife, and the jaw, with the tumour attached to it, pulled from its place. The bleeding vessels having been secured, and the cavity filled with folded pieces of lint, the external wound was carefully approximated and secured by the interrupted and twisted suture. The incision in the cheek healed almost entirely by the first intention; the cavity gradually contracted; and the patient, without ever having shewn a bad symptom, was dismissed cured on the 12th of June. The tumour, when examined after the operation, was found to have originated in the antrum, and was of the size of a small hen's egg.

The case presents nothing particular, with the exception of being another example of the safety with which the superior maxilla can be removed for tumours of a benign nature, and also of the almost certainty of extensive incised wounds of the cheek healing by the first intention, if care is taken to approximate accurately the cut edges.

Popliteal Aneurism—Ligature of Superficial Femoral.—Recovery.

John Cochrane, aged twenty-one, coachman, of a florid complexion, and somewhat dissipated appearance,—admitted 5th October 1852. States that seven weeks ago, when hopping on the pavement, he experienced a sharp pain in the right popliteal space, and something appeared to give way. For some time he paid no attention to it, merely conceiving he had sprained his limb. He continued his employment till a few days ago, when the pain in the ham and leg became so severe as to oblige him to apply for relief. On examination of the limb, a pulsating tumour about the size of a hen's egg is distinctly felt in the ham. By pressure, the tumour is much diminished in bulk, but the pain is increased. The leg is somewhat swollen, but does not pit on pressure. No *bruit* can be detected over the region of the heart. As it was thought advisable, owing to his former mode of life, that no operation should be had recourse to immediately, he was merely ordered to keep his bed, while attention was paid to his general health. For a fortnight all went on favourably, when, without any obvious exciting cause, inflammatory action suddenly commenced in the tumour, which increased to twice its size, accompanied by severe irritative fever, with delirium. From the swelling of the leg, the enlargement of the aneurism, and the great constitutional disturbance, it was apprehended that the sac had not only given way, but that gangrene was going to occur. Fortunately, the constitutional symptoms gradually subsided under the use of antimony, opium, and general antiphlogistics, and he was soon in as favourable a condition for the ligature of the femoral artery as could be expected from his previous condition, Accordingly, on the 22d of October, I performed the operation for deligation of the superficial femoral in the triangular space of the thigh. The vessel was separated from its sheath to as small an extent as possible, and no difficulty was experienced in passing the needle round it. The pain in the limb, of which he complained much before the operation, now ceased, and everything went on favourably, the tumour gradually diminishing in size. On the seventeenth day after the operation, the ligature came away. The wound by this time had healed, with the exception of the sinus formed by the ligature. On the twenty-second day, the edges of this looked inflamed, and somewhat sloughy. His appetite now began to fail. Sixteen days after the ligature was removed, I was informed that a considerable quantity of blood had flowed from the wound; but as the bleeding at the visit had entirely stopped, I merely applied a folded piece of lint to the sinus, without using any pressure, from fear of exciting inflammatory action in the wound, the edges of which were still somewhat sloughy. For several mornings, the lint was soaked with blood, after which it gradually ceased; but the sinus formed by the ligature did not heal till towards the close of January, and he was not dismissed cured till the 22d of February.

From the history of the foregoing case, I feel satisfied that had the treatment of external aneurism by compression been had recourse to, the patient in all probability would have sunk. By his own confession, on admission, he had led a very hard life: this, coupled with a peculiarly irritable temperament, which made him unable to bear pain, and the unsound state of his system, as indicated after the operation, by the long time the wound took to heal, would have made him, in my opinion, a most unfit subject for compression. From what source the bleeding that occurred on the thirty-third day came, must be a subject of some dubiety. If the statement is correct, that the quantity which flowed from the wound was sufficient to form a small pool in the bed, we must almost suppose it to have come from the femoral itself, ulceration having occurred in its coats from inflammatory action extending down to them from the sloughy edges of the sinus; but I cannot imagine hemorrhage from such a large vessel to have been arrested, as was the case in this patient. I conclude, therefore, that the bleeding proceeded from some small artery external to the femoral. It is by no means unusual for slight bleeding or oozing to occur at the time of, or shortly after, the separation of the ligature; but it is not common for such a large quantity to come as in this man, and at such a long period (sixteen days) after the ligature was taken away. The aneurismal tumour had almost entirely disappeared when he left the hospital.

ART. VIII.—*Clinical Surgery—Report of Surgical Cases occurring in Hospital Practice, &c.* By R. J. MACKENZIE, Esq., F.R.C.S.E., Surgeon to the Royal Infirmary, and Lecturer on Surgery.

Loose Cartilages in the Knee-joint.

In the number of the *Monthly Journal of Medical Science* for November 1852, Mr Syme drew attention to a new mode of remedying this very troublesome affection ; and the following cases illustrate well the serious consequences which occasionally followed the old mode of removing these bodies, as contrasted with the safety of the treatment lately proposed by Mr Syme, which is simple as regards the mode of operating, and appears to be effectual as regards its results.

Sarah Salkeld, a domestic servant, aged twenty, was admitted under my care in the hospital, December 3, 1850, on account of a loose cartilage in the right knee-joint. She attributed the origin of her complaint to a fall on the knee when she was a child of five years of age. The knee has been weaker than the other ever since that time. Within the last three or four months, the usual annoying symptoms accompanying the presence of a loose cartilage in the joint have almost entirely prevented her from following her occupation ; repeated sudden attacks of acute pain in the limb whilst walking, with simultaneous loss of power, followed by attacks of synovitis with effusion. She came to the hospital determined at all risks to have the cartilage removed. The loose body could be readily made to appear on the inner side of the patella, and could be pushed to all parts of the joint on the inner side, but it could not by any manipulations be made to appear on the outer side.

The mode more recently introduced by Mr Syme of fixing such bodies by subcutaneous incision was at this time unknown ; and, as I shall mention presently, I had reason to doubt the general efficacy or safety of the mode, generally practised, of extruding the cartilage from the joint into the surrounding cellular tissue, through a subcutaneous incision of the capsule, as formerly proposed by Mr Syme and M. Goyraud, or as modified by Mr Liston. I determined, then, to remove the body by a simple incision; and, as it could be easily made to protrude under the skin, there seemed no probability of there being any difficulty in its removal.

The first incision exposed the synovial capsule, but divided a vessel, which bled so freely from both ends as to require the application of two ligatures. The second incision exposed the cartilage, which, previously transfixed by a cataract needle, was readily drawn through the opening thus made. Unfortunately, however, it was found to have a long and pretty thick pedicle; this being divided, the wound was instantly closed accurately. The cartilage was of the usual lens-like shape, and rather larger in diameter than a sixpence. All precautions were taken to secure primary union of the wound, and to avert inflammatory action, but on the second day following the operation, acute inflammation of the joint set in; profuse suppuration followed, which extended from the joint beneath the deep muscles of the thigh as far as the groin. For nearly ten months it appeared that amputation was the only probable means of saving her life. This measure, however, was happily delayed, and after nearly twelve months of great suffering her symptoms began to improve. Her convalescence, however, was protracted, and even at this date (three years after the removal of the cartilage,) her health is not thoroughly re-established. Anchylosis of the knee has taken place, and she is still unable to walk without the support of a stick.

The following short history of a similar case affords a striking contrast to that of the sufferings of this poor girl. It is one of the few cases in which Mr Syme's later mode of operating has been followed; and as the operation is one which appears to have attracted too little attention, the following instance of its simplicity and efficacy appears to me worthy of attention.

Alexander M'Brain, a fisherman from Lochgilphead, was admitted into the hospital under my care on the 16th of February 1852. For about five months previously he had suffered from weakness of the left knee-joint, which prevented him from following his occupation, and which was aggravated by repeated attacks of effusion into the joint, which always followed any attempt to walk.

Having learned from Dr Hunter of Lochgilphead that these symptoms were dependent on the presence of a loose cartilage in the joint, he came to Edinburgh for the purpose of having the body removed. He stated, on his admission, that, although he never suffered much pain in the knee, the state of the joint rendered him so useless that he was willing to submit to anything which would restore the use of the limb.

The cartilage was readily detected. It was of a flattened shape, and of the diameter of a shilling, and could be easily pushed about to all parts of the joint.

On the 12th February, having pushed the body as far as possible outwards over the external condyle of the femur, and keeping it fixed there by the finger of an assistant, I introduced a curved tenotomy knife through the integuments, at about an inch distance, and turning the edge of the blade upon the cartilage, cut freely down upon it. Being satisfied that the joint was sufficiently opened, I pushed the cartilage edgeways into the opening and withdrew the knife.

A compress was then placed over the track of the subcutaneous wound, and a second placed on the inner side of the cartilage, so as to keep the body steadily in the above position, whilst, as a matter of precaution, a splint was placed on the back of the limb, so as to keep the joint immoveable.

No uneasiness followed this trifling operation, and the bandage was not removed till the 27th. The cartilage was then felt in the same position and evidently firmly fixed. The compress was re-applied and retained in its place by an elastic knee-cap. About a fortnight after this he was allowed to walk about, and he would have returned home had he not been suffering from a venereal sore which he had contracted on his journey from home.

He remained in the hospital till the 19th of May, when he returned home perfectly well. The cartilage was still to be felt firmly adherent in the same position, but diminished to less than a half of its original size. The limb had nearly quite regained its former strength, and he walked without any limp or uneasiness.

November 1853.—Dr Hunter informs me that this patient has followed his occupation as a fisherman ever since his return home, and has never experienced the least annoyance from the limb, which is as strong as ever. The cartilage can still be felt firmly fixed in its place, but reduced to a very small size.

Mr Syme, it appears, has followed this mode of fixing these cartilages in several cases, and in each case with success, and there seems every reason to believe that the plan is the most simple, safe, and effectual, which has yet been proposed for an affection, the surgical treatment of which was formerly never undertaken without some fear as to the result. Although the proposal is new, however, I think there is no reason to doubt that this very method must often have been practised (unwittingly) by those who attempted the extrusion of the cartilage from the joint, through a subcutaneous incision. This proceeding has been generally spoken of as a very easy matter, and no doubt has been entertained as to such bodies being pushed by proper manipulations into the cellular tissue outside the joint; without denying the practicability of effecting this object, or that it has been sometimes accomplished, I may mention that repeated and varied trials of the operation on the dead body have satisfied me of the extreme difficulty of its accomplishment, and of the force which must be used to push the body entirely outside the joint through a subcutaneous opening of the synovial membrane. This appears to arise from the difficulty of rendering a sufficient extent of the synovial membrane tense, so as to allow of its free subcutaneous division. A small opening in the membrane is easily made, where the prominent edge of the cartilage pushes it outwards; but I have found it a very difficult matter to make an opening of the size of the diameter of the cartilage, or one of sufficient size to admit of its escape without using unwarrantable force in its extrusion. These observations are borne out by the fact, that repeated attempts have sometimes been made to dislodge a loose cartilage in this way without success; and the risk of exciting inflammation of the joint by using force to effect the object has been proved by the operation having been followed by a fatal result in more than one instance.

The object of the operation more recently introduced by Mr Syme, and described in the case which I have related, is to fix the cartilage in the wound of the synovial capsule, with a view of obtaining its adhesion to the surfaces of the wound. For this purpose, the joint is opened by a subcutaneous incision over the cartilage, which is previously made to project so as to render the synovial membrane tense. The cartilage is then gently pushed edgeways into the wound, when it is fixed by a compress of lint, secured by a bandage or strips of adhesive plaster. The adhesion of the cartilage to the surface of the subcutaneous wound seems to take place readily, as if the body possessed a sufficient degree of vitality to render it capable of connecting itself to the tissues with which it is kept steadily in contact. That it adheres to the opening in the synovial capsule, and is not merely partially encysted like a foreign body, seems evident from the firmness of its attachment in its new position even a few days after the operation, and from the rapid absorption which it subsequently undergoes, as evinced by its speedy diminution in size, and apparently its ultimate complete removal.

Dislocations.

Of the cases of dislocation which have come under my observation in hospital practice, the following appear worthy of attention,—some as instances of the rarer forms of dislocation, and others as illustrating the immense advantage gained by the use of chloroform as an auxiliary to the mechanical means employed in the reduction especially of dislocations of less recent occurrence.

Dislocation of the head of the Thigh-Bone on the Dorsum of the Ilium—Reduction ten days after the occurrence of the injury.

Michael Gillon, aged forty-five, a railway porter,—admitted into the hospital October 15, 1853, having ten days previously injured the right hip by a fall, whilst carrying a heavy load on his shoulders. The signs of dislocation were distinct, but the shortening of the limb not being more than one and a half inch, the great toe resting on the ball of the great toe of the opposite foot, and the possibility of flexing the thigh on the trunk to less than a right angle, led at first to the belief that the head of the bone lay in the sciatic notch. He could, however, lie flat on his back whilst the thigh was pressed down on the mattress, a position which (according to an observation of Mr Syme, and which I have seen corroborated in several instances) cannot be assumed when the head of the bone is in the sciatic notch. Previously to any attempts being made at reduction, he was placed under the full influence of chloroform, when the head of the bone, which had hitherto been felt only obscurely, was now distinctly perceived on rotation of the limb, rolling on the dorsum of the ilium.

Manual extension with the heel in the perinæum having failed, the pulleys were employed in the usual way, and extension was made, according to rule, in the axis of the dislocated limb. The head of the bone descended readily to the level of the acetabulum, but every effort to raise the head of the bone over the edge of the cotyloid cavity failed. These efforts having been continued, with one or two intervals, for above half an hour, it was suggested to me that a trial might be made of extension in a different direction. I then had the patient laid straight on his back *and applied the extension in the natural axis of the limb*, that is to say straight downwards. The first attempt made in this way was at once successful; the head of the bone had no sooner descended to its proper level than it returned into the acetabulum with a distinct shock.

Dislocation of the head of the Thigh-Bone into the Sciatic notch—Reduction between five and six weeks after the occurrence of the injury.

A railway labourer, of moderate height, and of strong muscular development, met with an injury of the right hip from a fall of earth. Some doubt was entertained at the time of the accident as to the nature of the injury, but it was looked upon as a bruise of the joint, and treated as such for forty days, at the end of which time his surgical attendant suspected the existence of dislocation, and requested me to examine the joint. The usual signs of dislocation of the head of the bone into the sciatic notch were then well marked, and the peculiar mark of this dislocation, which I have already mentioned as pointed out to me by Mr Syme, confirmed the diagnosis; when the thigh was forcibly depressed on the mattress, the back was arched, and when the back was pushed down, the thigh was again advanced.[1]

The patient having been placed fully under the influence of chloroform, extension and counter-extension were applied in the usual way, viz., in the axis of the dislocated bone, whilst the head of the bone was raised by means of a sheet placed under the fold of the thigh. A continuance of these means for about twenty minutes was successful, the head of the bone returning into the acetabulum (as is commonly observed in dislocations of old standing) with scarcely any perceptible shock. The patient made a slow recovery, but ultimately regained entirely the free use of the limb.

Dislocation of the head of the Thigh-Bone on the Os Pubis—Unsuccessful attempts at reduction nine weeks after the occurrence of the injury.

J. A., aged forty-eight, a short but stout and muscular man, was admitted under my care in the hospital, December 17, 1850, on account of an injury of the left hip-joint, which he had sustained nine weeks previously. The state of the limb on his admission into the hospital was as follows:—Shortening to the extent of about three-quarters of an inch; eversion of the limb, the foot being nearly at a right angle with its fellow, the knee slightly bent, the prominence of the trochanter major nearly effaced; and the globular head of the bone felt in the groin, midway between the anterior superior iliac spine and the pubes, and rolling under the hand on the limb being rotated. The femoral artery pulsated strongly over the centre of the head of the bone. Numbness of the fore part of the thigh and knee was complained of, but no pain was felt when the limb was at rest, when it was extended, abducted, or everted; the slightest attempt at inversion produced acute pain along the front of the thigh and around the knee. The mode in which the dislocation was produced was peculiar, inasmuch as it was caused by direct violence. Whilst endeavouring to cross a railway line in front of an engine travelling at full speed, he was knocked down, one of the *buffers* having struck him on the left hip. He was seen on the following day, when the parts were much swollen, by a surgeon, whose acknowledged accuracy sufficiently attests the difficulty which must have attended the diagnosis. After repeated careful examination, the injury was looked upon as a fracture, and treated by the application of the long splint.

---

[1] This peculiarity of the dislocation into the sciatic notch probably depends on the stretching of the psoas and iliacus muscles, or on the pressure of the head of the bone on the sciatic nerve, when the axis of the limb is forced towards that of the trunk.

On the day following his admission into the hospital, chloroform having been administered, reduction was attempted by extension, first in the mode recommended by Sir A. Cooper, viz., in a line behind the axis of the body, so as to draw the thigh-bone backwards, whilst the patient lay on his back on a table, about two and a half feet from the ground; next in the same direction, whilst the patient lay on the opposite side; and lastly in a line straight downwards whilst he lay on his back.

The head of the bone readily descended under the influence of the extension by pulleys to the level of the acetabulum, but repeated and violent efforts failed to raise it over the edge of the socket. At the end of an hour I desisted from further attempts at reduction, and had the patient removed to bed.

Two days afterwards I prevailed on him to submit to a second attempt, and he was again, whilst under the full influence of chloroform, subjected to the same treatment. The direction of the extension was varied as before, and the utmost extent of force employed which appeared justifiable. The head of the bone receded under the extension till it could no longer be felt, and seemed ready to enter the acetabulum, but at once returned to its abnormal position when the extension was allowed to cease. After an hour and a quarter the patient was removed to bed with the dislocation unreduced.

A good deal of pain and swelling of the limb followed this second attempt, and he could not be prevailed on to submit to any further trials.

Three years have now elapsed since the occurrence of the injury; the head of the bone is still prominent in the groin, but moves freely with the motions of the limb. The patient can walk many miles a day without difficulty. The foot is everted and the limb is a little shortened, but he walks stoutly and with only a slight limp.

Dislocation of the head of the Humerus into the Axilla.—Reduction ten weeks after the occurrence of the injury.

Robert Weston, aged forty-five, a carter, was admitted into the hospital March 13, 1852, on account of an injury of the right shoulder, which he had received on January 3. He described the accident as having occurred by his falling from a cart on his side, but does not remember in what position the arm sustained the violence. The signs of the dislocation of the head of the humerus into the axilla were so distinct when he came into the hospital, that it was impossible to mistake the nature of the injury.

Having laid the patient on a mattress in the operating theatre, and administered a full dose of chloroform, I adjusted the worsted skein above the elbow, and prepared the pulleys and belt for the extension of the limb. Before applying these, however, I requested my clerk to take off his boot and apply extension with his heel in the axilla. My object in doing so was to shew the gentlemen who were present the mode of applying the extension in this way, as I had little or no expectation of its proving successful in reducing the dislocation. Extension was thus made from the wrist, whilst the muscular system was thoroughly relaxed by the full action of the chloroform, and reduction was instantly effected, the head of the bone returning into the glenoid cavity with a slight shock, and as readily as if the dislocation had occurred quite recently.

The unusual facility with which reduction was effected, led me to doubt the accuracy of the patient's statement as to the date of the accident. On farther inquiry, however, I ascertained that his statement was correct, that *seventy days* had elapsed between the occurrence of the dislocation and the reduction which had been so easily effected. I know of no instance of the reduction of a dislocation, in which the benefit of chloroform was so unequivocally marked; dislocations of the shoulder have been repeatedly reduced at a later period than ten weeks after the injury, but not, so far as I am aware, without considerable difficulty, and much more powerful efforts than in the above case. The facility of reduction in this instance is encouraging, as regards the attempt to reduce dislocations of much older standing, when the muscular resistance is thoroughly overcome by the full action of chloroform.

Dislocation of the sternal end of the Clavicle backwards.— Reduction.

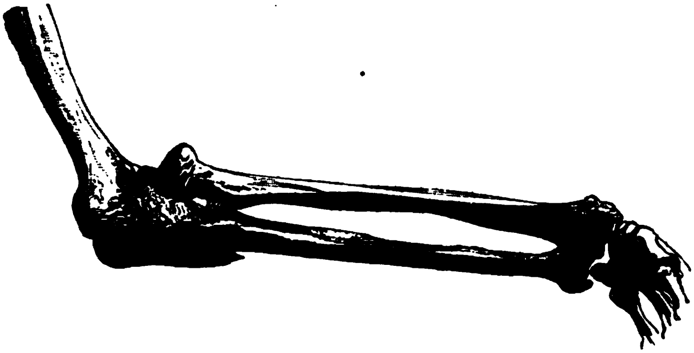
John Combe, aged twenty-six, a tall and muscular man, was brought to the hospital on account of an injury which he had received a few minutes previously in the following way: —Whilst walking along the street he tripped on the kerbstone, and falling forwards, pitched on th eright collar-bone against the corner of a heavy log of wood. For a short time he was unable to rise, and when he got on his feet, he was so unsteady that he was obliged to be supported on each side whilst he walked to the hospital, a distance of two or three hundred yards. I happened to see him as he was brought in, and was struck with the remarkable appearance which he presented. The right shoulder was thrown forwards and the neck bent so that his chin nearly rested on the sternum. The veins of the neck and over the temples and forehead were turgid, and the face and lips livid from venous congestion. He staggered when he attempted to walk, and could with difficulty be made to answer any questions put to him. On raising the head the nature of the injury was very apparent. The prominence of the sternal end of the clavicle was effaced; the bone could be traced with the fingers from its acromial end sinking backwards till its sternal extremity, thrown behind the upper part of the sternum, could no longer be perceived. I directed an assistant to place his knee on the patient's back, between the shoulders, and placing a hand in front of each shoulder, to draw them forcibly backwards. A second assistant raised the head with his hands placed under the chin. I at the same time endeavoured to push my fingers behind the clavicle near the shoulder, so as to urge the bone forwards. The first attempt made in this way was immediately successful, the bone returning to its place with a distinct shock. The venous obstruction and semicomatose condition, produced by the pressure on the brachio-cephalic trunk, were at once relieved, and in the course of two or three minutes the patient had fully recovered his senses and could walk steadily. Considerable swelling followed the injury, but had nearly disappeared when he left the hospital ten days afterwards.

Dislocation of the head of the Radius forwards, of four months' standing.

Patrick Connor, aged twenty, a labourer, was admitted under my care in the hospital, April 30, 1851, on account of an injury of the left elbow, sustained a little above four months previously, by his having fallen, when in a state of intoxication, among the machinery of a threshing-mill. On the following day he applied to a bone-setter, who applied splints around the injured elbow, which were not removed till seven weeks afterwards. The joint remained stiff for some time, but the power of flexion and extension gradually increased as he made use of the limb. On his admission into the hospital, the arm presented the following appearance:—the upper part of the forearm was remarkably rounded; the hand could be fully pronated, but could not be rotated outwards so much as to half-way between pronation and supination; the forearm could be extended nearly to a straight line with the arm, but could not be fully flexed, the motion being suddenly checked before the elbow was bent quite to a right angle: the external condyle of the humerus appeared to project unnaturally, and immediately below it a hollow existed in the natural situation of the head of the radius; in front of the condyles of the humerus, an unnatural prominence was perceptible to the eye when the forearm was extended, and this projection could be easily felt, in any position of the arm, to consist of the head of the radius lying in front of the condyles of the humerus, where it could be perceived to roll, on rotating the bone from the hand; lastly, an inequality was observed on the posterior surface of the ulna, a short way below the olecranon, which evidently depended on the union of an oblique fracture of the ulna.

Forcible extension and counter-extension had not the slightest effect in altering the abnormal position of the head of the bone, which it was evident was firmly fixed in front of the condyles of the humerus. I therefore made no attempt at reduction. He was directed to use the limb freely, and during the short time he remained in the hospital the mobility and power of the limb appeared to increase. The power of supinating the hand, however, seemed to become even rather diminished.

About a year afterwards he was admitted as a patient into the medical department of the hospital on account of an internal affection, from which he died. I obtained the elbow-joint, the condition of which is represented in the accompanying woodcut. The two points worthy of notice in the preparation are the position of the fracture of the ulna and the attachment of the orbicular ligament to the neck of the radius.



In another preparation in my museum of an old dislocation of the radius forwards, the ulna has been fractured in nearly the same line. A precisely similar complication of this dislocation is mentioned by Sir A. Cooper.[1] The explanation of the occurrence of the fracture seems very simple. The shape of the upper articulating end of the ulna is such as not to admit of its dislocation forwards, and the resistance thus offered to the violence which throws the head of the radius forwards, causes the ulna to give way. The dislocation being usually the result of indirect violence (a fall on the hand), accounts for the very oblique line of the fracture. Such a complication must render the after-treatment of the case more difficult; proper adjustment of the fracture would necessitate the extended position of the forearm, a position in which the head of the radius will have a greater tendency again to slip forwards.

The orbicular ligament (which is necessarily torn in this dislocation) has contracted adhesions on both sides to the neck of the displaced radius. This is seen on the inner side in the woodcut, and accounts in a great measure for the permanent loss of power in pronating and supinating the hand in such cases.

Dislocation of the Head of the Radius forwards.—Reduction ten days after the occurrence of the injury.

J. F., a girl of six years of age, was brought to me September 30, 1853, on account of an injury of the right arm, which she had received ten days previously, by a fall whilst playing with her companions. A surgeon who saw her shortly afterwards, misled by the bent appearance of the forearm, had looked on the injury as a partial fracture of the radius, and used some force to remedy the distortion. Finding, however, that the deformity remained undiminished, he requested me to examine the arm. The signs of the injury were then very distinct; the rounding of the muscular part of the forearm, the hollow beneath the external condyle of the humerus, the prominence formed by the dislocated head of the bone in front of the condyles, and its rolling under the thumb placed over it when the radius was rotated. The diagnostic mark, however, specially mentioned by Sir A. Cooper, viz., the sudden check to the motion of flexion, was not very conspicuous; the forearm could not be bent to less than a right angle on the arm; but the sudden stop to the motion was by no means so distinctly marked as I have seen it in the same dislocation in the adult.

---

[1] Dislocations and Fractures of the Joints, ed. by Mr Bransby Cooper, p. 456.

After attempting, without success, to reduce the dislocation without the use of chloroform, a full dose of the anæsthetic was administered. Extension was then made from the hand, the elbow being flexed, and counter-extension being made from the upper arm by an assistant. The head of the bone, under the pressure of the thumb, slipped readily into its place. A thick compress was then placed in front of the joint, and firmly secured by a bandage, the elbow being flexed at a right angle.

On removing the compress and bandage three days afterwards, I found the dislocation reproduced exactly as at first. It was easily reduced in the same manner as before without the use of chloroform; but I now flexed the forearm to the full extent on the arm, and secured it in this position by a bandage. After a fortnight I allowed the forearm to be gradually extended, and, at the end of five weeks, the child had regained the full use of the arm.

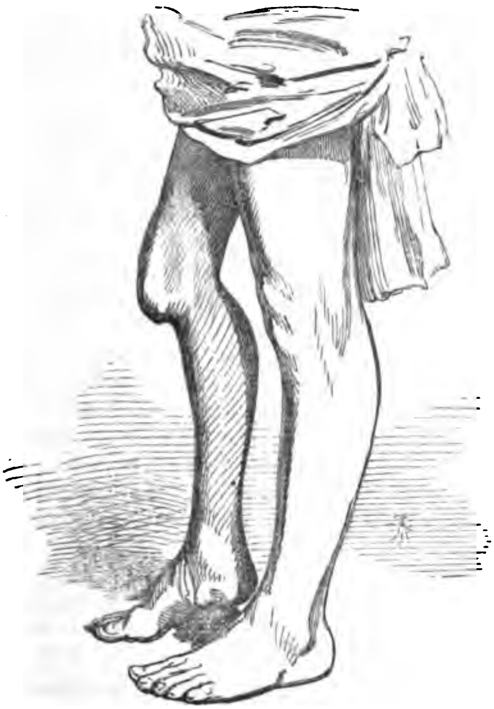
The rupture of the orbicular ligament, which must always take place in this dislocation, probably accounts for the liability to displacement after the dislocation has been reduced.

The most effectual means, I think, to prevent the recurrence of the displacement, is to flex the forearm, as in the above case, to the full extent on the arm, and to fix it in this position by a bandage. The joint being then fixed, the head of the radius cannot slip forwards, as it is prevented from doing so by the contact of the anterior surface of the condyles of the humerus.

Rupture of Anchylosis by forcible extension.

John Reid, aged eighteen, a tailor, was admitted under my care in the hospital, August 31, 1853, on account of sinuses behind the right knee, the result of an abscess which had formed about six weeks previously. The shape of the knee was very remarkable, the joint having been completely dislocated about ten years before. The dislocation, it appeared, had been effected by an irregular practitioner, who had violently extended the limb from the acute angle at which it had been fixed by anchylosis, the result of disease of the joint in early life.

The history given by the patient was as follows:—He received some slight injury of the knee when between three and four years of age; the result of this was "white swelling," from which he was confined to bed for four years, and was only able to move about with crutches afterwards The knee was anchylosed at an acute angle with the thigh. Some time afterwards (about five years after the commencement of the disease), he was put under the care of a bone-setter, who straightened the limb by sudden and violent extension. He does not remember the exact mode in which the force was applied, but he knows that the limb was immediately brought into the position which it now holds, that the operation was accompanied by great suffering, and that he nearly died from the profuse suppuration and



extensive superficial ulceration which followed. The limb was kept upon a splint, and in about six months his health was sufficiently restored to allow him to move about again on crutches. By degrees he became able to lay aside these artificial supports, and in about a year he was able to walk stoutly without even the aid of a stick. From that time he remained quite well, and able to walk several miles a day without difficulty, till about eleven months ago, when the knee was by accident forcibly bent under him, by which the integuments in front of the joint were extensively lacerated. The wound healed slowly, and before it was entirely cicatrized, the abscess already mentioned formed in the ham.

After about six weeks' residence in the hospital, the sinuses were closed, his general health was much improved, and the limb regained much of its former strength and usefulness. The preceding sketch of the limb, as compared with its fellow, was taken after he had again been walking about for a month or six weeks.

The condyles of the thigh-bone lie entirely in front of the head of the tibia, which projects in the ham. Osseous anchylosis has taken place. The leg and foot are much smaller than the other, but this is partly accounted for by their having been so little used for the last eleven months. The limb, though two inches shorter than the other, and though it looks shrunk, is very much larger than that of a child of eight years of age, the period at which it was forcibly extended, shewing that it has grown considerably since that period. Small and deformed as the limb is, however, it serves admirably the purposes of support and progression, and is much more useful than any artificial limb could have been had amputation been performed.[1]

But, although the result in this case has been successful, no one, I think, will have any doubt as to the means by which it was accomplished having been quite unjustifiable. My object in recording it is to shew under what adverse circumstances, both as regards the limb and the state of constitution, a limb *may* be restored to usefulness, and a recovery effected; and that in a sound constitution, and in deformity of less marked character than in the above case, the breaking up of an anchylosed joint may be undertaken with a reasonable prospect of success.

---

[1] This patient lay in the hospital in a bed next a patient who was convalescing after the operation of excision of the knee-joint. The straight position and almost natural appearance of the limb on which excision had been performed, contrasted remarkably with the appearance produced by the dislocated bone. Surely, if complete or even partial anchylosis can be obtained after excision of the knee-joint, the limb may be expected to prove as useful, and much more so, than the limb here figured.

Amputation at the Hip-Joint.

James O'Hara, aged eighteen, was admitted into the hospital under my care, January 3, 1853, with fracture of the right thigh-bone a little below its middle. He had suffered for more than seven years from necrosis of the bone, large portions of which had exfoliated from time to time, and he had left the hospital only a month previously with sinuses on each side of the thigh still discharging matter. Whilst limping along the street on New-Year's day, he was pushed over by a drunk man, and the thigh was fractured by the fall. When brought to the hospital the limb was much distorted, the upper fragment nearly projecting through the skin anteriorly, and the lad was suffering so intensely that he intreated that the limb might be amputated immediately. The fracture was adjusted, but the extension required for this only increased his suffering, and on the following day it was necessary to remove the splint, and the limb was kept in the easiest position between pillows.

On the 5th, at the urgent request of the patient, who was much exhausted by his continued suffering, I had him removed to the operating theatre, where I promised to amputate the limb, or remove any dead portions of bone, as seemed best on examination, when he was under the influence of chloroform. On introducing my finger into an opening on each side of the thigh, I found the whole shaft of the femur at the seat of fracture in a state of necrosis, and the very scanty substitute of new bone on one side of it fractured, and detached from the soft parts to the extent of three or four inches. Those of my colleagues who were present at once agreed with me as to the propriety of amputation, the extensive detachment from the soft parts of the thin shaft of new bone rendering the prospect of union hopeless. It was a more difficult matter, however, to decide at what point amputation was to be performed. The entire upper part of the thigh-bone was much thickened; and on introducing the finger into a large sinus, which existed on the back of the thigh, near the tuberosity of the ischium, the bone was felt rough and bare in the neighbourhood of the lesser trochanter, and the probe here entered a cavity in the bone.

Under these circumstances, it appeared doubtful whether division of the bone through the highest part of its shaft would suffice for the removal of the entire disease; and as the weak state of the boy rendered the speedy completion of the incisions very desirable, so as to avoid unnecessary loss of blood, it was deemed prudent not to make any exploratory division of the bone through the trochanter, but at once to disarticulate.

The operation was performed, nearly in the way recommended by M. Malgaigne, by tranfixion, and the formation of a long anterior flap; but a short posterior flap was also made, by carrying the knife a little obliquely outwards after disarticulating the head of the bone. The limb was thus severed in less than ten seconds, and the hemorrhage was so effectually controlled by my colleagues Dr Dunsmure and Mr Spence, that scarcely a teacupful of blood was lost.

The shock sustained by the system from this formidable operation was very slightly marked, a circumstance which I had previously had an opportunity of witnessing after the same operation in a similar case, and which wás plainly attributable to the full influence of chloroform during the operation. When I visited him two hours after the operation, I found him smoking his pipe, and he expressed himself as highly satisfied with the relief he had obtained by the removal of the limb.

For a week after the performance of the operation, every thing progressed favourably. Union had taken place on the eighth day in the greater part of the wound, and the suppuration was moderate. He slept and ate well, and suffered little constitutional disturbance. The bandage, however, had been slightly stained with blood for two days; and during the dressing of the wound on the 12th January, a little arterial blood flowed from the centre of the wound. The bleeding ceased immediately on the application of cold; but on the following day, profuse arterial hemorrhage suddenly occurred. This was arrested by firm pressure of the flaps, compression of the femoral artery on the brim of the pelvis having no effect in checking the flow of blood. On visiting him, I found him sunk and pallid, with a weak and rapid pulse. Not more than five or six ounces of blood had escaped externally, but the stump was distended by blood. The bleeding had apparently ceased, but it was evident that its recurrence to a very small extent would prove fatal. Ligature of the common iliac artery seemed the only probable means of arresting the hemorrhage; but, before tying this vessel, I determined to remove the mass of coagulum within the stump, and at the same time to ascertain from which part of the wound the bleeding had occurred, and whether it might not be checked by the application of a ligature to the bleeding point. Chloroform having been administered, the adhesions of the wound were separated by the finger, and, on clearing out a large mass of coagulum, the blood was seen to issue from three or four points in the posterior flap. Ligatures were applied to these vessels as quickly as possible; the wound was then closed, and the whole stump moderately compressed by the application of a bandage. The small additional loss of blood, however, thus incurred, increased the prostration. Under the free administration of stimulants, and by transfusion of fourteen ounces of blood into a vein, he rallied for a short time, but again sank, and died about two hours after the transfusion.

On examining the thigh-bone it was found expanded and rough on the surface as high as the trochanter major. Nearly the entire thickness of the shaft was in a state of necrosis for about five inches in length at the seat of fracture. The sinus below the tuberosity of the ischium led into a small cavity or abscess in the interior of the bone, in the close vicinity of the trochanter minor, and the dense walls of this cavity (as seen on a section of the bone being made) encroached on the cancellated texture of the trochanter major. The head and neck of the bone were sound.

The examination proved, I think, the propriety of the practice which had been followed. Had the saw been used, the bone must have been divided through the trochanter major; and I cannot but think that this operation is attended by a greater risk to life than the operation of disarticulation. The chief risk incurred in these operations is hemorrhage; for the vessels divided in the posterior part of the limb can be effectually compressed only after the incisions are completed, and the limb severed; and the time required for the formation of two flaps, and for the use of the saw, bears no comparison with the rapidity with which the incisions necessary for disarticulation may be effected. The soft parts cut in the two operations are nearly the same, and the greater facility of arresting the hemorrhage in the latter operation, I think, compensates for any additional risk which may be thought to be incurred by removal of the head and neck of the bone. When the disease or injury admits of division of the bone below the trochanter minor, disarticulation would of course be unjustifiable; but when the saw must be applied higher than this point, I think it is more prudent at once to remove the entire bone, than to protract the operation even during the short time occupied in the use of the saw.

Amputation at the hip-joint has been generally regarded as a desperate measure, and one from which the patient has a very small chance of recovery. Recent experience, however, has proved the operation to be attended by a less formidable amount of mortality than is generally supposed. Improved modes of operating, and a more judicious selection of cases, may so far account for the operation being in later years attended by more favourable results than formerly; but a large measure of the recent success must certainly be attributed to the employment of anæsthetics, the effects of which modify in such a marked degree the shock which followed the operation in former years.[1]

ART. IX.—*On the Inflammatory Affections of the Mucous Lining of the Larynx.* By ALEXANDER WOOD, M.D., F.R.C.P., Lecturer on Practice of Medicine.

Analysis of Contents.

I. *Introduction.*—Case of Washington, § 1; of Dr Pitcairn, § 2. Progress of our Knowledge, § 3.

II. *Anatomy and Physiology of Larynx.*—Its Functions, § 4; Variety and danger of its diseases, § 5. Mucous Membrane, its distribution, § 6. Epithelium, § 7. Basement Membrane, § 8. Fibrous Tissue, § 9. Muciparous Glands, § 10. The Secretion—Mucus, § 11. Alteration of Secretion by Disease, § 12.

III. *Pathology of Mucous Membrane.*—Inflammation, its varieties, § 13; Catarrhal, § 14; Exudative, § 15; Exanthematous, § 16. Morbid Changes, their variety of type, § 17; Vascular Injection, § 18; Increased Secretion, § 19; Arrest of Secretion, § 20; great ncrease of Secretion, § 21. Changes in Epithelium, § 22. Change in Calibre of Tube, § 23. Permanent Hypertrophy, § 24. Mucous or Cellular Polypus, § 25. Ulceration, § 26. Follicular Blennorrhœa, § 27. State of Submucous Tissue, § 28. Submucous Effusion, how produced, § 29; its nature, § 30; its seat, § 31. How Effusion is regulated, § 32. Exudation, § 33; its production, § 34; its nature, § 35; state of subjacent membrane, § 36. Varieties in Exanthematous Inflammation, § 37.

---

[1] In a statistical account of the results of amputation at the hip-joint, given by Dr Stephen Smith in the *New York Journal of Medicine*, October 1852, this contrast is shewn between the mortality of the operation in former and more recent years. Thus, "for the twelve years previously to 1840, there were twenty-nine cases reported, of which six were successful and twenty-three fatal, being a mortality of over seventy-nine per cent.; while for the twelve years subsequently to that date, as already noticed, the mortality was reduced to but fourteen per cent."

Of the last three cases in which the operation has been performed in the Edinburgh Infirmary, recovery followed in two (the first by Dr Handyside, the second by Mr Syme), whilst in the third (the present case) the ordinary dangers of the operation were past, and the wound in great part healed; and the patient sank from an accident, the occurrence of which was as probable after amputation through the trochanters, as at the hip-joint.

IV. *Forms of Inflammation*, § 38. Acute Laryngitis, § 39; Cullen, § 40; Hippocrates, § 41; Galen, Paulus Ægineta, § 42; as a distinct disease, § 43; symptoms, § 44. Mucous Rale in Chest, § 45; illustrative case, § 46; do., § 47; sources of danger, § 48; its terminations, § 49. Chronic Laryngitis, § 50. Various affections so named, § 51; Symptoms, § 52; Source of Symptoms, § 53. Symptoms of Polypi, 54. Symptoms of Ulceration, § 55. Symptoms of Follicular Disease, § 56. Symptoms of Exudative Disease, § 57. Termination of Exudative Disease, § 58.—Symptoms of Exanthematous Inflammation, § 59.

V. *Treatment of Acute*, § 60. Bloodletting, § 61. Purging, § 62. Sudorifics, § 63. Local application of cold, § 64. Allaying Spasm, § 65. Of second stage, § 66. Alkalies, § 67. Emetics, § 68. Bronchotomy, § 69. Calomel, § 70. Mercurial Bath, § 71. Of Chronic Disease, § 72. Of Follicular Disease, § 73. Of Polypi, § 74. Of Exanthematous Laryngitis, § 75. The topical treatment; its introduction; Sir C. Bell, Mr Vance, Trousseau and Belloc, Dr Horace Green, § 76. Nitrate of Silver, § 77; in Acute Laryngitis; in Croup, § 78; illustrative case, § 79; do., § 80; difficulty of application, § 81. Local application of other medicaments, Dr Scott Alison, § 82. Bronchotomy, § 83.

§ 1. At the very close of the last century, in an elegant mansion on the banks of the Potowmac, a great man lies at the point of death. The saviour of his country, and respected alike for his moral and intellectual qualities, his impending dissolution is felt as the threatening of a national calamity, and every effort which science can suggest or skill execute is put forth to save him. Alas, all is vain! In four-and-twenty hours from his first seizure, amidst fearful struggles between the power of the disease and the resistance of a naturally vigorous constitution, death at length claims its victim. The baffled physicians retire in silence, and a whole mourning country follows to his grave George Washington, the first President of the American Republic.

§ 2. Ten years later, one of the most eminent of our London physicians is seized with symptoms which, by himself and his friend Dr Baillie, are regarded as indicative of common cynanche tonsillaris; a few hours of suffering, an accession of quiet, and death closes the scene,—the most accomplished and experienced of existing physicians, and the founder of the British school of morbid anatomy, Dr Baillie, being unable to name the disease, although the sufferer himself, when no longer able to speak, wrote that his disease was croup, and should be treated as such.

§ 3. We mention these cases as marks to indicate the flow of the tide of advancing knowledge, as criteria by which we may seek to estimate what has been done for this department of practical medicine in the last fifty years. Of the minute anatomy of the various tissues which enter into the composition of the larynx, of the alterations which these tissues undergo by disease, of the effect of remedies upon them, and of the cases in which mechanical interference can be resorted to with the best prospect of success, we certainly know more than we did fifty years ago. But, leaving out of consideration the works of Porter and Ryland, which embrace the whole subject, our information is scattered through various medical periodicals, or recorded in the still less lasting form of pamphlets devoted to the consideration of some particular form of disease, or to the suggestion of some new remedy. The task we propose is to select from these what seems worthy of preservation, and to embody it in the condensed form demanded by the everyday requirements of the practical physician.

§ 4. The Larynx is the organ of two most important functions,—the one demanding for its performance a very complicated apparatus of bone and cartilage, of muscle and tendon, of ligaments and membranes ;—the other far simpler in its character, and yet more important in its results. These two functions, the production of voice and the admission of air into the lungs, are variously modified by diseases of that tube, and from those modifications are derived the most important symptoms which serve to guide in the diagnosis of its diseases.

§ 5. Each of these varied anatomical elements is, of course, liable to those diseases peculiar to its structure, in whatever part of the body it is found ; so that there is no difficulty in understanding the variety of affections to which the windpipe is liable ; nor, when we consider the offices which it performs, are we surprised at the urgency of their symptoms, the rapidity of their progress, and their too often fatal issue. These diseases, in whatever part of the tube they are situated, may be either acute or chronic, primary or secondary, structural or functional, idiopathic or traumatic. The different structures must be considered in detail.

§ 6. I. *Mucous Membrane.*—Continuous above with the lining of the pharynx, and extending downwards into the trachea and bronchi, the mucous membrane completely lines the tube of the larynx ; it is reflected over the epiglottis, whose fibro-cartilaginous structure is thus interposed between two layers. The projection into the tube of the edge of the arytenoid cartilage, and of the thyro-arytenoid ligament, causes two folds (the *chordæ vocales*), between which a pouch of the membrane is formed, denominated *sacculus laryngis*.

§ 7. The mucous membrane consists of three layers; (1*st*) the *epithelium*, the existence of which is denied by Béclard and Meckel, but which recent microscopic investigation has demonstrated, consisting of flattened cells laterally united, and serving the important purpose of eliminating the normal mucus in health and also certain abnormal products of disease.

§ 8. The epithelium serves also to protect the structure immediately subjacent—the delicate and temporary (2*d*) *basement membrane*, to which it owes its own existence, and which appears to have for its office the maintenance of the cell growth of the epithelium, which would otherwise be exhausted in the process of secretion. In performing this office the delicate and structureless basement membrane merely acts as the medium in which the nuclei, by the aid of various agencies, and chiefly by the supply of blood which they there receive, are transformed into cells.

§ 9. On this account it is essential that the structure should be well supplied with vessels to furnish the pabulum for formation, and these are found in the (3*d*) *fibrous tissue* lying in immediate contact with the basement membrane, supporting the vessels and nerves, and consisting of two kind of fibres, white and yellow.

§ 10. *Muciparous Glands* are found more or less freely studding the mucous lining of the pharynx and larynx. To Dr Green of New York the profession are indebted for first fully recognising and directing attention to the pathological importance of these glands. (*A Treatise on Diseases of the Air-Passages, &c.*, by Horace Green, M.D., New York, 1846.)

According to Dr Green the foramen cæcum at the back of the tongue is the opening of the excretory ducts of a cluster of these follicles. The lenticular papillæ of the tongue and the tonsils he regards as aggregated masses of these follicles; he traces them as very abundant in the uvula, pharynx, and œsophagus,—in the epiglottis are numerous glandulæ opening on the laryngeal surface; one of these of large size, imbedded in a mass of fat situated between the epiglottis and os hyoides, is named the epiglottic gland, and discharges its secretion through foramina opening on the laryngeal surface of the epiglottis. In the thickness of the superior vocal chords, and in the folds of mucous membrane in front of the arytenoid cartilage, there are also glands. The follicles of the trachea are also numerous, exhibiting the appearance of small flattened ovoid cells situated between the fibrous and muscular layers of the membranous portion of the trachea.

§ 11. *The Natural Secretion,—Mucus.*—From the mucous membrane of the air-passages there is secreted in health a small quantity of a bland transparent viscid matter, containing numerous air-bubbles which, reducing its specific gravity, cause it to float in water. It has an alkaline reaction, and is coagulated by heat and the mineral acids. Examined by the microscope it is found, according to the researches of Gruby, to consist of an amorphous viscid fluid, containing globules from two to four times the size of blood discs, presenting a clear dark outline, having very thin walls, and containing granules. It also exhibits particles of the superficial layer of the basement membrane arising from the non-development of some of its nuclei, and also shreds of cell-walls. Chemically, it is found to consist of organic matter, salts of calcium, sodium, and potassium, and water.

§ 12. This secretion becomes much altered and modified by disease. While the healthy progress of nutrition is carried forward the plasma required for the maintenance of the cell growth is furnished in due proportion by the capillaries ramifying below it, but when that plasma is poured out in excess, or of an abnormal kind, various secretory and structural changes are induced, and the state called inflammation is said to exist. To the morbid conditions included under that head it is proposed now to direct attention.

§ 13. *Inflammation* in this as in other mucous membranes presents three varieties.

§ 14. 1st, *Catarrhal Inflammation*, the slightest and most common form, which may be either acute or chronic, and which in that situation gives rise to several secondary changes of great importance.

§ 15. 2d, *Exudative Inflammation*, which, when seated in the larynx and trachea, constitutes a disease of signal importance and signal danger.

§ 16. 3d, *Exanthematous Inflammation*, of great and often fatal consequence, as complicating other diseases. Each of these pathological states merit and must receive separate consideration.

§ 17. *Pathological Changes in Inflammation of Mucous Membrane.*—When the varieties thus enumerated are viewed in the light of general pathology, it will be seen that these are not so much separate and distinct diseases as varieties of the same affection. In fact, the exudative and exanthematous varieties often run through the stages proper to the catarrhal.

The varieties seem to us to indicate differences in intensity, rather than differences in nature.

§ 18. (*a*) *Vascular Injection*, which may be confined to the minuter vessels, or may distend the whole, producing (*b*) *redness*, and generally (*c*) *cloudiness* of the membrane.

§ 19. The first effect of the inordinate vascular supply to the secreting surface is to stimulate the vital action of the part, so that " at the commencement of the inflammation the secretion exceeds the natural quantity, and is watery;" and thus (*d*) *increase in quantity and altered quality of secretion* is an attendant of the earlier stage.

§ 20. This state, however, continues but a short time, for sooner or later, what with the pressure of the blood forward by the over-excited heart, and the resistance backward of the tonicity of the vessels, a gorged state ensues, during which the transudation of fluid, and of course all (*e*)*secretion, is arrested.*

§ 21. At a later stage, again, the tonicity of the capillaries being overcome, the vessels yield to the vis-a-tergo and (*f*) *a great increase of secretion* is the consequence.

§ 22. During the period when the secretion is arrested, the epithelial surface is of course deprived of all nutrition, and of all lubrication; in consequence of this its structure is affected, (*g*) *the membrane becomes friable*, and readily separates from the subjacent structures.

§ 23. This state of engorgement of course takes place at the expense of the area of the tube, so that as a necessary consequence, even of this first stage, (*h*) *the calibre of the tube is diminished.*

§ 24. *Chronic Catarrhal Inflammation* is also met with not unfrequently, the result being more permanent change of the mucous membrane, especially (*i*) *permanent hypertrophy*, leaving the papillæ tumefied and swollen, and rendering the whole membrane tough. In these cases there is also generally an increase of secretion, and often apparent patches of abrasion on the surface. Hypertrophy of the mucous membrane is induced from submucous effusion in the manner which has already been described. " The hypertrophied membrane," says Rokitansky, " is pale, or more commonly of a rusty brown or slate-gray, and, after a time, of a dark blue colour; it is thick, compact, and firm. When it is uniform in thickness its surface is smooth; sometimes from the great increase in the size of its papillæ and follicles it is warty and rugged; and lastly, even various duplicatures and prolongations may be formed upon it. The last-mentioned two inequalities of the membrane are permanent immoveable folds of membrane; they constitute the subject of the next paragraph.

§ 25. *Mucous, Cellular, or Vesicular Polypus.* The manner of formation of these tumours on mucous membranes in general is well described by the same author (*Path. Anat.*, Sydenham Society Translation, vol. iii., p. 52). Partial hypertrophy of the mucous membrane and submucous tissues in a round circumscribed spot, dropping down by its own weight, containing submucous tissue and dilated capillary vessels. They are of course most common in bulky and weak membranes, having many follicles, and liable to catarrhal inflammation. The Schneiderian membrane, and that of the rectum and uterus are particularly liable to be their seat.

The occasional occurrence of polypi in the larynx seems scarcely to have been known to the profession until the publication by Professor Ehrmann of Strasburg, of a monograph on the subject (*Histoire des Polypes du Larynx*, Strasbourg, 1850). Two cases are recorded by Lieutaud in his *Historia Anatomico-Medica*, published in 1767. Bichat records three cases, two of which he observed himself, and the third was communicated to him (*Desault, Œuvres Chirurgicales*, Paris, 1813). Previously to publishing his interesting monograph, Professor Ehrmann, by researches among medical authors, and by an extensive correspondence with living medical authorities, collected the history of all the cases of laryngeal polypus known to have occurred up to the time when he wrote. Dr Horace Green of New York has also published on the subject (*On the Surgical Treatment of Polypi of the Larynx and Œdema of the Glottis*, New York, 1852). He has extracted much that is valuable from the work of Ehrmann, and has given some interesting cases of his own, and others gleaned from the American medical periodicals. Reference is also made to this morbid state by Ryland, and Trousseau and Belloc. Dr Watson, in his Lectures, narrates a case where warty excrescences were developed in the larynx of a boy of eleven years of age, apparent consequently on hooping-cough.

§ 26. *Ulceration.*—One of the effects of catarrhal inflammation of a mucous membrane is to cause desquamation of the epithelium, leaving denuded patches: these denuded patches frequently secrete pus, and not seldom cause ulceration of the basement membrane itself. These erosions of the mucous epithelium, often mistaken for ulcers, are particularly described by Trousseau and Belloc (*Traité Pratique de la Phthisie Laryngée*, Paris, 1837). According to these authors, if examined under water, they will be found to present a villous surface, unusual in the natural state of the mucous membrane. But mucous membranes are liable to ulcerations of various kinds. Any part of the membrane may be attacked by it. When it succeeds laryngitis, its most usual seat is in the lips of the glottis and upper part of the larynx; when associated with phthisis pulmonalis, it is generally met with in the under surface of the epiglottis, the covering of the cricoid cartilage, and vocal chords, and the posterior part of the trachea; while venereal ulceration, spreading as it usually does from the fauces or pharynx, most commonly occupies the upper part of the larynx.

M. Louis presents us with an accurate summary of the state of the epiglottis, larynx, and trachea, in cases terminating fatally from other diseases than phthisis. The general result would appear to be that exclusive of syphilis, phthisis pulmonalis is the only disease with which ulcerations of the larynx are found associated (*Cowan's Translation*, p. 35). These ulcers sometimes commence upon the surface, proceeding in the manner which has been described from a patch which has been denuded of epithelium, sometimes deep in the parenchyma of the membrane. They are often seated in the follicles, but this variety will fall to be considered under the next head. Of all the forms which these ulcers assume, the most important, as described by Mr Porter, would appear to be "a broad flat ulcer with uneven edges, covered with a bright yellow slough, and disposed to spread rapidly from the arch of the palate or the pharynx down to the larynx." They rarely heal spontaneously; but the patient generally dies from the depraved and cachectic state to which they owe their origin, and which is often connected with a scrofulous or venereal taint. A case is mentioned by Andral, in which the thyroid cartilage was perforated by an ulcer (*Clin. Med.*, tom. ii., p. 204). Very often there are chaps and fissures passing deeply into the substance of the subjacent cartilages, portions of which are removed. (*Porter.*) There can be no question that these ulcers occasionally heal, of which Porter gives several interesting cases. Both he and Ryland are of opinion, that they never heal by granulations, but only by a contraction of the edges of the sore. When healed they exhibit peculiar wrinkled stellated cicatrices.

§ 27. *Follicular Blennorrhœa.*—For our knowledge of this important affection we are especially indebted to Dr Green of New York. The disease of the mucous follicles may be either primary and uncomplicated, and confined to the fauces and pharyngo-laryngeal membrane; or it may accompany or succeed other affections of the air-passages, and be accompanied with induration of the tonsils and elongation of the uvula.

The appearances presented are, according to Dr Green, abrasion of epithelium; hypertrophy of the mucous follicles, especially of those studding the upper and posterior part of the pharyngeal membrane; induration of the follicles, which often contain a yellowish substance resembling tuberculous matter, while striæ of opaque viscid mucus hang from the veil of the palate, or coat the posterior part of the pharyngeal membrane.

In some cases the tonsils are the especial seats of the disease. It is frequently complicated with laryngitis, bronchitis, and phthisis.

Dr Green is decidedly of opinion, contrary to Louis, that tubercular matter is deposited on the surface, or in the substance of the larynx, trachea, and epiglottis.

Ulceration often succeeds to long persistent irritation. When it begins, the engorged follicle presents an ash-coloured point surrounded by an inflamed base, with red and slightly elevated edges. In their early stages small and superficial, these ulcers may ultimately spread so as to destroy the whole tissues.

Ulcerations often occur on the epiglottis or its inferior margin, which are apt to escape observation, and to keep up the irritation after the other parts are restored to their normal condition. To detect them the whole tongue must be drawn forward with a curved spatula.

Ulcerations on the epiglottis have a tendency to alter the form of that fibro-cartilage. When the ulcerations are on the border and laryngeal face, the organ loses its crescentic form and appears flattened, and its border may frequently be seen serrated with erosions. When the epiglottic gland becomes the seat of ulceration, the epiglottis will assume the erect form, and its crescentic shape be considerably increased; and when the glandulæ of the ventricles and those around the chordæ vocales are affected, the lateral edges are often rolled towards each other, so that the organ presents a tubular form. In a case observed by Dr Stokes of double perforating ulcers of the ventricles, the epiglottis presented what he has denominated the leaf-like expansion, being thinned and singularly elongated, so as to present the appearance of a battledore.

The inflammation may be limited to the follicles; and when their ulceration is accompanied with a profuse secretion of mucus, it receives the name of (*l*) *follicular blennorrhœa.*

§ 28. In most of these cases the (*m*) *submucous tissues become affected*, being hypertrophied and unnaturally irritable, or occasionally dilated and paralyzed.

§ 29. *Submucous Effusion.*—As long as the epithelium and basement membrane alone are affected, we have no submucous effusion, and the turgid vessels relieve themselves by pouring out an excess of the natural secretion with epithelial desquamation. If, however, the congestion be greater, or if it occur along with a deficiency of vital tonicity, from the absence of which exudation is not so long opposed, then the pressure of the blood overcomes the contractility of the vessels, and exudation both on the epithelial surface and into the submucous tissue takes place. Thus, as in erysipelas, we have a variety having its exclusive seat in the skin, and another where the subcutaneous cellular tissue is affected; so, in this disease, we may have appearances limited to the mucous membrane, such as have already been described, or these accompanied with changes in the submucous tissue. Cruveilhier (*Dict. de Med. et de Chir. Laryngite*) proposes on this distinction to found a division of laryngitis into two varieties; and more recently M. Sestier (*Archives Gener.* t. 83) objects to the names *angine œdèmateuse* and *œdème de la glotte* altogether, and proposes to substitute for them *angine infiltro-laryngée*, on the ground that it indicates that in the disease so named the respiration is affected, that the obstacle is seated in the larynx, and that the nature of the obstacle is submucous effusion.

§ 30. On the subject of the *nature of the effusion*, M. Sestier has entered very fully. Sometimes, he says, it consists of pure serum, sometimes of coagulable lymph, sometimes of serum and pus, and occasionally even of pure pus. Effusion, however, is not confined to the catarrhal inflammation, it also complicates the exudative and exanthematous.

§ 31. "Its seat," says Rokitansky (*Path. Anat.*), "is the submucous tissue. It generally forms a pale yellowish or grayish translucent or tense, or a flabby, moveable swelling, not distinctly circumscribed, over which the upper layer of the mucous membrane is stretched. When it is considerable, it extends through the whole thickness of the mucous membrane also, which then loses the character of its structure, and may be torn with the slightest effort."

§ 32. The description of Sestier is certainly more correct than the more general one of Rokitansky; but there is something interesting in tracing the manner in which the effusions of the various products which he describes is regulated. The exudation consists, in the first place, of water holding in solution fibrine, albumen, and various salts. Once removed from the vessels, the fibrine coagulates, forming a plasma from which cells are developed. These cells are either transient, exhibiting a process of cell development and cell decay; or persistent, when they produce permanent hypertrophy of the membrane. When, however, the inflammation is so intense as to overpower with nutritive elements, then, in the rapid development of cells, they are supplied with an excess of fibrine, and quickly undergo adipocerous degeneration, being converted into pus. (Black on *Pathology of Bronchio-Pulmon. Membrane.*) Hence, when the submucous effusion exhibits a purulent character, we are justified in concluding that the inflammation was of an intense character. The water holding the salts in solution is reabsorbed. It is evident that the prognostic importance of this complication will depend very much on its seat, being of more serious consequence when it occurs at a narrow orifice, from the consequent danger of occlusion, as at the *rima glottidis*, constituting *œdema of the glottis.*

§ 33. *Exudation* is a very common attendant of mucous inflammation when intense.

§ 34. In these cases, the epithelium would appear to be thrown off from the entire surface affected; the basement membrane is thus denuded, and plastic matter is exuded from its surface.

§ 35. Formerly the nature of the exudation occasioned much controversy. Home thought it was morbid mucus thickened; Cheyne regarded it as puriform; Rumsey compared it to the buffy coat of the blood; while the chemical investigations of Schwilgué, Maunoir, Peschier, and Jurine, served to shew that its chief constituent was albumen in various proportions. Microscopical search has to a great extent resolved the difficulty, and it is found to consist of exudation-cells, mucus corpuscles, a few blood-discs, and a peculiar substance not unlike imperfectly-developed mucous membrane. According to the description of Rokitansky, it "varies in consistence from that of cream to the toughness of leather, and is grayish-white, or yellowish, or fibrinous: it sometimes covers the membrane at a few isolated spots, and sometimes forms a more extensive film over it like hoar-frost; occasionally it invests the membrane like a layer of gauze; while in some cases it constitutes a membranous, and very often a tubular lining for the mucous surface." At first it is adherent to the mucous membrane, but at a subsequent period its attachment is loosened by the effusion below it of a viscid mucopurulent fluid, a circumstance long ago noticed by Francis Home.

§ 36. The subjacent membrane is always more or less inflamed, red, villous, and swollen; and its surface often covered by vascular granulations.

M. Brettoneau and a few of his ardent admirers have attempted to prove that there is something specific in the nature of croup. M. Billard, in an elaborate paper, has given a *résumé* of the whole subject (*Archives Générales*, Decembre 1826). But the recent progress of pathology has cleared away all doubt, and makes it evident that exudation with epithelial desquamation occurring in the manner which has already been described, leaves certain surfaces denuded. In these the exudation transuding through the basement membrane coagulates, and in it cell growth commences, and in this way the false membrane is formed. As, however, it cannot enter into structural relation with the surrounding tissues, it becomes cast off as inanimate matter,—a change which would appear to be accomplished by the effusion between it and the membrane of pus cells, which speedily effect its detachment. Albers and Bricheteau think that this membrane can become organized, but there seems no evidence to support their opinions. The membrane varies in the extent of surface which it covers. Brettoneau (*Traité de la Diptherite*, §§ 156, 158, 168) and Bricheteau (*Précis du Croup*) narrate cases where it extended from the epiglottis to the extremity of the bronchial ramification. Laennec thinks it is generally in the trachea, and seldom extends into the larynx; while Dr Thomas Davies (as quoted by Ryland, p. 134) remarks, that of six preparations on the table before him, nearly every one presents the false membrane on the inner surface of the epiglottis. M. Fauvel (*Reserch. sur la Bronchite capillaire*), after careful examination, has arrived at the conclusion that croup and capillary bronchitis are identical in their nature, and differ only in their seat. This identity is recognised and fully treated of by Dr Badham (*On Inflam. Affect. of Muc. Membranes*, 1814); and the exudation, as occurring in the delicate bronchial ramifications, is described under the name of *bronchial polypus* by several of the older authors, as Michaelis (*De Angina Polyposa*, 1788).

Dr Ryland is of opinion that croup was comparatively unknown till the publication of Dr Francis Home in 1765. It is true it was usually regarded as a complication of other diseases, but still we have a very good description of it by Baillou in 1576 (*Epid. et Ephem.* lib. ii., 19), by Christopher Bennet in 1656; and in the *Philosophical Transactions*, No. 495, Dr Starr describes the false membrane, as was subsequently done (1747–8) by Martin Ghisi, in his description of the Cremona epidemic.

§ 37. *Exanthematous inflammation* of the air-passages may either occur with, or succeed to the exanthematous fevers. In smallpox, for example, we may see the pustules developed on the mucous lining of the larynx, and running through a a course there *pari passu* with that which they exhibit on the integuments; or towards the close of the disease, when the eruption is disappearing from the skin, a secondary inflammatory process may be set up in the air-passages. The exanthematous process on the mucous membrane of the air-passages may assume any of the forms which exhibit on the cutaneous surface,—erythematous or erysipelatous inflammation, vesicles, papulæ, and pustules. Allusion has already been made to the concurrence of this affection with erysipelas. Measles, as is well known, especially affects the reticular tissue of the skin and the mucous membrane of the air-passages, which are generally found red as in catarrhal inflammation. Rayer has seen this disease accompanied with intense inflammation and pseudo-membranous exudation on the air-passages (*On Diseases of Skin*, Eng. Ed. pp. 147 and 163. Case 12). In scarlatina also, although the tendency is obviously more towards the mucous lining of the intestinal canal, yet nevertheless the air-passages are occasionally affected. I have elsewhere published some cases shewing the alliance between this and erysipelas (*Lon. Med. Gazette*, January 1852).

"In smallpox," observes Rayer (*op. cit.*, p. 393), "the mucous membranes present alterations which are not less remarkable than those of the skin . . . . The mucous membrane of the mouth is of a livid red, and shreds of the epithelium and a gray pseudo-membranous deposit are observed, as far as the eye can penetrate, hanging from the velum palati to the aryteno-epiglottidean ligaments. In cases of confluent variola the interior of the larynx and trachea also present unequivocal signs of inflammation; the mucous membrane is of a violet hue, with whitish or grayish points scattered over it, circular in shape, and varying from half a line to two lines in diameter, having a red central point without any epithelium, and several other spots, irregular in their shapes and of various sizes, probably consecutive to groups of coherent pustules."

§ 38. After this short sketch of the pathology of the parts of which the larynx is composed, we shall be the better prepared to consider consecutively those diseases to which it is liable. Three forms of *Acute Inflammation* are met with in this situation.

§ 39. (1.) *Acute Laryngitis.*—While, as has been already stated, we are chiefly indebted to the cases of General Washington in America, and of Dr Pitcairn and Sir John Macnamara Hayes in this country, for directing our attention specially to the acute inflammation of the larynx, a reference to some of the older writers will shew that the symptoms of the disease at least were not unknown to them, although they may have failed accurately to distinguish the affection from others of a similar kind.

§ 40. In the *First lines* of Cullen the description is meagre in the extreme, and "by uniting his account of this disease with croup, he evidently led many practitioners to think that both diseases were the same, and left to the physicians of the present day the merit of observing and drawing a more accurate distinction.[1]" A reference, however, to older authors will bear out our former assertion.

§ 41. Thus Hippocrates speaks of angina with symptoms of strangulation and no apparent affection of the fauces (*De Morb.*, lib. iii., cap. 10). Aretæus also observes (*De Morb. Acut.*, lib. i., cap. 7), "Duæ anginæ species reperiuntur. Altera cum caliditate et magna tonsillarum inflatione et extrorsum versus tumefactione; quin etiam lingua, gurgulio, et omnes adjacentes particulæ in tumorem efferuntur.

"Altera istis locis collapsis et submissis efficitur, sed interius compressio majore strangulatu efficitur: ut interna inflammatio ad cor usque pertinere videatur. Huicque celerrime occurrendum est, nam et hac celerrime ægroti rapiuntur."

§ 42. Galen also mentions an affection of the throat producing sudden suffocation, and alludes to the opinion of the Asclepiades that bronchotomy was necessary for its cure. Paulus Ægineta, too, refers to it as a disease requiring bronchotomy.

§ 43. The earliest accounts of this affection as a distinct disease are to be found in the cases by Dr Baillie (*Trans. of a Society*, vol. iii.), the report on it by Dr Farre (*Med. Chir. Trans.*, vol. iii.), by Bayle in France (*Dict. de Sc. Med. Œdème de la Glotte*), the *Memoire sur l'Œdème de la*

---

[1] Craigie's Practice, vol. i., p. 809.

Glotte by M. Valleix (*Mem. de l'Acad de Med.* t. xi.), and another by Delasiauve on the same subject (*Annales de la Chirurgie*, xii., xiv.)

§ 44. *Symptoms.*—The disease may be either confined to the larynx, or it may begin, and more commonly does so, with symptoms similar to common sore throat,—rigors followed by general fever, full and rapid pulse, great heat of skin, intense headache, extreme thirst. Locally the fauces appear inflamed, and the patient complains of sore throat and difficulty of swallowing. Subsequently, and often very suddenly, a dull uneasiness is felt about the pomum Adami, which is increased by pressure on the cricoid cartilages; if the tongue be pressed downwards and forwards the epiglottis may often be seen red and swollen like a cherry. The respiration is affected, usually in paroxysms; its sound is peculiarly harsh, as if the patient were blowing into a brazen trumpet; it becomes every hour more and more laborious, symptoms indicating impending asphyxia appear, the countenance becomes livid, the eyes protrude, the voice is gone (rather an inability to articulate at all, than what is usually termed hoarseness —*Porter*), the pulse becomes weak and intermitting, the patient feels sleepy, but if he indulges in it the sense of suffocation soon arouses him; gradually, however, coma supervenes, to be speedily followed by low muttering delirium, and death.

§ 45. There is one symptom which does not appear to have been dwelt upon by systematic authors so much as its importance deserves, especially as indicating the treatment to be pursued. In the earlier stage of the disease a stethoscopic examination determines the respiration to be weak; in fact, the harsh stridulous sound from the larynx almost supplants it; but at a more advanced period a subcrepitant ronchus may be heard usually over the whole of both lungs, and I look on its appearance as important, because after it appears there can be little hope from an operation.

Two cases may be subjoined as illustrative of this.

§ 46. Between 4 and 5 A.M. of the 11th of February 1840, I was called to see a young man, aged seventeen, a clerk in a bank in Glasgow. He had come to Edinburgh the preceding day, to see the festivities attendant on a joyful occasion (the Queen's marriage), and having been much overheated with walking about, had been exposed to a shower of rain, and had sat for some hours in his wet clothes. When I saw him he was shivering, the surface cold, face pale, pulse 100, small, slight dry cough, hoarseness. The fauces were slightly enlarged and red, and thickly coated with mucus. When I saw him again, about four hours later, the inflammatory fever was stronger, and the respiration more difficult; from that time the symptoms went on increasing till 2 P.M., when I determined to have recourse to bronchotomy. Some delay occurred in getting a surgeon, but about 4 P.M. he was seen, with me, by Dr Duncan. On applying the stethoscope to the chest, there was universal subcrepitant ronchus, under which circumstances it was deemed advisable not to proceed with the operation. He died at 8 P.M. No *post-mortem* examination was permitted. This is one of the most rapid cases I ever met with.

§ 47. J. M'K., aged twenty-two, an assistant in a draper's shop, called on me on the morning of the 19th of April 1839. He complained of the usual symptoms of cynanche tonsillaris. I continued to visit him at his own house till the 22d, when I left town for a few days, consigning him to the care of a professional friend. At this time he exhibited no more than the ordinary symptoms of a severe attack of sore throat. On returning to town on the evening of the 26th, I received so bad an account of him that I immediately visited him. He was then evidently labouring under acute laryngitis, with paroxysmal suffocative fits of great violence. The crepitant ronchus was universal. He died in two hours after. On examining his body the epiglottis appeared free from disease, but there was a good deal of striated redness in the mucous lining of the trachea. The tonsils were much enlarged and congested. The preparation is now in my museum.

In some cases, however, there is no antecedent sore throat, the fauces are natural throughout, and the larynx seems to be primarily affected; these generally are the most severe, and the most rapidly fatal cases.

There seems no good reason for separating this disease from the one once denominated œdema glottidis. Viewed pathologically, they are identical, and often concurrent.

§ 48. The acute form of laryngitis which has now been described is dangerous from one cause, viz., impeded entrance to the air; and this is produced in two ways.

1*st*, From the affection of the mucous and submucous tissue, the calibre of the tube is narrowed.

2*d*, From the narrowness of the rima, a comparatively trifling effusion in it is sufficient to close it altogether.

§ 49. The disease, therefore, terminates, and that usually with rapidity, either in death or resolution, or occasionally it passes into the chronic state, which, however, often arises without being preceded by the acute affection.

§ 50. (2.) *Chronic Laryngitis.*—The utmost confusion exists among writers on this subject; for under the above name, or that of *Phthisis Laryngea*, are included all sorts of affections, provided they have two conditions in common, viz.—

1st, That they are seated in some one of the many tissues entering into the composition of the larynx.

2d, That they are, comparatively at least, slow in their progress.

§ 51. In the admirable work of Mr Porter (*On the Surgical Pathology of the Larynx and Trachea*) are classed together various affections receiving this common name:—

1. True chronic inflammation of mucous membrane.
2. Ulceration of mucous membrane.
3. Abscess in neighbourhood.
4. Disorganization of cartilages.
5. Mortification of cartilages.
6. Caries of the bones.

In this paper we have only to do with those depending on a morbid state of the mucous membrane.

§ 52. The *symptoms of Chronic Laryngitis* are a modification of those which have been found to indicate the existence of the acute form. Hoarseness, cough, tenderness of larynx, increased by pressure, and sense of constant difficulty of inspiration, increased in spasmodic fits. The general symptoms are often very similar to those of pulmonary phthisis, night sweats, hectic fever, and emaciation which has led to the disease being named *Phthisis Laryngea.*

§ 53. These symptoms usually depend on an actual state of chronic inflammation of the membrane, or its consequences, as already described.

§ 54. *Symptoms of Polypi of Larynx.*—After a very careful examination of the cases recorded, I am satisfied that as yet we have no distinctive symptoms rendering us certain of the existence of polypi during life. According to Ehrmann, the only positive sign is the discharge by expectoration of small portions of the tumour, as occurred in one of his cases and also in one of Otto's. The tumour was seen in two cases by Dr Green, and in a case seen by M. Roux the tumour was detected by the touch. In some cases the patients seem themselves to have been sensible of the movements of a foreign body in the larynx. Aphonia, either partial or complete, took place in a large proportion of the cases. Dyspnœa, progressively augmenting in severity, is also usually present. After the disease has continued some time, the patient would appear to be subject to alarming suffocative attacks threatening to destroy life. The cough which usually exists presents nothing distinctive in its character, and the alterations of the respiratory sounds are very similar to what would be produced by the presence of any foreign body in the tube.

§ 55. *Symptoms of ulceration* are those of phthisis laryngea generally.

§ 56. *Symptoms of Follicular Disease.*—In general, soon after the glands have taken on a morbid action, there is perceived in the region of the fauces an increased mucous secretion, with a sensation of something sticking in the gullet, increased by all vocal exertion. The secretion has become adhesive, often alkaline, and of an irritating character, the tones of the voice are altered; this is more marked in the evening than in the morning. The mucous membrane of the fauces appears preternaturally red, denuded of epithelium in patches, and studded over with enlarged mucous follicles, often to such an extent as to give a rough or granular appearance to the whole surface; it is also usually coated, patched, or striated with viscid tenacious mucus. Sometimes the follicles are enlarged, and contain a tubercular deposit, at other times they are ulcerated. At first there is not much cough, the irritation being rather relieved by hawking, but cough appears when the glandules of the larynx and trachea are affected; the voice is then much more altered. If the disease is limited to the tonsils, velum, and pharyngeal membrane, generally the sounds are obscured and articulation imperfect. When the ulcerations extend below the epiglottis the hoarseness is increased, and the voice loses its power. Should the ventricles and vocal chords be attacked, it is reduced to a state of complete aphonia, a harsh mutter alone remaining.

Dr Green is inclined to found something in the diagnosis between this disease and phthisis on the mental state.

In the former mental depression is generally present, while in the latter, as is well known, the spirits of the patient are buoyed up, and hope often continues bright to the last.

§ 57. *Symptoms of Exudative Disease.*—These are now so well known that it is scarcely necessary to dwell upon them. Those chiefly to be depended on for diagnosis are, (*a*) the peculiar breathing, always most difficult on inspiration, and liable to periodic exacerbations of a spasmodic character, (*b*) the single, shrill, ringing, hollow cough; (*c*) the husky, hoarse, and brassy voice; (*d*) the inflammatory fever which usually accompanies these symptoms; (*e*) the expectoration of portions of exudation, which is often present.

§ 58. That croup, when fatal, terminates by asphyxia, must be admitted, and yet the immediate cause of the asphyxia is, according to Cheyne, not the false membrane; for in all the fatal cases which he examined the false membrane by no means completely closed up the passage. It would appear that death may take place in one or other of four ways:—

1*st*, Debility of suffocation, from the impeded respiration arising from the narrowing of the passages.

2*d*, Sudden closure of the glottis, either by spasm or submucous effusion.

3*d*, Valve-like closure of the trachea, from the detachment of a piece of false membrane.

4*th*, Rapid and extensive effusion into the bronchi.

These are the dangers to which a patient labouring under croup is subject, and to prevent these occurrences should be the chief care of his attendant.

§ 59. *Symptoms of Exanthematous Inflammation.*—The tendency of certain of the exanthemes to attack the mucous membrane is too apparent to be overlooked, and in regard to one of them, erysipelas, its tendency to attack the larynx and trachea, and "to produce symptoms of dyspnœa of a singularly formidable character," have especially engaged attention, and they have received a separate notice both in the work of Porter and of Ryland. Sometimes they appear along with the cutaneous eruption, and proceed *pari passu* with it; sometimes there seems a species of metastasis, the disease disappearing on the skin when it attacks the internal tissues.

In any case the symptoms are similar to the uncomplicated disease, though of course the complications require to be attended to in treatment.

§ 60. *Treatment.*—In reviewing what has been said of the pathology of the various inflammatory affections to which the mucous membrane in this situation is liable, it will be apparent that the commencement, and, up to a certain point, the progress, of each is the same, and that it is chiefly in certain of the ultimate processes that any difference is found. The consideration of the treatment will therefore be much simplified by reviewing these changes *seriatim*, and endeavouring to deduce from the pathology a rational system of treatment.

§ 61. *Congestion of the Vessels.*—Producing by its pressure *cough*, and by its interference with the functions of the capillaries, arrest of secretion, *dryness*. The *indication* for treatment in this stage, then, is simply to unload the vessels, and thus relieve the congestion. The means found to be most effectual for accomplishing this indication are bloodletting; derivation either to some other mucous membrane or to the skin; exciting locally the tonicity of the capillaries of the affected part.

With regard to bloodletting in acute laryngitis some difference of opinion exists. *Dr Baillie* has declared that bleeding does no good in this disorder. *Mr Lawrence* asserts that "bleeding and the usual means of subduing inflammation are here found totally inefficacious." *Dr Armstrong*, "bleeding has in general less power over this than most other acute inflammations." *Dr Cheyne* cautions against bleeding to syncope, as from the imperfect arterialization of the blood its stimulant power is diminished, and were the action of the heart interrupted it might not be resumed again. *Mr Porter* states that the disease is generally seated exclusively in the submucous cellular tissue, and there being generally effusion, the bleeding cannot be expected to be useful." This remark appears to afford a key to the opinions recorded. There can be no doubt that bleeding appears to be the remedy rationally directed against the *first* stage of these disorders; but, as Dr Farre observes (*Med. Chir. Trans.*, vol. iii., p. 327), "unhappily the necessary time can rarely be obtained in this disease, in which the activity of the inflammation is expended on a small but most vital seat; for the glottis, once narrowed, may be closed by the local action even after the general powers are prostrate." There can be no doubt that in practice we find local bloodletting of more service than general in subduing the inflammation of mucous membranes, but it is also undeniable that in so severe an inflammation as this, and in one so sympathized in by the constitution at large, general bloodletting is required to relieve the tension of the vessels, and enable the local depletion to have proper effect.

§ 62. The second means of unloading the capillaries is by causing a drain elsewhere; the gastro-intestinal mucous membrane affords a ready channel for this, and the bloodletting should be followed by a purgative likely to produce copious serous evacuations.

§ 63. Every effort should at the same time be made to prevent the blood from leaving the cutaneous surface, and from being determined inwards. This will be best accomplished by warm clothing and the internal use of sudorifics; and as several of this class of remedies have a depressing power, such should by preference be selected as will serve at the same time to keep down the force of the general circulation. Lastly—to promote the tone of the capillary vessels, the free application of cold air will, as has been well shewn by Dr Black, in discussing the treatment of the first stage of bronchitis (*Monthly Journal*, February 1853), be of avail.

§ 64. "The constant breathing of cold air," as recommended by Dr Black, and the local application of cold externally as pursued in the hydropathic treatment, are rationally adapted to this stage of the disorder. One caution, however, suited to the seat of the disorder, must be made in regard to the free application of cold air.

§ 65. A great source of distress and even of danger in all cases of laryngeal disease arises from the spasmodic exacerbations to which the breathing is liable. If the fibres in the larynx retained great irritability, they would be liable to be spasmodically affected by the stimulus of cold air.

§ 66. When the disease has made further progress, when the vital tonicity of the capillaries has been overcome, when their congestion is relieved by exudation into the submucous tissue, or on the surface of the basement membrane, new indications for treatment present themselves. The pathological conditions are—

(*a*) Exhaustion of the vital tonicity of the capillaries.
(*b*) More or less denudation of epithelium.
(*c*) Submucous effusion.

To meet these some modifications of the former indications are demanded. How does nature remove exudation? Medicine affords no remedies by which it can be accomplished, and the highest reach of our art in this, as in much other curative treatment, is to be the " servants and interpreters of nature." Disintegration of the exudation by its passing through the various stages of cell-growth is the natural and indeed the only way in which such an effusion can be removed.

§ 67. The physical aids to all growth are heat and moisture, so that in this state the indications vary from those which the first required. But the liquefaction of the coagulated portion of the exudation may be promoted and its disintegration hastened by the use of solvents, of which the alkalies hold the first rank.

§ 68. At the same time emetics may be given to dislodge the effusion on the free surface.

In illustration of the beneficial effects of emetics, M. Valleix states, that in thirty-one out of fifty-three cases of croup where emetics were employed, fifteen recovered, of the remaining twenty-two only one recovered. The depression produced by the employment of some remedies of this class would contraindicate their use, from its tending to diminish the vital tonicity of the capillaries, and thereby favouring congestion and exudation. Dr Aberle recommends the sulphate of copper to be used in all cases of this kind, or where the tone of the stomach is impaired, or where antimony produces diarrhœa.[1] It is in this stage also that expectorants may be expected to be of service, by stimulating the fibre and enabling it to throw off the effusion.

---

[1] Sulphate of copper was first recommended in croup by *Hoffmann* (1821). He gave from a quarter to half a grain every two hours with great success. Dr Zimmermann recommends the same treatment (*Hufeland's Journal*, B. 2. 1835).

§ 69. The suggestion of the use of all these remedies implies the existence of sufficient time for them to reach the affected part through the current of the circulation, and perform their important offices there. When the effusion is poured out with such rapidity or to such an extent as not to give time for this, mechanical means must be adopted to prevent immediate death.

§ 70. There is one remedy regarding the power of which in the removal of exudative inflammation much difference of opinion prevails—*Calomel.*

It was first introduced to any extent in the treatment of croup by the American physicians, especially Rush, Bard, Kuhn, and Anderson, and according to their statements its effects are at once to suspend the exudative process and to detach what has been already effused.

By Redman gr. iii were given every third hour, until gr. xv had been taken, and this even to the youngest and most delicate infant. The late Professor Hamilton of this city gave to a child aged seven years cxxxiii grs. in 60 hours. Maurs gave to infants of two years old from grs. v to ℈i every hour.

After such an inordinate use of this powerful poison it is not surprising to find that a reaction has taken place; but that the late Professor Hamilton should have in the end so bitterly opposed its employment, gives too much room for the vehement exclamation of Dr Mackintosh "Is there an individual in the British empire, with an ordinary share of common sense, who having cured forty-six out of fifty cases of such a dreadful disease as croup, as Dr Hamilton alleges he has done, would not feel justified in recommending others to follow the same treatment? But this useful remedy has since been cast in the background, and Dr H. has had the extreme folly to state that the action of mercury tends, by exciting inflammation and effusion, to produce thickening of various membranes, particularly of the pleura." (*Practice of Physic*, vol. ii., p. 442) Medical opinion generally seems to be in favour of the use of calomel. *Dr Ryland* trusts to it and bloodletting exclusively. *Dr Forbes* is inclined "to intrust the case to the more durable action of calomel." *Mr Duncan* (*Dublin Journal*, Jan. 1842) places his chief reliance on emetics and calomel. *Dr Stokes*, on the other hand, regards the "mercurial treatment as insufficient and unnecessary." *Billard* (*On Diseases of Infants*) gives a good summary of the objections that have been made to its use, and is strongly in favour of its employment. This preponderance of evidence in its favour clearly warrants the assumption that mercury has been *empirically* found of use in this affection, and the same conclusion may also be arrived at on *rational* grounds. There is no doubt that the tendency of the salts of mercury taken into the system is to induce heightened vascularity and increased secretion. In cases where it affects the removal of recent fibrinous effusions, as, for example, those into the aqueous humour of the eye, it often does not commence to do so until the state called mercurialism is induced (Christison's *Dispensatory*, p. 511); the explanation appearing to be this, that mercurialization gives rise to a peculiar morbid action, attended by an exudation so copious as to cause the disintegration of that which is the product of the disease. Alkalies, were they more rapid in their action, would be invaluable from the solvent power which they exercise on the exudation corpuscles.

A last and most important indication for treatment is to heal the denuded membrane. When the morbid process ceases, or is arrested by the means indicated, the healing process commences; but it must be favoured by perfect quiet of the part, and by such topical applications as experience has shewn to be useful. These, however, will form a separate subject for consideration.

§ 71. *Dr Rockwell of New York* has succeeded in cases of croup by producing speedy mercurialization by means of a warm vapour-bath. His plan is, for a child from two to four years old, to take about one scruple of cinnabar, place it upon a red-hot brick, and cover this and the patient for a short time with a sheet or large bag; the mercurial vapour is in this way breathed and absorbed, and a few applications in from two to twenty-four hours produce the constitutional effect, and with it a relief from the croupal difficulty. (*Transactions of the Medical Society of the State of New York*, at its Annual Meeting February 1853.)

§ 72. *The treatment of Chronic Laryngitis* is similar to that of the acute,—leeches to the seat of the disease, counter-irritation, and the use of the salts of mercury. In the chronic form mercury may be given with more confidence, as the progress of the disease is not so rapid, and gives it time to manifest its constitutional effect. Dr Cheyne was in the habit of conjoining the mercurials with expectorants. In scrofulous laryngitis, for example, especially that variety which occurs in *muddlers*, he was in the habit of recommending the following prescription for ten days together. (*Cyc. Prac. Med.* Art. LARYNGITIS):—

℞ Pill hydrarg ℈j.
Pulv. conii ℈ii.
Ipecacuanhæ gr. xv.
Ammoniaci contrit. ʒss.

Divide in pill xl. Sumat ii. mane et meridie super bibendo poculum lactis asinini.

The same author also recommends change of air and caustic issues on either side of the larynx.

§ 73. *Treatment of Follicular Disease.*—This, like all the other varieties, resolves itself into general and topical, the latter of which we reserve for separate consideration. For our knowledge of this disease we are chiefly indebted to Dr Green (*On Diseases of the Air-Passages*); and though a host of servile imitators have founded their claim to fame on his discoveries, we do not find that they have added much to our knowledge either of the nature or of the treatment of the disease. Of the remedies which he employs he appears to have most confidence in the iodide of potassium. This he conjoins with the sesquioxide of iron, and various sedatives and expectorants to alleviate the cough. His favourite sedatives are belladonna, conium, hyoscyamus, and the salts of morphia. The expectorants are muriate of ammonia, Sanguinaria canadensis (Blood-root),[1] and the oil of Gualtheriacæ. He also recommends mercury in alterative doses.

§ 74. *Treatment of Polypi of the Larynx.*—To the same author, Dr Green, we are indebted for all that we know concerning the treatment of this disease, which, if not removed, proves certainly fatal. (*Polypi of the Larynx, and Œdema of the Glottis,* by Dr Green.) M. Ehrmann (*Histoire des Polypes du Larynx*) states that the removal of the tumours by laryngo-tracheotomy is the only cure. Mr Ryland shews (*On Diseases of Larynx,* p. 230) the extent of the operation required to effect this, but adds " as the consequences of polypus remaining are always mortal at some period or another, it would be right to give the patient this chance of relief from otherwise certain destruction."

Dr Green, while fully concurring in the danger of polypus, points out that in the recorded fatal examples but few of the tumours were found sessile; by far the larger proportion having been seated above the inferior vocal chords, and having been attached by pediculated roots to the lips of the glottis, the base of the epiglottis, or the sides of the ventricles or vocal ligaments. Dr Green farther points out how this peculiarity and seat of the tumour enables the surgeon to discover its existence, and by proper manipulation to remove it by torsion or excision.

---

[1] Dr Green describes the Sanguinaria as a stimulant expectorant, having slightly narcotic properties. The tincture is made by macerating 4 ʒ of the root in two pints of alcohol. Dose q^{tt}. xv.–xx. thrice a-day with q^{tt}. v.–vii. of laudanum, and the sixth of a drop of Ol. Gualtheriacæ.

§ 75. *Treatment of Exanthematous Laryngitis.*—The treatment of this form of the disease varies according to the kind,—1*st*, when the laryngeal affection complicates or succeeds the exantheme; 2*d*, those cases, especially erysipelatous, where the original disease transfers itself to the larynx, and concentrates all its virulence there. In regard to the former, the larnygeal affection is usually of a subacute character, and either ceases spontaneously with the disease, or if succeeding to it, it is to be treated on the same principle as if it had occurred idiopathically. As respects the latter, it must be borne in mind that erysipelatous inflammation has its peculiarities, and that one of these is seldom to bear depletion well (See paper by the author "On some of the more unusual complications of Scarlatina," *London Medical Gazette*, Jan. 1852); on the contrary, that species of inflammation is peculiarly amenable to the topical applications which are next to be considered.

§ 76. *The Topical Medication of the Larynx.*—The late Sir Charles Bell appears to have been the first surgeon to whom it occurred to apply nitrate of silver directly to the mucous membrane of the larynx; the case is recorded in his *Surgical Observations*, Lond. 1816. Dr Watson also narrates a case where Sir C. Bell, when consulted by him about the case of a patient in the Middlesex Hospital, labouring under acute laryngeal disease for which bronchotomy had been performed, applied a strong solution of nitrate of silver to the epiglottis and upper parts of the air-passages. (*Lond. Med. Gaz.*, vol. xxviii., p. 777; *Lectures* (1843) vol. i., p. 809.) To the same author we are indebted for the information that the late Mr Vance, who had been a naval surgeon, was frequently in the habit of thus applying caustic directly to the affected organ, and that to him we are indebted for the nautical phrase, "*swabbing*," by which it is usual to designate it. The same practice was followed by Trousseau and Belloc (*Traité Pratique de la Phthisie laryngée.*) They recommended a solution of the nitrate of silver in the proportion of two drachms, and sometimes four drachms, to the ounce of water, as a most efficacious and harmless application. They also employed the sponge attached to the bent whalebone, as still used; or threw the solution into the larynx by a small syringe, as has been again suggested by Drs Warren and Theophilus Thomson;

or as can be done most readily by the ingenious instrument contrived by Mr Coxeter. The French authors, however, do not so much recommend the passing of the sponge through the rima glottidis, as practised by Mr Vance and Sir C. Bell, as the resting of it on the epiglottis, and then expressing the fluid so that it shall enter the trachea.

But it is to Dr Horace Green of New York that the profession are indebted for the recent revival of the practice. When in London in 1838 this gentleman had a conversation with Dr Johnson the veteran editor, who, speaking of the prevalence of affections of the larynx, suggested that if proper local applications could be made below the epiglottis, no difficulty would occur in successfully treating the disease. Acting on this suggestion Dr Green, unaware that such an operation had been ever before practised, attempted the introduction of the sponge, and with complete success; and in September 1840 he brought before the New York Medical and Surgical Society the result of fifteen cases successfully treated in this way. Strange to say, however, the statements of Dr Green were scarcely credited, and few of the members believed it to be possible that he had really effected an entrance into the larynx, although Desault had insisted on the possibility and advantage of keeping a tube in the larynx, as a substitute for bronchotomy. At length, several years later, the New York Society again took up the subject, and appointed a committee to "inquire more fully into the practicability of making these topical applications," and the result has been the complete establishment of the practice, not without some danger of the weaker minds in the profession rushing into the opposite extreme, and *swabbing* to an extent that is unwarrantable.

§ 77. *Local Application in Acute Laryngitis.*—The cases in which the caustic solution was applied by Sir C. Bell were of this character. The late Dr John Scott of this city applied it to a case of acute laryngitis to which he was called by Dr Brown (*Monthly Journal*, July 1850). Dr Green, in his last publication (*Polypi of the Larynx, and Œdema of the Glottis*), gives the history of several cases which were treated by him by the local application of caustic. In a very recent work by Dr Scott Alison of London (*The Medication of the Larynx and Trachea*), we are surprised to find the following remarks:—

"In acute inflammation of the glottis I have not employed the solution of nitrate of silver, because the presence of stimulants on parts suffering from such attacks has always in my experience aggravated the disease. . . . . What a serious aggravation of the disease of the glottis we should have, were we to increase its swollen condition, and farther contract the aperture for the ingress and egress of air, already compromised to the very serious danger of the patient" (p. 6).

Now this opinion is directly opposed to the experience of Dr Green and others, and proceeds upon the assumption that nitrate of silver does in these cases act as an irritant, which we are by no means prepared to admit. Ricord has shewn that the acute stage of gonorrhœa may be cut short by the injection of a strong solution of nitrate of silver, and the pathology of that disease bears no small analogy to that of the one under consideration. Perhaps the explanation of Dr Alison's opinion is to be found in the following sentence from p. 4:—

"The solution of nitrate of silver which I employ is weaker than that which is in general use, and contains only about gr. v. to the ounce of water. With such a small proportion of silver I have obtained as good results as with a larger, and I have therefore rested content with that strength."

Dr Green, on the other hand, employed in acute cases a solution of the strength of forty-five grains to the ounce, and expressly states that one of less strength will have little effect on the diseased mucous surfaces. We may also, while on this subject, make the following extract from a "Report on Croup" made to the Medical Society of New York at its meeting in February 1853, and since published in its Transactions. The author of the report is *Morgan Snyder, M.D.*

"Several physicians have said to me (p. 134) they have tried this application, but the results did not meet their expectation, and they had no confidence in it. Upon inquiry I have ascertained that it is used in strength varying from ten grains to the ounce of water up to sixty or eighty grains, and that some use it in the solid form. It has seemed to me that the unsatisfactory results pertaining to the use of this article are to be accounted for from the mode of using it, and a want of proper attention to the effects produced by it upon the parts upon which it is applied." Dr Snyder then enforces the necessity of obtaining a perceptible effect on the mucous membrane, without destroying its surface, and also of applying the solution to the whole of the diseased surface. These remarks of Dr Snyder, however, apply chiefly to croup, in which we have abundant evidence, both in our own experience and in that recorded by others, of the benefit of the local application of nitrate of silver; as yet, however, the practice is chiefly confined to America.[1]

---

[1] While these sheets were passing through the press, I had an opportunity of employing the caustic solution in acute laryngitis with complete success. H. W.

§ 78. *Application of Nitrate of Silver in Croup.*—In the *American Journal of Medical Science*, April 1842, at p. 364, we have an interesting account by *Dr R. W. Gibbes* of the treatment of a case of croup following ulcerated sore throat. Dr Gibbes commences by stating that such cases are rarely cured. After all the usual means had failed, a saturated solution of nitrate of silver was "applied to the fauces, and far back into the pharynx, with success."

In the *Southern Medical and Surgical Journal* for June 1848, *Dr Bryan* relates a case especially valuable, because, with the exception of a few grains of calomel, the nitrate was trusted to alone. The strength used was gr. xl. to the ℥j. *Dr Townshead* narrates in the *American Journal* a case where croup was successfully treated by cauterization of the larynx.

I have tried it in four cases, and with perfect success. Two of these are especially interesting, as the cases seemed hopeless.

§ 79. Of the first I shall give the narrative of the father, a medical man, residing about thirty miles from Edinburgh.

"J. B., aged fifteen months, a stout child of his age, took measles, and passed through the different stages of the disease very favourably, and without much cough. About ten days after the commencement of the affection, and subsequent to the entire disappearance of the eruption on the skin, which had been very partial and scanty, aphthous ulceration of the buccal mucous membrane took place, which rapidly extended to the gums and tongue, involving at last the tonsils and fauces. Alarming symptoms now presented themselves; the voice completely changed, the respiration became stridulous, and the cough croupy. In fact, every symptom of acute croup was present. The usual local astringent remedies were applied, and the constitution nourished; but the respiration became more and more affected, and the voice less perceptible, until at last it cost the little sufferer evident pain to cry at all, and paroxysms of difficult breathing were becoming more frequent and violent. At this juncture I requested Dr Alexander Wood to see him, which he kindly and promptly did, and applied a solution of forty grains to the ounce of nitrate of silver, by means of a small piece of sponge on a bent probarg, direct to the affected part, through the glottis to the larynx. Instant relief was produced; for hours afterwards the respiration became much less stridulous and much easier. A weaker solution was applied at first thrice, and in three or four days twice a-day, for a week, and occasionally afterwards, whenever the symptoms seemed to require it, for a fortnight longer. Cod liver oil was given, and the chest rubbed regularly with a strong ammoniated liniment. Relief always followed the application of the remedy, immediately after the local irritation consequent on its application, which was not great, subsided; but in a few hours afterwards the breathing invariably became embarrassed, until, by the continual application of the solution, the morbid action of the parts was completely changed. The patient recovered well; but for three weeks after Dr Wood's visit he could not emit any sound, and it was six weeks ere he could emit a continuous husky cry; and even at this date—three and a half years after—his voice is frequently husky, and, on the occasion of cold, his cough is more or less croupy."

§ 80. The second was a child at Stockbridge, aged three and a half years. He had been attacked, three days before I saw him, with all the symptoms of croup, which were intelligently described by the mother; and these had gone on increasing, with temporary abatements, till the period of my visit. He was then lying on his mother's knee, perfectly pallid, though his face had been described as flushed (livid ?) shortly before; pulse very equal and steady. The breathing was very stridulous, and there was mucous rale over both lungs. I immediately introduced the sponge, soaked in a solution of nitrate of silver (ʒi. to ℥i. of water); slight cough followed, but there was no other perceptible effect. I prescribed an emetic. In three hours I saw the child again. He had vomited freely, and was much relieved. I again applied the caustic, and prescribed small doses of calomel and ipecacuan (gr. ¼ of each every second hour). From this time a decided amendment took place. The caustic was three times applied the next day, and a weaker solution (gr. xl. to ℥i.) twice on the following two days. On the fourth day he was convalescent.

§ 81. *Dr Snyder* (*op. cit.*, p. 13) says, "I have found great difficulty in some cases in applying the sponge satisfactorily to the fauces of children, and believe that, on account of their resistance, and the smallness of the passage, it is almost impossible to reach beyond the epiglottis. On this account the cases where the disease has extended considerably into the larynx usually prove fatal. In such cases I believe the principal hope of recovery rests upon the rapid induction of mercurial action in the system." In practice I have not found the difficulty so great as the above extract would lead us to expect. It is certainly more difficult to apply the sponge in children than in adults, and the size used should be much smaller.

In the treatment of the follicular and chronic forms of the disease, the application of the caustic is, if possible, still more beneficial.

§ 82. *Local application of other Medicaments.*—Nitrate of silver is not the only substance which has been applied with advantage to the lining membrane of the larynx. *Alum* and *sulphate of copper* have for long been used occasionally by insufflation; and very lately Dr Scott Alison has given his experience of the use of a variety of remedies in this way. His experience, however, seems to be chiefly in the use of *olive oil;* and he oils a husky throat as readily as we would apply the same medicament to a grating hinge, and apparently with a like effect. (*On the Medication of the Larynx and Trachea.* By S. Scott Alison, M.D. London, 1853, pp. 49.) The medicaments which he has chiefly used are olive oil, glycerine, cod-liver oil, mucilage, solutions of morphia, atropine, and conia. He also suggests the local application of expectorants as astringents, especially tonics.

§ 83. *Bronchotomy* is certainly not to be considered as a cure for this affection, but is employed as a mechanical means for preventing immediate death, and thus for giving time for medicines to have effect. The length to which this *resumé* has already extended, prevents our entering on this wide subject now; hereafter some of our surgical coadjutors may furnish us with a discussion on it. The opinion of the profession in this country seems to be inclining rather more to its performance than it did some years ago. An admirable analysis of 168 cases of acute laryngitis, in 38 of which bronchotomy was performed, is contributed by Dr Felix Sestier to the *Archives Générales* for August 1850.

2 St Colme Street, Edinburgh,
November 1853.

PART II.

CRITICAL ANALYSIS.

ART. I.—*The Science and Art of Surgery.* By JOHN ERICHSEN, Professor of Surgery in University College, and Surgeon in University College Hospital. 8vo, pp. 950.

The rapid advance of surgery, both as a science and as an art, in the last few years, has tended to throw into the shade the sterling works of the great surgeons whose authority we were accustomed not long ago to look up to with respect and implicit confidence. The admirable lectures of Cooper and of Colles, even the elementary works of Liston and of others of the same date, are already "behind the day," and are no longer in our students' hands; whilst the knowledge obtained by the rising generation of the labours of Pott, of the Bells, of Desault and Dupuytren, is gathered nearly entirely from the surgical manuals or text-books which of late years have been offered in such profusion for their choice. A good elementary work, however, bringing its information up to the progress of the day, is an essential requisite for the student of surgery; and with every respect for the admirable manuals of Syme, Miller, Pirrie, Druitt; for the works of Chelius by South, and of Cooper, of Fergusson, and others, and with a due appreciation of the great value of these volumes, we would consider a succinct and lucid exposition of the principles and practice of surgery, as far as they are established at the present day, by no means unacceptable. Our idea of such a work is, that it should be the production of a great surgeon, one whose name carries the weight of authority, whose experience warrants its being the result of observation, who has not disdained to keep himself well informed on the researches of modern pathology, and who is untrammelled by the prejudices of any particular school. Dismissing, however, such a Utopian idea of a surgical guide for the present, we gratefully acknowledge the praiseworthy labours of our more recent writers on systematic surgery, among whom our author stands the latest and not the least successful.

The work before us represents, in the shape of a systematic treatise on surgery, the substance of the lectures delivered by Mr Erichsen as Professor of Surgery in University College.

The subject is embraced in three divisions—" First Principles," " Surgical Injuries," and " Surgical Diseases ;" to the first of which four chapters are devoted, and to the second eighteen ; whilst the remaining thirty-eight chapters, comprising two-thirds of the volume, are occupied by the third division.

The consideration of diseases of the eye is omitted ; very little is said of diseases of the ear ; and those diseases only of the skin are described which come specially under the treatment of the surgeon, such as warts, lupus, &c. To these omissions we do not object. The subject of ophthalmic diseases, no doubt, forms a most important part of a complete system of surgery ; but its range is so extensive that little more than a superficial sketch can be given in such a work without the addition of a separate volume. We think, on the whole, that the less the teacher of systematic surgery enters on the subject of ophthalmic medicine the better ; for the hasty sketch, of which his limits will alone allow, is apt to leave the student satisfied with the very imperfect knowledge thus obtained of a subject which is of so much importance to him in practice. We are scarcely prepared, however, to extend these observations to diseases of the appendages of the eye, such as fistula lachrymalis, encysted tumours of the eyelids, strabismus, &c., which find no place in Mr Erichsen's book. The subject of diseases of the skin, we think, is wisely omitted, as being scarcely within the special domain of surgery ; and the pathology of the internal ear, of which we are only now beginning to have some more accurate knowledge, is prudently avoided.

Before speaking of the truly surgical part of the volume, let us look into the " First Principles" which Mr Erichsen puts forth as the groundwork of surgical pathology.

Practical writers have always experienced, and some of them have freely expressed, the difficulty of reconciling the highest generalizations of pathological science with the complex and infinitely varying phenomena submitted to their daily observation. Hence the constant use of terms which, though not representing the ultimate results of thought as applied to disease, nor even strictly definable from a pathological point of view, are found to embody, with sufficient accuracy, the ideas of the great majority of practitioners, and have continued accordingly to occupy a place in the systems, doctrines, and practice of many successive ages. *Inflammation, scrofula, malignant disease,* in surgery; *fever, rheumatism*. in medicine, are some of these words, which (following the idea of a great thinker) we might call the " middle terms" between pathology and practice. They are not indeed calculated, nor were they intended, to meet the wants of the theorist, and to baffle the criticism of the logician ; the ideas to which they refer cannot be demonstrated, or defined, or numbered ; but they are recognised by common consent as corresponding to those phases of disease which meet us at every turn in nature, and therefore form the legitimate basis of our practice, as well as the most correct starting-points for our philosophical analysis. We do not hesitate to state it as our belief that the practical writer would in general do well to keep within the bounds of these recognised and useful generalizations ; and that he is in great danger, if he attempts to go further, of involving himself and his reader in a mist of debateable and hypothetical propositions, without any commensurate gain in the form of discoveries bearing on practice.

The reader will, we trust, not suspect us of any desire to set a limit to speculations in general pathology, or to deny their utility in advancing surgical knowledge ; on the contrary, we value highly the exercise of the human intellect in reference to the doubtful problems of science. We desire, however, to see hypothetical considerations and experiments, from which no *immediate* practical results can be safely deduced, excluded to a great extent from the pages of elementary practical text-books ; and we are sorry to observe an increasing tendency in surgical works of this kind to overwhelm the memory, and confuse the judgment of the student by a display of information as to modern pathology, which we have not unfrequently observed to rest upon rather a shallow knowledge of its principles.

These reflections have been suggested by the first chapter of Mr Erichsen's treatise, a considerable portion of which might, we think, have been with advantage omitted ; or if inserted, should have been elaborated with more care, in the spirit of modern pathology. We are told at the commencement that this first chapter consists of " first principles ;" now what are we to think of the first principles of a surgical teacher who takes his start from such a pathological anachronism as we meet with in the opening sentence, and indeed throughout this chapter ?

> " *Increased vascular action* lies at the bottom of all surgical processes; no important surgical action taking place without it. No process by which the separation of dead parts is effected, or by which the repair of wounds and ulcers is carried out, can occur without an increased activity of the vessels of the parts concerned. Every tissue is susceptible of it. . . . . Increased vascular action is of three kinds:—1*st*, Congestion ; 2*d*, Determination ; 3*d*, Inflammation."—(P. 1.)

In speaking of ulceration and gangrene, a little farther on, Mr Erichsen himself endeavours to shew that these processes take place in some instances independently of " increased vascular action." But we shall not insist upon this inconsistency ; we demand of Mr Erichsen if he can adduce a single clear and unequivocal proof that *increased* vascular action lies at the bottom of *any* surgical process. We have no doubt that a well-informed pathologist of the present day would answer in the negative, and would inform him that the " *active* dilatation " of the vessels is nothing but a hypothesis, and in all probability an erroneous hypothesis, of the Hunterian pathology. In fact the vessels are now admitted to play a very secondary part in every one of the processes to which Mr Erichsen refers ; and many of the non-vascular tissues, as well as many of the lower animals in which no vessels exist, are eminently prone to all those " surgical processes" which are here described as the consequence of " increased vascular action ;" so that the fundamental proposition, *the* " first principle " of Mr Erichsen's pathology is an unquestionable and an irretrievable blunder.

The division of " vascular action" into three kinds suggests other pathological pitfalls, and forms a source of abundant confusion for the student. " Increased vascular action" is surely a simple enough idea; how it should spread itself into so many ramifications of disease, and assume, Proteus-like, so many forms, having such essentially different and even opposite pathological consequences, is a query that will doubtless stagger the faith of many a clever young logician. Let us hear what is said *ex cathedra* :—

> " Congestion . . . . is apt to occasion serious structural changes, and to run into inflammation in tissues affected by it. Congestion is *always* a passive and mechanical condition—(What ! although apt to *occasion* serious structural changes !)—hence the term active congestion should not be employed. Indeed, what has been described as

> active congestion is in reality a variety of the inflammatory process." (What, then, is the congestion described above as "apt to run into inflammation?")
>
> "Congestion is a true hyperæmia (and hyperæmia is a true congestion, for both words mean exactly the same thing.) . . . . The symptoms of congestion are well marked, &c. &c. . . . The effects of congestion are of much surgical importance. . . . The causes of congestion, always mechanical, may be divided into two great classes, which we find often conjoined: 1*st*, Those causes that act by obstructing the return of blood through the veins; 2*d*, Those that act by *enfeebling the walls of the capillaries and veins*, so that they are no longer able to withstand the outward pressure of the contained blood."

We should like to direct Mr Erichsen's particular attention to this last clause, and to ask him what becomes of congestion as one of the "three distinct kinds of *increased* vascular *action*," seeing that it is "always passive," and often the result of mechanical pressure upon *enfeebled vessels.*

We have perused attentively the section on "Determination," as one of the "three distinct kinds;" but we confess to a very misty and imperfect perception of the pathological relations and peculiarities of this process. We learn that it is sometimes a disease, and sometimes not a disease; that it "is often called '*increased action,*' *consequently* resembles congestion" (which we have seen above to be an *enfeebled* or *diminished action*), "in the blood being in excess, but differs from it in every other respect." We also learn that "determination of blood may result in true inflammation;" and that "the treatment of determination of blood is nearly identical with that of inflammation" (from which we should judge that the process itself is not very different). Perhaps the following sentence may throw some further light upon the distinctions of the "three kinds of increased vascular action:"—

> "We have seen that in congestion the quantity of blood is increased, and its rate of action is lessened: in determination we have everything augmented; there is an increased size of the vessels, and an increased quantity of blood within them, circulating through them with increased velocity. In inflammation we have a combination of these conditions; we have an increased size of the vessels, an increase in the quantity and rapidity of the motion of the blood; but, conjoined with this, we have a tendency to its arrest, to its stagnation at points."

We have a tendency to arrest and stagnation of our ideas at this point; between "increased vascular action," mechanical obstruction, *enfeebled* walls of capillaries and veins, increased motion and diminished motion, and motion both increased and diminished, and motion arrested altogether, we confess to something like a loss of mental equilibrium, or at least a very complete state of mystification as to what "increased vascular action" may exactly mean, or in what particular way it manifests itself in each of the "three kinds" before referred to. We hold the above perhaps to be unequalled as a puzzle for a student. We have indeed a recollection of something similar in the "Principles of Medicine" of Dr Williams, and particularly of a certain comically obscure "definition" of the term *inflammation*, which we always thought had much better have remained for ever undefined than be locked up in such a formula. Here it is, too clearly the "*fons et origo malorum*," as regards Mr Erichsen's book:—

			RESULTS.
HYPERÆMIA. Excess of Blood.	General—Plethora	With motion increased—Sthenic.	Hemorrhage, Flux, Dropsy.
		... ... diminished—Asthenic.	
	Local	With motion diminished—Congestion.	
		... ... increased—Determination of blood.	
		... ... partly increased ... ... partly diminished	Inflammation.

We think it right thus to lay bare one of the sources from which our most earnest students derive their tendency to the pursuit of verbal distinctions instead of true science, and by which it appears our best practical teachers are now beginning to be led out of their way. It is useful to direct attention to an *ignis fatuus*, not only because of its provoking tendencies as regards the individual, but because it indicates a state of the ground below dangerous, or at least profitless, to the community, and requiring thorough drainage and careful cultivation. The state of mind of our profession, in which such an *imbroglio* as that above quoted can be accepted as an exposition of the most difficult doctrines of pathology, may well cause a little alarm to one who has been at the pains of studying these doctrines for himself; and it is almost a comfort to feel that those who learn by rote such formulæ as these, and even those who expound them to others, do not allow them to have much influence on their own minds. We are little surprised at the idea that pathology is barren of results in practice, if this be pathology; but the simple truth is that it is nonsense, and therefore necessarily barren of all results whatever, except confusion. The ideas which made the great and practical mind of Hunter stagger and reel under their weight, so that his utterances became like the leaves of the Cumæan sybil to those who have followed him, are not to be disposed of after this summary fashion. The phantom of "increased vascular action" which occurred to him as the most profound expression of an imperfect physiology, cannot be now blazoned forth on the front of our text-books, without shewing a total disregard of those "first principles" which they profess to expound. To those who wish to see the subjects to which we refer treated in the spirit of modern science, we recommend (without professing to adopt all his views) a perusal of the excellent and thoughtful paper of Dr Cappie in our present number; and to Mr Erichsen we recommend, in future editions, to be strong in "middle terms," and avoid getting beyond his depth in "first principles;" and above all, to eschew the "principles" of Dr Williams, and adhere to his own, as we find them a little further on.

Chapter II., entitled "General Considerations on Operations," contains some practically good information on such subjects as the duties of the operating surgeon, the treatment preparatory and subsequently to an operation, the administration of chloroform, the causes of death after operations. The following remarks on a subject not generally spoken of by surgical authors are worthy of attention :—

> "The surgeon, being convinced of the necessity of having recourse to operation, should proceed fully and unreservedly to lay before his patient the state of the case, and, if necessary, state the reasons that render an operation imperative, in order to obtain his consent and that of his family. In the event of the patient refusing to submit, what course should the surgeon pursue? In this he must be guided partly by the nature of the proposed operation and partly by the state of the patient, and by his capability of forming a correct judgment of his case. If the operation be one of expediency, merely for the relief of an infirmity, or the removal of an ailment, that does not directly jeopardize life, most certainly no surgeon would think of undertaking it without the full consent of his patient. If, on the other hand, it be an operation that is imperatively necessary for the preservation of life, in which the delay of a few minutes or hours may be fatal to the patient, as in the case of the proposed ligature of a wounded artery, or the relief of a strangulated hernia, and in which the patient, not being aware of, or capable of being made to understand, the necessity of immediate action, is unwilling to assent to the proposal, the surgeon truly will be placed in a dilemma of anxious responsibility, between allowing the patient to fall a sacrifice to his ignorance or timidity, and attempting, perhaps unsuccessfully, to rescue him from inevitable death against his own consent. I believe the proper course for the surgeon to pursue under such circumstances is to judge for the patient in a matter on which he is clearly unable to form an opinion, and to compel him, so far as practicable, to take the necessary steps for the preservation of his life. In the event of the patient being insensible,

as after an injury of the head, the surgeon must necessarily take upon himself to act as the case requires. Children cannot be considered capable of giving an opinion as to the propriety of an operation; the consent of the parents is here necessary and quite sufficient, and in their absence, the case being an urgent one, the surgeon must stand *in loco parentis*, and take all responsibility upon himself."—(P. 47.)

We fully concur in these opinions: the surgeon is no doubt placed in a difficult and very grave position by the refusal of a patient to submit to an operation which distinctly holds out a fair prospect, and the only one, of saving his life; but we think that in general the duty of the surgeon is clear, if the case is hopeless without operation, and if the operation holds out a reasonable prospect of saving life. We have not hesitated to act on this principle, on more than one occasion, in cases of hernia and of amputation, and have, after exhausting the powers of persuasion, put an end to the suicidal resistance of the patient by the administration of chloroform without his consent.

A chapter on the subject of amputation, with a description of the special amputations, is included rather oddly under the head of "First Principles," the author not finding a more convenient place for their description.

A preference is given to the operation by flaps, but this preference is properly qualified by the statement that an equally good stump may be formed by either circular or flap operation, the choice chiefly depending on the special dexterity the surgeon may have acquired by the habitual performance of one or other operation.

Due regard, however, is paid to the special situations and to the condition of the limb, as demanding a combination of the two operations.

The advantage of operating by the formation of skin-flaps, and circular incision of the muscles, is recognised in amputation of a muscular arm or leg. We are inclined to think the same operation in the lower third of the thigh, especially when the limb is muscular, preferable to Vermale's operation by lateral flaps, which our author recommends in this situation. The special amputations are rather briefly described. Although the scope of a work of this kind does not admit of the description of every variety of performing the different amputations, yet the peculiarities of many cases of disease, as well as of injury, so frequently require a deviation from the ordinary modes of operating, that a brief notice of some of the other methods in common use would have been desirable. Thus the combination of the flap and circular operations, which is so frequently demanded in primary amputations, receives very little attention; the operation by circular incision is not described; amputation through the metatarsus, an operation infinitely to be preferred to Hey's amputation, when the case will admit of it, is not spoken of; one mode only is described of amputating through the shoulder and ankle-joints. Amputation of the distal phalanx of the fingers is spoken of as being frequently required on account of necrosis. This little operation appears to be a favourite subject of practice among students in the dissecting room, but we do not hesitate to say it is one which is scarcely ever required in the living body in cases of injury; and, we think, if Mr Erichsen will remove by simple incision a few distal phalanges in a state of exfoliation, and observe how slightly the usefulness and appearance of the finger is thereby impaired, he will not in future speak of this small mutilation as being frequently required. Some of the illustrations in this chapter (such as that of amputation of the upper arm, fig. 18) are not so carefully executed as most others throughout the volume, and, on the whole, the chapter, considering the importance of the subject, is rather meagre.

Recent researches in pathology have tended in various directions to simplify and render more concise our ideas on many points in surgery; on some subjects, however, the nice distinctions of modern authors appear to us to have a tendency rather to divert the student's attention from the leading features of some important processes. The union of wounds is commonly said to take place in three different ways, union by first intention, by secondary adhesion, and in open wounds by granulation.

Our author, following Mr Paget in his lectures on "the repair of injuries," says—

> "Incised wounds may unite in the five following ways:—1*st*, By the direct growing together of their opposed surfaces; 2*ndly*, by scabbing; 3*rdly*, by the opposite surfaces uniting through the medium of coagulable lymph; 4*thly*, by granulations springing up from the sides and bottom, and thus filling up the wound; and 5*thly*, by the growing together of two granulating surfaces."—(P. 84.)

Now, with all respect for the observations of Dr Macartney and Mr Paget, we doubt the expediency of surgical writers prominently separating the first and third of these modes of union. Mr Paget describes Dr Macartney's "immediate union" as the direct growing together of two cut surfaces without the occurrence of any uniting medium. A mamma is excised, and accurate union of the wound is obtained; the parts are carefully examined after death three weeks afterwards, and no trace of interposed lymph is discovered. Analogous experiments are performed on rabbits, and a similar result is obtained at the end of a few days. Is this conclusive evidence that no plastic material had at first formed the medium by which these surfaces coalesced? We think not. The very fact of the *growth* of two surfaces to each other suggests, we think, the necessity of a cementing medium. We are not prepared, however, to throw down the gauntlet on a question like this to an observer like Mr Paget; but we think, without more conclusive evidence on the subject, the surgical writer would be justified in describing "immediate union" and "primary adhesion" as different degrees of the same process. The student of surgery will have less difficulty in understanding this than in reconciling what he hears every day spoken of as "primary union," with the Hunterian term "adhesive inflammation," which our author speaks of in his "first principles" as a "distinct disease."

Then, as to the second mode of union by scabbing, we know that small open wounds, protected from the air by a crust of blood, cicatrize without the occurrence of suppuration or granulation; but is this not the same process as the "adhesive inflammation," modified by the form and nature of the wound?

The lymph, in the case of an incised wound which has been closed, forms the interposed medium; in the case of an open wound, the superimposed layer by which the healing is effected. These remarks may be thought indicative of a tendency to retrogression; but we think the object of the surgical writer should be to lay down broad and well-ascertained facts as "surgical principles;" nor does this imply the necessity of his omitting the mention of the suggestions of modern pathology. Thus, the ordinary modes of healing of wounds might be simply described in the old-fashioned way as *primary* and *secondary*, the cicatrix being formed in the one case without, and in the other with, the intervention of the process of granulation, and forming in the case of closed wounds the interposed medium, in the case of open wounds the superimposed layer, by which the wound is closed, whilst the "immediate union" of Macartney and Paget might be separately spoken of as a degree of the first process, or as a distinct mode of union which was not yet generally recognised.

We regret to see the objectionable term "filling up" revived as descriptive of the healing of an open wound by granulation, as it is used too at p. 39 as descriptive of the cicatrization of an ulcer. Due weight is given to the contraction of the surface and of the surrounding tissues during cicatrization, and the development of the "cicatrisial tissue;" but the old notion of the *filling up* of an ulcer should no longer be sanctioned by the use of a term which leads to misconception as to the true nature of the reparative process. We gratefully acknowledge, however, the omission of such refinements of modern nomenclature as the terms "union by growth," "modelling process," &c., which we think are eminently suggestive of erroneous ideas to the student of surgery.

We are glad to find that the principles of treatment of wounded arteries, inculcated by John Bell, and more recently urged on the profession by Mr Guthrie, have a strenuous advocate in Mr Erichsen. Clearly and decidedly as the necessity of a double ligature to the wounded point of an artery has been insisted on, no rule in practice is more frequently set aside, and the Hunterian treatment of aneurism is applied in spite of all the warnings as to the inefficacy of the practice.

The ulnar artery, for instance, is wounded, as described by our author, and to arrest the hemorrhage the brachial artery is ligatured; the bleeding shortly afterwards breaks out afresh from the original wound, a single ligature is next placed on the wounded vessel higher in the forearm, the inosculation of the palm next renews the bleeding, and the radial artery is tied; but even this is of no avail: we presume the surgeon would next have tied the interosseous arteries if he could have found them, but having exhausted every mode of applying the Hunterian principle he ties the wounded ulnar artery below the wound, and the bleeding is permanently arrested. Four operations are performed needlessly, bringing the life of the patient into no small danger, and the life of the limb into imminent peril, instead of one operation attended by no risk either to life or limb, the enlargement of the original wound, and the application of two ligatures above and below the bleeding point in the vessel. The case is not imaginary, and it is the type of a phalanx of cases quoted by Mr Guthrie, and of many others similarly treated in spite of his warnings, where patients have been bungled out of their lives by defiance of the surgical axiom, *to tie above and below the wounded point.*

Few rules in surgery, however, are absolute, and cases exceptional to this rule must occur to the recollection of every surgeon who has seen much of arterial hemorrhage. Few surgeons, we presume, would venture to follow the rule in a stab-wound penetrating the highest part of the interosseous ligament between the tibia and fibula, in uncertainty as to which vessel was wounded, and in a situation where the application of ligatures is barely practicable. Again, although well aware of the importance of the rule, we cannot go along with our author to the full extent when he says—

> "The rule of cutting down on the injured part of the artery applies to all cases in which the wound is still open, *whatever be its condition.* However deep, inflamed, and sloughy the wound, however ill-conditioned and infiltrated with pus and blood the neighbouring parts may be, there is no safety to the patient unless the vessel be cut down upon and tied at the part injured."—(P. 139.)

We feel that we should be treading on dangerous ground in calling attention to exceptions to this principle, knowing as we do its importance as a general rule. We can almost agree with our author when he speaks of the Hunterian operation, when it succeeds under such circumstances, as being "accidentally successful." But is there safety to the patient when a wounded artery is ligatured, where it is bare and isolated among sloughing tissues? We have seen the Hunterian principle applied to bleeding arteries so frequently with success in such circumstances, whilst we have seen failure follow the double ligature of a disorganized artery, that we cannot help feeling that Mr Guthrie has ridden his hobby a little too hard. The correct general principles of treatment, however, are well and forcibly stated in the work before us, and this is of much greater importance to the student than his attention being diverted from the rule by prominent mention of the exception.

An exception, however, to the rule occurs a few pages further on, which we feel bound to notice. The treatment of wounds of the palmar arch, where some time has elapsed since the occurrence of the injury, and when the tissues are infiltrated and swollen, is thus spoken of:—

> "Under these circumstances it is necessary to deviate from the ordinary rule of practice in wounded arteries. The plan that I have more than once found to succeed is the ligature of that artery, above the wrist, which appears to furnish the principal supply of blood to the wounded arch, the ulnar, if it be the superficial, the radial, if it be the deep, and the application of compression by the ring tourniquet to the other. Should this not succeed both arteries must be tied."—(P. 157.)

Now, if there is a situation in the body in which the Hunterian operation is inapplicable in the treatment of wounded arteries, it is the wrist. We have seen many such cases as that here described, and we never failed to find the hemorrhage permanently arrested by dilatation of the wound, however infiltrated the tissues, and the accurate application of a graduated compress to the bleeding point. Were this to fail, surely compression of the two arteries at the wrist or of the brachial should supersede the necessity of attempting to check the bleeding by the unsurgical method of applying a single ligature to one or both arteries above the wrist.

The necessity of extending the Bell principle to the treatment of traumatic aneurism of the diffuse kind is inculcated; but we find our author is in favour of the single ligature above, where the aneurism is circumscribed and situated on a vessel of some size. A traumatic aneurism in the ball of the thumb is thus treated by ligature of the brachial; and the common traumatic aneurism at the bend of the arm would, in accordance to this rule, be similarly treated by a single ligature to the artery above. We doubt the advisability of this plan. There is not the same tendency to solidification of the tumour by fibrinous deposit in the circumscribed traumatic aneurism, as in the spontaneous form of the disease; and instances are recorded where the old operation has been required in cases of the kind, after the single ligature above had failed in effecting a cure. Moreover, the objection which exists to the ligature of the vessel at the seat of disease in spontaneous aneurism, is absent in the aneurism from wound. The artery is healthy, and consequently in a condition to bear the ligatures safely.

The operation of applying ligatures to a bleeding artery in a deep wound is described as being frequently attended with great difficulty, even when the vessel can be commanded by pressure at a higher point. Now we would suggest that this difficulty is occasioned to a great extent by our author having followed Mr Guthrie's advice, never to employ a tourniquet in the operation. We know from experience the very great difference in the facility of the operation, when the circulation is controlled by a tourniquet, and by the fingers of an assistant; and if there is one operation more than another in which the use of the tourniquet is invaluable, it is this. Its use is said to "congest the parts below." We think this objection an imaginary one, and of infinitely less moment than the loss of blood, and the obscuring of the wound by bleeding, which are sure to take place when we trust the vessel to the pressure of an assistant's fingers, which are apt to become exhausted in the most difficult part of the operation. If the tourniquet is quickly and firmly screwed up, there is no great congestion produced, the incisions are bloodless, the slit or puncture in the vessel is at once seen on clearing out the clots, and the operation is thus rendered in general a *remarkably easy one.* Having thus employed the tourniquet in the thigh, leg, and arm, in cases of wounded artery and traumatic aneurism, and having seen it on various occasions similarly employed by others, and never with any other than the best effect, we do not hesitate to oppose Mr Guthrie's authoritative *dictum* on this point.

The subject of senile gangrene is one which has engaged much of the attention of practical writers, and on which, as regards both the pathology and the treatment of the disease, very different opinions still seem to prevail. Mr Syme describes the disease as " a combination of weakness and over-action ;" and, although the description is not perhaps in strictly correct pathological terms, we think it contains the best idea of the condition of the part; the part dies from its feeble power of resisting a very slight amount of inflammation. The various modes of treatment of the affection which have been employed since the time of Mr Pott have been directed, 1*st,* towards the alleviation of pain ; 2*d,* towards the maintenance of power (both local and constitutional) ; and 3*d,* to subdue the local inflammation. The first of these objects is attained by the use of the preparations of opium ; it is in the choice or combination of the modes of treatment which fulfil the second and third indications, that surgeons appear still to hold different opinions. Mr Syme holds that " the weakness cannot be remedied either by local applications or internal remedies ; and that the means employed with this view powerfully excite the tendency to over-action:"[1] the antiphlogistic regimen is accordingly carried out " by enforcing a strictly vegetable diet, abstinence from every sort of stimulant, and the maintenance of perfect quiet in the horizontal posture ;" and Mr Syme adds that his experience has satisfied him of the safety and applicability of this system of treatment even at very advanced periods of life. Sir B. Brodie, again, believing the maintenance of power to be the first object, says that "one principal object to be kept in view is the maintenance of a sufficient supply of blood in the system. As the abstraction of blood is mischievous, so the opposite treatment is likely to be beneficial. Let the patient, then, be put on a system of nutritious diet— . . . . let him live chiefly, but not entirely, on animal food— . . . . In addition to this, administer some such stimulants as ale, wine, or brandy."[2] Again, local remedies fulfilling precisely opposite indications are recommended by these authorities. Sir B. Brodie employs cotton-wadding, Mr Syme linseed-meal poultices. Our author, adhering to the practice of Sir B. Brodie, supports the system by nutritious diet and the moderate use of stimulants, but warns against the opposite extreme, over-stimulating patients labouring under this disease. How is the undoubted frequent success of these opposite modes of treatment to be accounted for? By difference of atmospheric influence, or of epidemic constitution, to the north and to the south of the Tweed? Or is the subject one of those which are to be left to be pointed at by the homœopathist, who, *in medio tutissimus*, prescribes his globules, and triumphantly proves his success to be greater than that of either system? An explanation of the facts must exist, and the one which naturally suggests itself is, that the disease, which is evidently aggravated in some cases by the one and relieved by the other mode of treatment, must be dependent, in different circumstances, on different degrees of the exciting or predisposing cause of the affection. When the stimulant plan is successful, we must believe the " weakness " to have been the predominating cause of mischief; and when the starving plan succeeds, we must believe the " over-action" to have been the chief source of evil. In the most common form of the disease, when it is accompanied by severe and constant pain, and by the bright blush of inflammation in the neighbourhood of the mortified parts, we have frequently witnessed the relief and progressive improvement afforded by the change from the stimulant plan to a rigidly spare diet; but in the other form of the affection, which is of less unfrequent occurrence among the poor and ill-fed of the lower classes, where the toes or fingers shrivel up and blacken with little or no attendant suffering, when the neighbouring parts are scarcely discoloured, and when the gangrene has less tendency to spread upwards, we have seen the disease progress, and the powers of the system threaten to sink under the starving plan, and improvement has at once followed the use of a more generous diet and a moderate allowance of stimulants, under which the dead parts have been thrown off and a complete cure effected. The error appears to us to lie in a want of discrimination as to the treatment which the special circumstances of the cases demand. In the most common form of the disease, when the pain and redness, the quick and irritable pulse, indicate local inflammation as the predominating cause of the gangrene, antiphlogistic regimen, such as can be borne, is called for; whilst the necessity for nutritious and moderately stimulant treatment seems to be indicated by the comparative absence of these symptoms in those cases in which the gangrene appears to be dependent entirely or nearly so on actual local deficiency of circulation. Local treatment, as far as simple applications are concerned, is probably a matter of minor importance; but reasoning on the nature of the disease under all circumstances, we can have no doubt as to the advantage of maintaining the part as far as possible at an equal temperature by enveloping it in cotton-wadding, an application which, in addition to its being more cleanly, is usually more grateful to the patient, than poultices.

---

[1] Contributions to the Pathology and Practice of Surgery. 1848.

[2] Lectures on Pathology and Surgery. 1846.

We are disappointed to find our author giving countenance to the revival of amputation at a distance from the disease, as a remedy in this malady of feeble old age, and to find that to succeed " the amputation should be done as near the trunk as possible." At the hip-joint?

No subject is treated, in our opinion, in Mr Erichsen's work with greater judgment than that of tumours. The researches of Lebert, Paget, Bennett, and other recent pathologists, have forced this subject on the attention of practical surgeons, and have done much towards a more correct and scientific classification of morbid growths; and our author has judiciously selected from the labours of the pathologists what appears to be most essential to the practical surgeon, without entering too deeply on the endless varieties of structure which have been observed, or have been said to have been observed, in the different forms of abnormal nutrition. To the two great divisions of simple and malignant growths, an intermediate group is added, to which the term semi-malignant is applied. In this division are included the tumours which surgeons have latterly, for want of better terms, described as *doubtful*, *recurrent*, *cancroid*, or *pseudo-cancerous* growths. The group is divided into the *malignant fibrous*, the *fibro-plastic tumour* of Lebert, the *recurring fibroid* tumour, and some forms of *enchondroma*. These intermediate tumours are described as partaking of the characters of the benign tumour in their structure, and of cancer in their progress, their tendency to recurrence, and in their results. Two important practical observations of Mr Paget are referred to in connection with the prognosis of the semi-malignant growths, viz., the increasing malignancy usually shewn by each recurrence of the tumour after removal by operation; the second observation of Mr Paget we give in our author's words.

> " In different persons, and under different conditions, the same disease may pursue very opposite courses, appearing in some to be of an innocent, in others of a malignant type; and he makes the very interesting practical remark, which agrees entirely with the result of my own observation, that the children of cancerous parents may be

the subject of tumours apparently innocent in structure, but certainly resembling malignant growths in the rapidity of their progress, their liability to ulcerate and bleed, and their great disposition to return after their removal."—(P. 369.)

The subject of the propriety of removing malignant disease by operation is treated we think wisely, and in accordance with the general practice of the present day. The subject is one which, like many others in surgery, it is vain to attempt to determine by large statistical results. In the interesting statistical computation presented by M. Leroy to the Royal Academy of Medicine of Paris, the prolongation of life effected by operations for the removal of cancer is shewn in the mass not to exceed six months; and the natural conclusion to be drawn from such a result is, that it would be better to abandon entirely the attempt to remove the disease by operation. But in a mass of cases drawn like these from all quarters, we cannot fail to conclude that cases of all descriptions are included; cases favourable for the performance of the operation, and those (the too frequent *opprobria* of operative surgery) in which surgical interference cannot but hasten the fatal event, are indiscriminately placed in the category from which the practical result is to be drawn. The power of annulling the pain of operation by the use of chloroform is with justice referred to as an element in the question of operative procedure, even in cases where removal of the local disease can be looked upon as little else than a means of temporary relief to the patient. The following remarks on this question are a fair specimen of the sound common sense which we find to pervade Mr Erichsen's work generally, but especially the more practical departments of the subject.

> " So long as an operation was a source of great pain, and of much consequent anxiety and dread, a surgeon might very properly hesitate in subjecting his patient to severe suffering with so doubtful a result. But now that a patient can be freed by a painless procedure from a source of much and constant annoyance, discomfort, and suffering, the surgeon may feel himself justified in thus affording him a few months or years of comparative ease, though he may be fully aware that, at the expiration of that time, the affection may return, and will certainly prove fatal. . . . . . But in discussing the propriety of operating in a case of cancer, the surgeon can have little to do with general or abstract considerations. He has to determine what had best be done in the particular case that he is actually considering, and it will serve him little in coming to a conclusion as to the line of practice that he should adopt, to refer to the statistics of the gross results of operations, or to general comparisons between the results of cases that are not operated on and those that are. The whole question narrows

itself to the point as to what should best be done in order to prolong the life, or relieve the suffering, of the individual whose case is being considered. In order to come to some definite conclusion on this, it is necessary to classify the different cases of cancer, and to arrange them under the heads of those in which no operation is justifiable; those in which the result of any such procedure would be very doubtful; and lastly, those in which an operation is attended with a fair prospect of success."—(P. 392.)

A long chapter is devoted to the subject of syphilis, which we are glad to find more fully and more scientifically treated than is usual in systematic treatises on surgery. Syphilis and gonorrhœa, we need scarcely say, are described as two distinct forms of disease, a circumstance the mention of which may be thought superfluous by those of our readers who know anything of the researches of M. Ricord, but which is called for by the astonishing fact that the two diseases are still spoken of by some authors, constituting themselves authorities on the subject of the treatment of syphilis, as dependent on one and the same virus.

The limits of the work do not admit of a full discussion of the question of the *simple* and the *mercurial* treatment of syphilis. Our author, however, indicates pretty strongly his opinion as to the necessity of mercury in the treatment of some forms of primary and secondary syphilis; and this, we think, accords with the opinion of the majority of surgeons of the present day, who have seen much of the disease. The indiscriminate, superfluous, and reckless use of mercury of the earlier part of the century is mercifully abandoned; but the reaction which took place in favour of the strictly non-mercurial treatment of the disease seems again to have given place pretty generally to restored confidence in the remedy, when it is employed with discrimination and prudent care. Although we cannot agree with the statement that no evidence has been adduced to prove that syphilis combined with mercury (*administered profusely*) forms a poisonous compound, or rather that the poisons combined produce a cachexia of a more aggravated form than the syphilis alone; and although we should have been glad to find the dangers of mercurialism more strongly urged than in the statement that

> "It is never necessary to continue the mercury so long, or to give it to so great an extent, as to produce very profuse salivation"—(P. 415);

yet, with some of the prejudices against the use of mercury in syphilis, imbibed from the teaching of a school in which, perhaps more than any other the non-mercurial treatment has been inculcated, we cannot shut our eyes against the marked beneficial influence which the remedy exerts generally over the disease, nor refuse our cordial assent to such statements as the following, the correctness of which we have had considerable opportunities of verifying to our own satisfaction.

> "That a great number, perhaps the majority of cases of chancre can be healed without the administration of mercury is undoubtedly the fact; but in many instances it is equally true that the primary sore will not cicatrize properly unless the mineral be administered, or if it do close, that it heals in an imperfect manner, readily breaking out again. But it is a most serious error to confound the healing of the sore with the cure of the disease. The cicatrization of the ulcer and the prevention or neutralisation of the constitutional infection are two distinct things; and the test of the relative value of these two plans of treatment must depend rather on the relative frequency with which they are followed by constitutional symptoms, and on the character that these assume under one or other of these methods, than on the mere skinning over of the ulcer. I cannot agree with the statement that secondary symptoms are less frequent after the simple than after the mercurial treatment of syphilis. I have seen the non-mercurial plan of treatment very extensively employed, ... and I have had repeated occasion to observe the frequency with which it was followed by secondary symptoms, ... and I can safely say that I have seen the simple treatment more frequently followed by secondary symptoms than the mercurial plan has been when properly and judiciously employed."—(P. 413).

We would direct especial attention to the divisions of the subject included under the heads of *consecutive* symptoms, *constitutional* and *infantile* syphilis, which convey much sound practical information in a well-arranged form.

In a chapter devoted to the pathology and treatment of aneurism the relative merits of the treatment of external aneurism by ligature and by compression are shortly discussed, due honour being given to the Dublin surgeons, who have reduced the treatment by compression to its present state of applicability, and have done away with much of the prejudice which formerly existed against this mode of treatment, by explaining correctly the principle on which it effects a cure of the disease. In spite of all that has been said and written against the screws, clamps, and other unwieldy apparatus employed for the purposes of compression, and the risks and sufferings attending their use, confidence in the treatment of popliteal and femoral aneurism by this mode has been pretty generally established at least in England and Ireland, whilst in Scotland (apparently from the nearly invariable success attending the use of the ligature for a good many years past) the treatment has as yet met with less favour. Hitherto surgeons have been chiefly occupied in discussing the relative merits of the two modes of treatment, and the impression left on our own mind from all we have read and seen is that in the abstract the advantages on each side are so nearly balanced that the treatment, as a general rule, is a matter of choice to the patient and to the surgeon. Recent experience of the treatment by compression, however, has, we think, done a good deal to elicit the class of cases in which the one or other mode is preferable. In the many cases in which it has been found necessary to resort to the use of the ligature, from failure of the compression, it will not do to say that the compression has failed from its being improperly applied. Compression fails occasionally (in the Dublin hospitals as well as in those of London), however skilfully applied, and we think the object to which attention should now be chiefly directed, in connection with this subject, is discrimination of the cases in which the one or other method is more specially applicable. Our author is a strenuous advocate of the treatment by compression, and sums up his remarks on the subject by the following conclusions, in which, as regards one point, the length of time occupied in the cure, as compared with that effected by the ligature, his arguments do not convince us that he has not overshot the mark.

> "After carefully considering the relative merits of the two plans of treatment, I think we may conclude that, though in some few cases neither method can be adopted, and amputation is the sole resource, yet that in others compression can be employed when it would not be safe to have recourse to the use of the ligature, and that in all ordinary cases of femoral and popliteal aneurism especially compression should be preferred to the ligature, inasmuch as it is not a more tedious and an infinitely safer method of cure. At the same time it must not be forgotten that its success depends very greatly on the continuous care bestowed upon the case during the progress of the treatment."—(P. 499.)

The subject of special aneurisms is fully and ably treated. The results of ligature on the distal side of the tumour, of ligature of one and of both carotids, of the brachio-cephalic artery, and the causes of death after the ligature of these and other arteries, are illustrated by carefully arranged tables; a most useful plan, which has been followed too in other parts of the volume.

The subject of excision of diseased joints is treated, we think, rather less fully than its importance demands in the present day, when the practice is by no means so frequently made to supersede amputation as it ought to do, even in diseases of those joints of which excision is admitted by all surgical authorities to be an established operation. The two conditions which demand excision of the joints are thus stated:—

> "One is, when the limb that is diseased is useless to the patient, so long as the affected joint is allowed to remain; and the other is, when the discharge is of so morbid a character, or so abundant, and the irritation induced so great as to entail risk of life, the surgeon has the choice of two operations in diseased joints; one is the simple excision of the disorganized articulation, the other the amputation of the diseased limb."—(P. 610.)

No mention is made of a third condition in which excision may be almost said to be indispensable, viz., anchylosis of a joint in such a position as to render the limb nearly or entirely useless; a condition which, at least in the elbow-joint, late experience has shewn may be most satisfactorily remedied by excision of the anchylosed joint, and which is much more certainly followed by the formation of a new joint than the removal of a wedge-shaped portion of bone according to the method of Dr Barton. We object to the statement that the surgeon has the choice of amputation or resection in diseased joints, and the more so because we know that it expresses truly the impression which still too generally prevails as to these operations. In cases of diseased elbow-joint, with rare exceptions, the surgeon has no choice; amputation is totally unjustifiable.

As a general rule, we admit the correctness of the statement, that

> "Care should be taken to remove as little of the bone as possible, the excision being strictly limited to the diseased articular surfaces;"

but in the most common operation of this kind, excision of the elbow-joint, the rule must not be too strongly adhered to; two points must be borne in mind in the performance of the operation, the entire removal of the disease, and the usefulness of the new joint; and nothing will be found to interfere more certainly with the mobility of the new articulation than too limited removal of the articular ends of the bone—a mistake which we think is more frequently committed than the too extensive removal of the bones, against which a special caution is given by our author a little further on. With regard to the treatment after excision of the elbow-joint, we again differ with Mr Erichsen as to the following recommendation:—

> "Until the contraction of the cicatrix has fully taken place, and the neighbouring tissues are quite firm, the joint should be well supported by lateral leather splints."—(P. 613.)

In Edinburgh, where we believe excision of the elbow-joint has been practised by Mr Syme and his followers more extensively than anywhere else, the use of splints is entirely avoided, and especially during the period above mentioned, as tending to lead to rigidity of the new joint, which, with all care and attention in the after-treatment, is frequently apt to take place to some extent.

Mr Erichsen with justice represents excision of joints of the lower extremity as of more questionable utility than that of the shoulder and elbow. Excision of the ankle-joint, and of the astragalus and os calcis together, are shortly disposed of as of very doubtful propriety. Removal of the os calcis, and of the smaller tarsal bones, obtains the full attention, which, as an important means of saving the foot, these operations deserve. Favourable mention is made of excision of the knee-joint; but our author maintains a prudent reserve in giving his opinion as to the merits of the operation, of which sufficient time has not yet elapsed since its late revival to test the exact value. Excision of the head of the femur is spoken of as being, under certain circumstances, a warrantable and advisable measure. The operation is represented as one which is to be undertaken chiefly as a means of saving life, and not so much with the view of preserving a useful limb as the excision of other joints. The subsequent amount of usefulness of the limb (which we know in some cases has been very considerable) is not sufficiently taken into account. Limitation of the disease to the head of the bone, its dislocation from the acetabulum, and a hectic state of the system, are described as the rare combination of circumstances which demands the performance of the operation; and the important fact, in connection with this subject, of the greater reparative power of the acetabulum than of the head of the femur, is adverted to as an additional argument in favour of the operation. The difficulty of ascertaining whether the acetabulum is involved in the disease appears to be, under all circumstances, the chief objection to the operation being undertaken. Examination with the probe is frequently quite insufficient to establish this point, and, under these circumstances, the question must be chiefly decided by the position of the head of the femur.

> "If this has been dislocated early in the disease, and be lying in a carious condition on the dorsum ilii, the probability is that the morbid action is confined to it, the acetabulum being filled up with fungous plastic matter. If, however, the displacement has been of recent occurrence, it is not unlikely that the osseous surface of the cotyloid cavity is involved. If the head of the bone still continue in its socket, the operation should not, I think, be undertaken; for in these

cases, if the disease is far advanced, the morbid action will almost invariably have extended to the bottom or sides of the acetabulum." —(P. 638.)

We are sorry to find that the recent improvements on the old mode of removing loose cartilages from joints have not found favour in University College Hospital. The dangerous operation of removing these bodies by means of a free incision into the joint is fully described, and all that we find said in favour of the proceeding is, that

> " Severe inflammation of the joint less frequently follows this operation than might have been expected."—(P. 619.)

When inflammation occurs, " it must be treated on general principles." It should have been added, however, that these general principles of treatment frequently fail in saving either the limb or life of the patient. The mode of removal of these loose cartilages by means of extrusion through a subcutaneous incision, as proposed by Syme and Goyrand a good many years ago, is spoken of; but we would call Mr Erichsen's attention to a safer and much more effectual mode of remedying this troublesome affection, as described by Mr Syme in the *Monthly Journal of Medical Science* for November 1852. The treatment of this affection has till of late years certainly been one of the standing *opprobria* of surgery, from the list of which it has been removed, like many other of the *opprobria* of our art, by Mr Syme.

The subject of stricture of the urethra is fully treated, and we would refer to this chapter as giving the best and most impartial summary of this important subject, as regards the morbid anatomy and treatment of the disease, with which we are acquainted. Leaving out of view the treatment of stricture by *caustics* (in which, having no confidence, we have no experience), we so entirely agree in our author's excellent *résumé* of the subject, that we find nothing to criticise. The operation of urethrotomy, as introduced into practice by Mr Syme, is spoken of as " a very decided advance in the treatment of certain forms of stricture ;" and the benefits of the operation are, we think, neither exaggerated nor underrated, but are fairly stated.

The classes of cases in which it may be advantageously employed are thus described :—

> " 1*st*, In very old dense cartilaginous strictures, often of traumatic origin, which admit an instrument with great difficulty, and cannot be dilated beyond a certain point, owing to the conversion of the urethral structures into a kind of dense fibrous, almost cicatrisial tissue, that neither admits of expansion nor of absorption by the pres-

sure of instruments; and in which a considerable extent, half an inch or more, of the urethra is involved.

"*2d*, The same kind of stricture complicated with fistulæ in the perineum or scrotum, with perhaps considerable plastic infiltration of these parts.

"*3d*, Very tight strictures, accompanied by excessive sensibility of the urethra, in which each introduction of the instrument is attended by intense suffering, spasmodic movements of the limbs and rigors, so that the patient cannot be induced to submit to a proper course of bougies.

"*4th*, Very elastic, though perhaps narrow strictures, that can be dilated readily enough, even up to the admission of full-sized instruments, but which, when the treatment is discontinued, immediately begin to contract again, so that the patient is never out of the surgeon's hands and sees no prospect of cure."—(P. 857.)

Mr Erichsen insists, with Mr Syme, that a stricture is never *impermeable*, a term on which we think words have been rather needlessly wasted. If the epithet is used collectively, unquestionably no stricture is absolutely impermeable; if a passage exists at all, it is capable of allowing something to pass through it; if it is literally impermeable, the passage is obliterated; but if the term is used individually and personally, many strictures are impermeable; a thing may be possible to one individual, and impossible to another; a stricture impermeable to one surgeon may prove permeable to another. We fully appreciate, however, the advantage of the conviction on the part of the surgeon of the general permeability of strictures, believing with Mr Syme that a degree of confidence is thus given in the use of the bougie, which will frequently lead to its successful employment under very difficult circumstances.

With regard to the "old operation" of cutting into the urethra behind the point of a catheter introduced to the stricture, Mr Erichsen offers the following conclusion, which is well worthy of quotation:—

"Fortunately this operation is now scarcely ever necessary; with patience, and under chloroform, the surgeon may almost invariably get a staff, however small, into the bladder; he then has a sure guide upon which to cut, and by following which he must certainly be led through the stricture into the urethra beyond it. In all cases, therefore, urethrotomy should, if practicable, be substituted for the division of the stricture without a guide."—(P. 859.)

With these remarks we must conclude our notice of Mr Erichsen's work. We had proposed to have directed attention to other important subjects, especially those of hernia and injuries of the head, which meet with full consideration, and are exceedingly well treated in the volume; but we have said enough to indicate the spirit of the work, and to enable the reader to form an estimate of its practical value.

Although on the subject of "first principles" we have thought it necessary to criticise with some severity our author's views on general pathology; and although on more strictly surgical subjects we have expressed our opinions decidedly in matters in which our opinion differs from that of Mr Erichsen, we beg to say that we do so with all deference to a teacher of whose qualities as a practical surgeon and as a writer the perusal of this volume has led us to form a high estimate. We duly appreciate the classically correct style of writing which characterizes the work, and even more we appreciate the circumstance of the diction being of a kind which is easily intelligible to the student of surgery. The illustrative woodcuts, though rather less numerous, perhaps, than could have been wished, are on the whole very well executed, and possess the novelty and advantage which are presented by few of the recent works on surgery, of being for the most part original.

Without instituting a comparison between the work of Mr Erichsen and the various systematic surgical treatises which have appeared within the last few years, we do not hesitate to say that the volume before us gives a very admirable practical view of the "Science and Art of Surgery" of the present day, and we have no doubt that it will be highly valued as a surgical guide, as well by the surgeon as by the student of surgery.

ART. II.—*A Treatise on Auscultation and Percussion.* By DR JOSEPH SKODA. Translated by W. O. MARKHAM, M.D. London, 1853. Pp. 346.

The name of Skoda has been long familiar to those who take an interest in the progress of medical science, particularly in its application to diseases of the lungs and heart. This familiarity has, however, been in great part limited to the name; for, except to those acquainted with the language in which they were originally written, the works of Skoda have been till lately a sealed book. The medical public is therefore much indebted to Dr Markham for his excellent translation, which affords to all interested in the subject an opportunity of judging for themselves. And we cannot but think that whatever opinion may be formed regarding some of the theories of the Viennese professor, no one can rise from an attentive study of this book without feeling that it is the composition of a most careful and original observer, and that every statement contained in it is worthy of very diligent examination. In fact, we have no hesitation in stating that we consider the work before us, as the most important contribution to this department of medical literature, which has been made since the time of Laennec. Many will, we have no doubt, be disappointed on merely glancing over it, at least if they expect to find each subject exhausted by the writer; on the contrary, much is left to the reader, and this volume is rather an admirable treatise on the principles of the diagnosis of thoracic diseases, than a manual containing all the signs required for the recognition of each particular affection.

We propose to give a brief analysis of the more important doctrines brought forward, confining our remarks chiefly to those points on which Skoda differs from others.

The work is divided into two parts; the first embraces a general description and explanation of the phenomena observable by the aid of percussion and auscultation; while under the second are considered the phenomena connected with special conditions of the organs of the thorax and abdomen. Our remarks must be confined almost entirely to the first part, as the second really contains only the application of the doctrines there laid down.

The first chapter treats of percussion, in practising which Skoda follows the method of Piorry, making use of the pleximeter and finger, on the ground that this mode of procedure is the least irksome to the patient, and the most satisfactory to the physician. This mode has now come into very general use, and all who have employed it must, we should think, have no hesitation in according it the preference. We are glad to see that Skoda completely throws aside all the refinements of Piorry and his followers of the French school; thus he says "There is no difference in the percussion-sound by which we can distinguish between organs not containing air, such as the liver, the spleen, the kidneys, hepatized lung, or lung completely deprived of air by compression, and fluids; a hard liver yields the same sound as a soft liver, a hard spleen as a soft spleen, except when these organs contain bony or chalky materials; and blood the same sound as pus, water, &c." (Pp. 4, 5). And again, "there is no such thing as a liver, spleen, heart, lung, or stomach sound; the sound over the lung may, under certain circumstances, be exactly similar to the sound produced by percussion over the liver." (P. 7).

We cannot, however, look upon his own classification of percussion-sounds as characterized by that simplicity for which he is generally remarkable. He gives the following classes of sounds :—

1. Full—Empty.
2. Clear—Dull.
3. Tympanitic—Non-tympanitic.
4. High—Low.

Now, we are far from saying that these divisions are not founded on fact; we believe that these varieties of sound do really exist, but that for practical purposes most of them may be advantageously disregarded; and we look upon this arrangement as an unnecessary complication of the subject. For example, the varieties of *full* or *empty* may, according to Skoda, depend either on the quantity of air contained in an organ, or the situation of the air, or still more frequently on the condition of the walls of the thorax or abdomen; but for purposes of diagnosis, a distinction which may be founded on such different circumstances, is practically valueless. Indeed Skoda himself says, "It is only when the most marked differences exist between the full and the empty sound that any certain conclusions can be drawn." It is unnecessary to say anything regarding the varieties of *clear* and *dull*, as these terms are taken in the sense usually attached to them. They may, however, according to Skoda, be combined with various degrees of fulness or emptiness, for he says, "A percussion-sound may be full and clear, and also full and dull, empty and clear, and empty and dull." (P. 12). But we must say that such a combination as full and dull appears to us unintelligible, because, as Skoda himself states, "a completely dull and a completely empty sound have naturally the same significance, and they are represented by the thigh percussion-sound." For these reasons we would discard the varieties of *full* and *empty* altogether, as we consider that by attending to the *clearness* or *dulness* of the sounds, taking in connexion with their character in these respects the feeling of resistance recognised by the finger, we shall obtain all the information which the practice of percussion is capable of affording. We also look upon the introduction of the class of *tympanitic* and *non-tympanitic* as unnecessary. Tympanitic merely implies an exaggerated degree of clearness, accompanied in some cases by an elevation of pitch, but having nothing peculiar to itself; while the term non-tympanitic expresses simply a negative, and may be applied with equal propriety to every variety of sound, from that elicited from the healthy lung to that of the liver or thigh.

Professor Skoda makes some very interesting remarks on the rationale of the production of tympanitic sounds, and the conclusions he arrives at are very different from what has been the commonly received opinion upon this point. His general results are thus expressed:—

> "It is thus proved, both by observations on the living body and by researches in the dead, that the percussion-sound is invariably tympanitic, when the parietes of the organ which contain the air are not stretched; but that, on the contrary, when they are firmly stretched, the percussion-sound becomes less or not at all tympanitic, and even dull. Thus the fully distended stomach, the strongly inflated lungs, the tense thorax (as in pneumothorax), the firmly contracted abdominal walls produce a non-tympanitic or merely an indistinctly tympanitic sound; whilst, on the other hand, the relaxed stomach, the collapsed lungs, the compressible abdominal walls, give a distinctly tympanitic sound."—(Pp. 18, 19.)

Of the correctness of these opinions we entertain not the slightest doubt; and no one we think who will take the trouble to repeat the very simple experiment of percussing a stomach in various states of inflation, can have any hesitation in arriving at the same conclusion.

The fourth class of percussion-sounds embraces the *high* and *low*, but much value is not attached to differences in this respect even by our author; the only practical application of any importance, appearing to be the occasional possibility of diagnosing the presence of a small amount of tubercle in the upper part of a lung, by the different pitch of the percussion-sound.

Skoda recognises two other varieties of sounds, which cannot be classed under any of these divisions; they are the metallic ringing and the cracked-pot sounds.

The former he considers as allied to the tympanitic, and he does not agree with Piorry in considering the presence of both air and fluid as necessary for its production.

The cracked-pot sound he looks upon as a compound of the ordinary percussion-sound of cavities, combined with the hissing produced by the forcing out of a portion of the contained air into communicating bronchial tubes. He also alludes to the circumstance that for the production of this sound it is necessary that the mouth be kept open.

In the second chapter a few observations are made on auscultation in general; while the third is occupied by a description of the auscultatory phenomena of the organs of respiration.

The phenomena of the voice, as heard in the thorax, first engage the attention of Skoda. He divides it into strong or weak, and clear or dull. By strong is meant that the voice "not only emits sound, but also produces a degree of concussion in the ear;" while it is called clear "when its articulation is audible." These distinctions (which appear natural and simple) having been established, the following somewhat startling statement is then made: "*Variations in the strength and clearness of the thoracic voice cannot be explained by the laws of conduction of sound.*"

Ever since the time of Laennec the modifications of the thoracic voice have been explained somewhat as follows; the lung in its healthy state is a bad conductor of sound, but when its density is increased, its power of conducting sound is likewise increased; and it is evident that if the above statement of Skoda be correct, a complete revolution in our ideas must take place, for, as we shall afterwards see, the same principle is applied to the other phenomena of respiration as altered by disease.

The grounds on which the assertion of Skoda is founded are two—first, clinical observations, and secondly, theoretical considerations on the laws of sound. The clinical facts are thus set forth,—"If we repeatedly auscultate the thorax of a person suffering from hepatization of a lung, we shall find that the thoracic voice is at one time increased, and at another diminished in force, without any alteration—discoverable by percussion or other means—having taken place in the condition of the hepatized part." And this he ascribes to the occasional blocking up of the bronchial tubes by fluid, which prevents the communication of the voice, while when it is got rid of by a cough, the bronchophony returns. On this we would merely remark, that while such a fact may be occasionally observed, we believe that it does not occur nearly so frequently as the statement above quoted would lead us to believe.

Skoda then proceeds to attack the doctrine of Laennec on theoretical grounds, and here we must say we consider him to have been anything but successful. The following general statements are premised, with which of course we agree. "Sound does not pass readily from dense bodies into the air, or from the air into dense bodies;" and again, "the more dissimilar the media are, in respect of density and cohesion, the greater is the reflection of the sound, and the less freely does it pass from the one into the other" (p. 36.) Skoda then endeavours to prove, that air is the best conductor of sound; he says, in the first place, "the difference in the conducting power of air, wood, and other bodies, has not been experimentally determined." But various experiments have been made, which prove that air is very inferior as a conductor of sound to many other media. Thus, many years ago, Biot experimented upon the cast-iron pipes which were being laid down to supply Paris with water. A bell was suspended at one end of a train of pipes; on striking the clapper at the same instant against the side of the bell and the internal surface of the pipe, two distinct sounds were successively heard at the opposite end; the one which arrived first was that propagated by the iron wall of the pipe, which was found to conduct sound ten or twelve times more rapidly than the atmospheric air. Leslie also shewed by calculations, that sound is propagated four times more rapidly in a deal board than in air.[1] With a view, however, to illustrate his own views, Skoda proposes the following experiment:—

> " Let any one, for example, so place the end of a wooden tube on a watch, that the whole rim may be in contact with it, and then listen at the other end; he will hear the ticking at the same moment through the wood, and through the air in the tube. If a solid cylinder of wood be now fitted into the tube, and it be placed as before, the ticking will be heard passing through the wood of the tube, and through that of the solid cylinder. Now, if wood were a better conductor of sound than air, the ticking ought to be heard more loudly in the latter than in the former case; but any one may readily convince himself that the reverse of this is the fact. It is a remarkable circumstance that auscultators should make use of a hollow tube, and not of a solid cylinder, and yet assert that dense bodies are better conductors of sound than air."—(Pp. 37, 38.)

With this explanation we entirely disagree. It is true that the sound is heard rather more clearly through a hollow than through a solid stethoscope; the difference, however, is very trifling. This we conceive is owing, not to the superior conducting power of a column of air, as compared to a cylinder of wood, but to the fact that within the hollow stethoscope (the walls of which we believe to convey the sound) the sound is to a certain degree *intensified*, being re-echoed within the cavity of the tube. That this is true is proved by repeating the same experiment with a hollow stethoscope made of a bad conductor of sound, such as cork, when it will be found that the sound so heard is considerably less clear than that communicated through the solid wooden one. Were the sound conveyed by the column of air, the material of which the stethoscope is composed should be indifferent, but if any one make the above experiments, he will find that there is a considerable difference between a wooden and a cork stethoscope. That the sound is conveyed through the walls of the stethoscope is also proved by this; the sounds produced within the chest are conveyed through the thoracic walls, and are hence placed in the same position as if they had been generated there; hence they are best conveyed by a solid medium, for, according to the law formerly mentioned, the more dissimilar the media, the greater is the reflection of sound. And as wood is much more analogous in density and cohesion to the solid thoracic parietes than is air, it follows that the former will convey the sound much more perfectly than the latter. For these, and some other reasons which it would be tedious to enumerate, we cannot come to the conclusion arrived at by Skoda—" Variations in the strength and clearness of the thoracic voice cannot be explained by differences in the sound-conducting power of normal and abnormal lung parenchyma" (p. 89). Although we consider Laennec's views to be in the main founded on truth, we cannot conceal from ourselves that cases every now and then occur, which cannot be explained by them alone.

---

[1] Encyc. Brit., 8th Edition, vol. ii., p. 100.

Skoda, having laid it down as a postulate " that the voice passes into the parenchyma of the lungs through the medium of the air contained in the trachea and bronchial tubes," proceeds to explain the differences in the thoracic voice by what are called the laws of consonance. As his views on this subject constitute the key to the understanding of almost his whole system of auscultation of the organs of respiration, it is necessary to explain pretty fully what he means by the laws of consonance; and in doing so we shall make use as far as possible of his own words.

Consonance is a well-known phenomenon, and consists in the occurrence of vibrations in another body in unison with those of one already sounding, whereby the sound produced by the latter is strengthened. Thus, a tuning-fork sounds much more loudly when laid on a table than when held in the air, and the sound of a jew's harp is scarcely audible unless it be made to vibrate within the mouth; the table in the one case and the air contained in the mouth in the other, consonating with, and so increasing the sound of the vibrating bodies. That the voice, as formed in the larynx, consonates with the air contained in the mouth and nose, is proved by the modifications of the voice which occur according as these cavities are open or closed. In the same way, the air contained in the trachea and bronchial tubes consonates with the sounds formed in the larynx; the parenchyma of the lungs, being neither sufficiently firm nor tense, cannot act as the consonating body. Air only consonates when confined in a given space, and the force of consonance depends upon the form and size of the inclosed space, and upon the nature of the walls circumscribing it. Now, the air contained in the trachea and bronchial tubes consonates with the voice in so far as the walls confining it have, in respect of their power of conducting sound, a similar or analogous condition to the walls of the larynx, of the mouth, and of the nasal cavities. Therefore in the trachea and in the right and left bronchi, the walls of which are formed of cartilaginous rings, the consonance of the voice is nearly as forcible as the voice itself heard in the larynx. But as the bronchial tubes pass into the lungs and diminish in size, their structure alters, the cartilage gradually disappearing, so that the finer ramifications are merely thin membranous canals; consequently, the consonance of the voice is here very much diminished. In order to produce increased consonance in the fine bronchial divisions, one of three conditions must be fulfilled,—either the walls of the tubes must be cartilaginous, or if membranous, the membrane must be very dense, or the tissue of the lung around the tubes deprived of air. Of course the communication between the air in the tubes and the air in the larynx must remain uninterrupted.

Such is an abstract of Skoda's theory brought forward to explain an increase in the strength or clearness of the thoracic voice, but we do not propose to enter into a detailed consideration of it—partly, because many of the points at issue being purely theoretical, a disquisition on such topics would be of no practical benefit; and partly, because there is still a want of such facts (particularly derived from clinical observation) as might enable us to bring the truth of the theory to the test of experience.

We shall merely observe that this theory carries with it a certain amount of probability in its favour, and that in particular it may be held to explain some of those rare cases, where the voice is heard louder over some part of the thorax than over the larynx. We can understand the occurrence of consonance in an extensive cavity communicating with a large bronchus, and therefore in pretty direct communication with the larynx; but it appears, to say the least, doubtful whether consonance could occur in air contained within such a system of tubes, as that of the minute ramifications of the bronchi.

For an account of the experiments upon which Skoda founds, or rather by which he illustrates his theory, we must refer our readers to his own pages.

The following are the diseased conditions of the respiratory organs, which, according to Skoda, can produce an increase in the strength or clearness of the thoracic voice.

1. All diseases by which the parenchyma of the lung is

deprived of air, and rendered firm, dense, and solid. This head embraces pneumonia, tubercular infiltration, and pulmonary apoplexy.

2. Those conditions of the lung in which its parenchyma is deprived of air by compression. A variety of circumstances are capable of producing this pressure; for example, fluid or solid exudations, gas present in the pleura, tumors, enlargements of the heart, pericardial effusions, aortic aneurisms, enlargements of the abdominal viscera, curvatures of the spine, or other deformities of the thorax; by far the most frequent of these causes being pleuritic effusions and pneumothorax.

3. Thickening and enlargement of the cartilages of the bronchial tubes within the lungs. A simply dilated bronchus is, according to Skoda, incapable of producing an increase of the thoracic voice, unless the surrounding parenchyma have been deprived of air.

Skoda then proceeds to consider the indications afforded by increase of the thoracic voice, and to classify the different modifications of it which are met with. After a somewhat extended criticism of Laennec's opinions, he comes to the following conclusion regarding them:—"I believe I have shewn that Laennec's pectoriloquy and bronchophony represent one and the same phenomenon; and that his ægophony is a sound which is occasionally heard accompanying the consonating voice, and which has no necessary connexion with the presence of fluid in the pleura, nor, as a sign, any especial value."

Skoda's own divisions of the modifications of the thoracic voice are—

1. Loud bronchophony.
2. Weak bronchophony.
3. An indistinct humming.
4. Amphoric resonance and metallic echo of the voice.

Our limits forbid us to point out what information is to be derived from the existence of one or other of these conditions; we shall merely observe that we consider he has done a great service in turning away the attention of auscultators from pathognomonic signs. In this respect he is exactly the counterpart of many writers of the recent French school, and we cannot too strongly recommend the student to adopt the practice of Skoda, and only construct his diagnosis upon a combination of signs. A good illustration of these remarks is afforded by what he says regarding loud bronchophony:—

"The mere existence of loud, clear bronchophony does not enable us to decide whether or not cavities, or enlarged bronchial tubes, are present in a lung which is hepatized, or which is infiltrated with tubercular matter. But, as we know that abscesses are very rare in pneumonia, and, on the other hand, that vomicæ are rarely absent in tubercular infiltrations, we shall not often err if in tubercular diseases we prognosticate the presence of cavities at those parts where the voice is heard loudest, and never infer the existence of an abscess in pneumonia from the thoracic voice, however loud it may be heard."

All the other statements made regarding the importance to be attached to the other conditions of the voice are of the highest possible value.

Skoda then goes on to consider the murmurs or sounds heard during respiration. These he classifies under the heads of respiratory murmurs, râles, and whistling and sonorous sounds; the distinction between the murmurs and the other sounds being, that in the case of the former "the air, in its passage through the lungs, meets with no obstruction;" while the râles, as well as the whistling and sonorous sounds, "are caused by the presence of fluid in the air-passages, by thickening of the bronchial mucous membrane, and by any partial narrowing or compression of the bronchial tubes."—(Pp. 81, 82.)

Nothing can exceed the simplicity of the classification proposed; the murmurs are divided into—

1. Vesicular breathing.
2. Bronchial breathing.
3. Amphoric echo and metallic tinkling.
4. Indeterminate respiratory murmur.

Laennec's "cavernous breathing" is altogether, and we think most properly, discarded, as it does not differ essentially from bronchial respiration, and the employment of the term is very apt to mislead.

Bronchial breathing is considered as a consonating phenomenon, while under the class of *indeterminate* he classes all those respiratory murmurs which "have neither the character of vesicular nor of bronchial respiration, and are not attended by amphoric echo nor metallic tinkling." These *indeterminate* murmurs are set aside as valueless, giving no information as to the state of the parenchyma of the lungs.

The râles are next considered, and the same simplicity prevails in their classification as characterizes the divisions of the murmurs.

Almost all are ascribed to the passage of air through liquids accidentally present in the bronchial tubes or in pulmonary cavities, and the differences between moist and dry air râles is stated to depend upon the consistence of the liquid present, dry râles being occasioned by the presence of a fluid more tenacious than that which causes the moist râles.

Skoda's division of the râles is as follows:—

1. Vesicular râle.
2. Consonating râle.
3. Dry, crepitating râle with large bubble, or craquement.
4. Indeterminate râles.
5. Râles accompanied by amphoric resonance and metallic tinkling.

Of this division it is merely necessary to say that the vesicular râle is that which was called by Laennec crepitation, and which, according to Skoda, is not pathognomonic of pneumonia, but merely "denotes the presence of fluid in the finer bronchial tubes and in the air-cells, and the entrance of air into the latter." The same signification is given to the dry crepitating râle as was attached to it by Laennec.

It will be observed that this classification is exceedingly simple. We miss the subcrepitant, the mucous and submucous, the cavernous, and a variety of other terms, the introduction of which has proved so embarrassing to the student of auscultation. All these are at once got rid of by Skoda under the head of *indeterminate*, which comprises "all those râles which are neither vesicular nor consonating, and are not accompanied by amphoric resonance or metallic tinkling; they offer no special indications respecting the condition of the lung tissue."—(P. 131.)

A few observations on pleuritic friction conclude the consideration of the auscultatory phenomena of the organs of respiration.

The third chapter treats of the auscultatory phenomena presented by the organs of circulation; but although it contains much highly important matter, our notice of it must be very brief.

The first point to which Skoda directs attention, is the cause of the impulse of the heart. We cannot attempt to follow him through the arguments by which he endeavours (and in general we think successfully) to prove that various of the generally received opinions are erroneous. The explanation which he adopts is this: when fluid escapes from a vessel the equality of the pressure produced by the fluid on the walls of the vessel is lost; at the point where the fluid escapes no pressure is exerted, but at the point opposite to this it is still in full force. The pressure exerted in this last situation has a tendency to move the vessel in a direction opposite to that of the stream which is escaping. This is the state of matters in the heart; on the contraction of the ventricles the blood is forced into the aortic and pulmonary arteries, and consequently no pressure is exerted over their openings, but at those points opposite to them the pressure continues, and hence causes a movement of the heart in a direction contrary to that of the stream of blood, and in consequence of this, the impulse of the apex of the heart is produced. After what has been now stated it may be unnecessary to add, that Skoda believes that during the systole the heart moves downwards and forwards, while in the diastole it moves upwards and backwards. Other causes are, however, in operation to produce the impulse of the heart; these, according to Skoda, are the lengthening of the aortic and pulmonary arteries, and a change in the form of the heart caused by its contraction.

The force of the heart's impulse is divided into three degrees.

1. " An impulse which does not raise the thoracic walls, nor shake the head of the auscultator, or which is imperceptible."

This sign is, however, very indefinite, as it may coincide with pericardial effusion, with a perfectly normal heart, or with various degrees of hypertrophy or dilatation.

2. " An impulse which does not raise up the thoracic walls, but imparts a strong concussion to the head of the auscultator."

Such an impulse indicates hypertrophy, or may be the result of increased action of a healthy heart.

3. " An impulse which raises the thoracic walls during the heart's systole, the walls sinking again during its diastole. The head of the auscultator necessarily follows these movements." To produce such an impulse as this, hypertrophy and dilatation of both ventricles must exist.

The sounds of the heart are next considered, but on this point we shall only remark, that Skoda looks upon them as being produced in a very complicated manner,—he considers that " the ventricles, the aorta, and pulmonary artery, severally contribute in the production both of the first and of the second sound of the heart" (p. 182). An able analysis of the murmurs of the heart is then laid before us; but here we shall confine ourselves to two quotations, the first regarding the friction-sound of the pericardium, the second regarding incompetency of the mitral valves.

> " I know no sign by which the friction-sounds of the pericardium can be distinguished from the internal murmurs of the heart, excepting this, that the internal murmurs correspond pretty exactly to the rhythm and to the natural sounds of the heart, whilst the pericardial

friction-sounds seem to follow upon the movements of the heart. This distinctive sign is only available when the murmur is somewhat prolonged; if it be of short duration we cannot determine whether it is endocardial or pericardial."—(P. 219.)

And—

> "When the closure of the mitral valves is not complete, a portion of blood at each systole regurgitates from the left ventricle into the left auricle, causing distension of the left ventricle (auricle?) and of the pulmonary veins and arteries, so that increased efforts of the right ventricle become necessary in order to force the blood onwards through the distended vessels; the pulmonary artery, thus strongly dilated, presses with an exaggerated force upon the blood within it, and drives it more suddenly and more forcibly backwards against the semilunar valves during the heart's diastole, whereby the second sound of the pulmonary artery is increased."—(P. 229.)

This increase of the second sound in the pulmonary artery in cases of mitral incompetency, is much insisted on by Skoda; we believe it to be a very important diagnostic sign, and one which has been very generally overlooked. By means of it we may, according to our author, decide whether a ventricular murmur with the first sound be due to mitral regurgitation, or to roughening of the lining membrane of the ventricle about the arterial opening.

This chapter finishes the description of the phenomena observable by the aid of auscultation and percussion. The second part of the work is occupied by a brief application of the principles already laid down, to the diagnosis of special diseases of the organs of the thorax and abdomen. On this, however, we cannot enter, both because our limits are exhausted, and because this part is in itself a summary of facts, so that to do it justice we should require to quote it nearly entire. We must content ourselves by recommending it strongly to the attention of our readers, assuring them that they will find in it many valuable suggestions.

It only now remains for us to compliment Dr Markham on his very faithful and excellent translation, and to repeat that the profession is much indebted to him, for having made so valuable a work so readily accessible.

ART. III.—1. *Elements of Psychological Medicine.* By DANIEL NOBLE, F.R.C.S. London, 1853. 12mo, pp. 340.

2. *On the Nature and Proximate Cause of Insanity.* By JAMES GEORGE DAVEY, M.D. London, 1853. Pp. 77.

3. *Popular Errors on the subject of Insanity examined and exposed.* By JAMES F. DUNCAN, A.M., M.D. Dublin, 1853. 12mo, pp. 265.

Mr Noble is known to the profession by a volume on the Anatomy of the Brain, one of the principal features of which was its defence of phrenology. The present treatise is offered as an introduction to the practical study of insanity adapted for students and junior practitioners, and one of its most striking peculiarities is, that it contains a deliberate recantation of the phrenological doctrines formerly held and inculcated by the author. We heartily congratulate Mr Noble on his conversion to the more catholic faith of the great mass of his professional brethren, and still more on his manly candour in the avowal of it he has made. He is one of the very few men we have known who, having once committed themselves in print to the defence of a dogma, have afterwards relinquished, and as publicly recanted it. Mr Noble accredits Dr Carpenter with the merit of his conversion, or at least ascribes to a criticism in the *British and Foreign Medical Review* of his work on the brain, by Dr Carpenter, the conviction which first led him to a re-examination of the whole question, and ultimately to the recognition of the general truthfulness of the doctrines laid down by Dr Carpenter as the more correct physiology of the brain.

The work before us is the substance of a course of Lectures delivered at the Chatham Street School of Medicine, in Manchester, at the request of the lecturers of that school. The vast importance of some practical knowledge of insanity to the general practitioner is very lucidly and forcibly illustrated in the introductory lecture.

We are happy to perceive the more general recognition of the importance of a special attention to this department of medical education, in the fact that the Honourable East India Company now requires certificates of a three months' asylum attendance from their medical officers, and that regular courses of clinical lectures have been commenced in the great London asylums, and also in the Royal Edinburgh Asylum, in connection with the Edinburgh medical school.

Definitions of insanity have been the opprobrium of medical witnesses in our courts of law, and not less, we fear, of medical writers in systematic treatises on the subject. Mr Noble ventures a new one, viz., that insanity consists "*in chronic disorder of the brain, inducing perversion of ideas prejudicial to, or destructive of, the freedom of the will.*" Admitting that there is a chronic disorder of the brain (a point in regard to which many objections might be urged, as far, at least, as regards its truth, or importance in a definition), what does the expression "perversion of ideas" amount to more than is expressed by the term *mental derangement?* If it defines anything at all, it defines what almost all practical *alienistes* will object to, namely, that there is in every case of insanity a perversion of *ideas*. And in regard to "the freedom of the will," Mr Noble seems to confound *free agency* and *free will* in a way that would make Jonathan Edwards stand aghast, if he were in the body, and which would place any medical witness, rash enough to use this definition in a court of law, in such a nice position for the counsel for the "opposite party," as might well lead him to curse the day he ever saw "Noble's Elements," instead of thanking him for preparing him, as he proposes to do, for the duties of a medical witness.

Mr Noble gives a very excellent summary of our knowledge of the physiology of the brain, and suggests that while the gray matter of the convolutions (the hemispherical ganglia) is the seat of the intellectual faculties, the optic thalami and corpora striata are the seat of the *emotions*. The anatomical relations of those ganglia, and various other ingenious considerations are offered in support of this hypothesis; for which, however, we must profess that, much as we admired its ingenuity, and welcome it as a suggestion worthy of further inquiry, we still regard it as even anatomically improbable, and think that there is something even more consonant with our notions of the vast importance, extent, and variety of the passions and emotions of the human soul, in the large territorial expansion awarded to them as their seat and organ by the phrenologists, than in the comparatively limited insular ganglia assigned to them by Mr Noble.

We have only one more exception to take against the work before us, and that is in regard to the new classification adopted of the different forms of insanity. Mr Noble distinguishes the varieties under three heads,—*Notional, Intelligential,* and *Emotional.* Under the first he arranges the various forms of insanity generally classed under the term *monomania,* where there are delusions regarding particular objects, or in certain trains of thought. Although the term *monomania* is not strictly correct in reference to almost any case of insanity, and the term *notional* is not open to the same objections to which the former is exposed, we cannot see any great benefit to be derived from the change of nomenclature, the present one being at least a very convenient one for the general purposes of classification, and the proposed one being as vague as the other is precise. Under the *second* head Mr Noble classes the most opposite forms of mental derangement that could be collected together, viz., *Idiocy*, *Dementia*, and *Acute Mania*—the first a congenital defect, the second a disease of impaired mental activity, and the third one of greatly increased activity. Whatever speculative opinions may appear to justify such a classification, we cannot but regard it as eminently inconvenient, incongruous, and confusing.

Under the *third* head Mr Noble classes those cases in which the emotions or passions are pre-eminently affected, but with a singular inconsistency he maintains that in all such cases there are also delusions or perversion of ideas. This is certainly consistent with his *definition*, but damaging to his *classification* ; for if there are *delusions* in such cases, then they are properly cases of *notional* insanity. On this point, however, Mr Noble is at issue with all modern writers on insanity, denying the existence of *moral insanity*, or insanity without delusion. We regret that our limits do not at present permit us to enter at large upon the discussion of this highly important topic, and that we must content ourselves with begging Mr Noble to review his experience in regard to it. For our part we feel assured, with Esquirol, that perversion of the emotions, passions, desires, and impulses, is the most essential feature of insanity, and that there are innumerable cases of madness, varying from every degree of maniacal excitement, down to intense homicidal and suicidal impulses, and the deepest melancholy in which there exists no perversion of the understanding (ideas), and no delusion, illusion, or hallucination whatever.

Of the other portions of Mr Noble's book we think most favourably ; it is written in a clear and interesting style, and the general principles it inculcates in regard to the diagnosis of insanity, and its mode of treatment, are unexceptionable.

Dr Davey's work propounds, as an original theory of insanity, that it depends upon " *irritation* or *morbid* sensibility" of the gray matter of the brain. Unlike Mr Noble, he retains the entire system of phrenology, which he regards with profound admiration. Without entering upon a discussion of his theories we cordially concur in the practical importance and truth of his principles of treatment, which coincide with those generally espoused and practised in all our best asylums for the insane, and which consist mainly in the removal of all external and internal sources of irritation, the administration of a nutritious diet, with the judicious addition of stimulants and anodynes.

Of the excellent work of Dr Duncan we cannot speak too highly. It is a popular work designed to remove popular prejudices, and admirably adapted for this end ; but it combines also much matter of great interest and value to the medical practitioner, and will amply repay his perusal.

ART. IV.—*Urinary Deposits, their Diagnosis, Pathology, and their essential Indications.* By GOLDING BIRD, A.M., M.D., F.R.S., &c. &c. Fourth Edition, revised and enlarged. London, 1853.

Within nine years the first and the fourth edition of this work have appeared; a striking proof of its own merits, and of the favour with which it has been received by the profession. To Dr Bird belongs no small share of credit for directing attention to the study of the healthy and morbid conditions of the urine; for though the labours of Drs Prout and Bright, and not a few others, had preceded the publication of his own, we scarcely think it will be denied that much of the impetus which the investigation of the characters of the urine, both chemically and microscopically, have of late years received, is due to the author of the " Urinary Deposits." To the publication of this work is mainly attributable the greatly increased and more generally diffused knowledge of the conditions of the urine which presently, as compared with formerly, obtains. In saying this, we would by no means be considered as depreciating, or even casting into the shade, the labours of a host of other observers; but the fact is undoubted, and the success Dr Bird's work has attained is to be ascribed as much to its appearance at the very time at which it did, as to the excellent manner in which it is written, and the fulness of detail on all the points of which it treats.

It is inconsistent with our purpose to notice at length other than the first edition of any work, and the present shall form no exception. The fourth edition of the Urinary

Deposits is both thoroughly revised and enlarged, while on one very interesting and important subject an entire and altogether new chapter has been added. The chapter we refer to, the last in the book, is on the action of some diuretic agents, and of it we shall now subjoin a short notice. In the preface Dr Bird remarks:—

> "I cannot but feel that the subject matter of the last chapter demands some explanation, and I fear it may be thought that I have introduced it without its possessing sufficient connection with the pathology of urinary deposits. The subject of a more rational and philosophical system of therapeutics, one more consistent with an inductive plan of inquiry than we at present possess, has for many years been with me a cherished idea, and I had hoped to have been permitted to have contributed something to the common stock of knowledge on this subject, one of the most important in its bearings on our mission of alleviating the distresses of sickness, and of combating the effects of disease. Severe and protracted illness, with which it pleased Divine Providence to visit me in the early part of the past year, rendered a diminution of labour and a more limited devotion to the duties of my profession imperative. I have been therefore made deeply sensible that such an inquiry must fall into other and more vigorous minds and abler hands, still I hoped that a brief summary of what I had been able to make out respecting the action of some diuretic agents might appear in these pages without censure."

So far from desiring to censure Dr Bird, we feel very much indebted to him for the introduction of this chapter, as well on account of the very interesting statement it contains of his own researches, as for the reason of directing the attention of others to a subject which still requires much careful experiment and observation before many points in connection with it can be looked upon as settled or made out. The idea of "the washing away of a *materies morbi* from the blood in the urine" is far from new; on the contrary, the crisis of the urinary secretion was familiar to the most ancient physicians, indeed, intimately so to the father of physic himself. Careful and repeated analysis of the urine in various diseases, more particularly in ague and acute rheumatism, having satisfied our author that this ancient doctrine was based, at all events, on truth, he set himself to consider how far it was possible to aid this peculiar blood depurating function which all agree in the main is possessed by the kidney. At the outset of his most interesting and instructive inquiry, Dr Bird draws a necessary distinction between the different diuretic remedies, and divides such into ordinary diuretics or *renal hydragogues,* and depurating or chemical diuretics, or *renal depurants.* To the former class belong such remedies as squill, copaiba, broom, juniper, guaiac, cantharides, &c., which increase the water discharged by the kidneys, and are, as is well known, very frequently most valuable in enabling us to effect the removal of dropsical accumulations. If the quantity of solids be estimated in urine secreted under the influence of any such direct diuretic as those just mentioned, the researches of both Dr Bird and Professor Krahmer (in Heller's *Archiv.*, December 1847) prove that their amount will be found seldom to exceed, and often to fall short of, the usual average. It is otherwise under the action of the renal depurants. This class includes the alkalies, their carbonates and their salts, with such acids as in the animal economy are capable of being converted into carbonic acid, including the acetates, tartrates, citrates of soda and potass. These remedies have a like effect with the former class, in that they stimulate the excreting function of the kidneys, and manifestly increase the amount of urine voided; but they do far more,—they increase, and often to a very considerable extent, the solid ingredients of the urine. As to the manner in which the chemical action effected by this class of diuretics is brought about, we shall quote Dr Bird's exact words. "It is well known," he writes, "that alkalies and their carbonates readily dissolve albumen out of the body, and even break it up into various secondary products—thus, digested with an alkali, albumen yields leucine, protid, and erythroprotid (bodies allied to gelatine), formic acid, and other compounds. In like manner caseine is broken up into tyrosine, leucine, valerianic acid, and other elements. It is exceedingly probable that some such changes occur in the living organism itself; and we should almost expect to find the chemical diuretics effecting important changes. This," adds Dr Bird, "I have repeatedly confirmed by absolute experiment." In one instance, in which the acetate of potass was the depurant employed, the result of the analysis shewed that after deducting the excess of the amount of soluble salts, arising from the conversion of acetate into carbonate of potass, in its passage from the stomach to the kidneys, the solids of the urine separated from the blood, under the influence of the remedy, exceeded those excreted without its aid by no less than 100 grains. Dr Bird further found that though a considerable increase had taken place in the uric acid and urea, it was the extractive matters which were chiefly augmented, in other words, the creatine, creatinine, uroxanthine, and other matters rich in sulphur. We cannot avoid giving this farther interesting reflection in our author's own words. "As it cannot be denied that vital force is ever active in opposing the chemical changes to which all living fibres are obnoxious, it is fair to assume that this resistance will increase with the vital endowments of a part, or, in other words, that the elements of our frames resist chemical influences in the ratio of their vitality. It would follow that such constituents of our bodies as present the greatest departure from health are less highly vitalized, and thus would be expected to yield the easiest to the influence exerted by the alkaline diuretics or renal depurants."

Here we must stop, though more willingly we would transfer the whole of this most interesting and most suggestive chapter to our pages. We intreat our readers to study it for themselves, and, acting upon our author's recommendation, to give trial to the acetate of potass, and the other remedies named, in the treatment of that numerous class of diseases in which, whatever may be the essential, whatever the chemical nature of the *materies morbi* in the blood, we know that *there* it certainly exists.

We feel confident that all who peruse the concluding chapter of this work will join with us in the hope that (notwithstanding the expression of his own fears to the contrary) the health and vigour of its accomplished author being fairly recruited, he may yet enrich the science he has so ardently cultivated, and add to his own reputation by the publication of his observations in the more extended form he had at one time contemplated.

ART. V.—*On the Prevalent Treatment of Disease.* Two Lectures delivered in the Theatre of the Royal College of Surgeons in July 1853. By FREDERIC C. SKEY, F.R.S., Professor of Anatomy and Surgery to the College, &c., &c. London, 1853. 8vo, pp. 68.

The author of this Book is Professor of Anatomy and Surgery to the Royal College of Surgeons of England. It consists of two lectures delivered at the Hall of the College. Thus that Body, standing Godfather to this production, has unwittingly committed itself to sentiments disclaimed, we are sure, by the Fellows at large. The College is still in the infancy of its patronage of Therapeutics. It was therefore doubly unwise to expose itself to the risk of having the authority of its name pledged to hap-hazard lucubrations, offered by a volunteer, in a line of speculation the remotest possible from the proper business of his chair. The issue is such as might have been anticipated. Mr Skey boasts of his success. The College, on the other hand, must feel galled and wonderstruck that its Professor should not have felt in time the force of the "*ne sutor ultra crepidam.*"

It is a prevailing persuasion in the medical world that before the date of these lectures (the month of July 1853), some few men had arisen in medicine of a practical turn of mind, whose precepts for the treatment of diseases had conferred a benefit on mankind. Mr Skey is not of the number who entertain such an idea.

> "This important subject may be expressed in the following question, viz., 'How far is it compatible with the soundest principles of pathology to combat disease by the agency of depletion?' It can, I think, be scarcely denied, if we survey by such observation as intercourse with the professional community has furnished us, that this principle is largely dominant in the world; nor can I entertain a doubt that a principle so universally prevalent, so almost exclusive, is the product, not of conviction, but is adopted as the legacy of antiquated ignorance, supported by prejudice and by habit, and by an unjustifiable reliance on the opinions of our forefathers."—(P. 1.)

Again:—

> "But why, in the name of truth and reason, and of independence, we are to allow the doctrines either of an antique or even of a medieval time to usurp the authority of the present age on a science in itself especially modern, which owes nearly everything to our own times—I mean the science of disease—I am at a loss to tell otherwise than by an appeal to credulity and to a discreditable subserviency to the authority of those who have lived before us."—(P. 2.)

Again:—

> "To dare to think and to act for himself, against the recognised doctrines of the majority, argue the bold and independent man, and few men can boast these requisites."—(P. 3.)

Mr Skey is a rationalist in the strictest sense of the word—he is a worshipper of pure reason in medicine. The past affords him nothing; he looks inwards upon himself and extracts from his own brain a system of treatment with as much ease as a caterpillar or a spider spins out a web from its own body.

> "There is one thing quite certain, viz., that you cannot reduce the circulation by any one or more of the ingenious and often-resorted-to devices which our profession wields, without reducing at the same time the tone and the vigour of the nervous system. Tonic health demands for the fulfilment of the highest functions the combined force of the circulating and nervous systems in their most tonic condition.

Reduce these powers, and the functions of life fail. Maintain them in all their strength, and we shall not only *cure* disease, but we shall avert the liability to its occurrence."—(Pp. 6, 7.)

Again :—

" I presume it to be an infallible law of nature that health and strength are *harmonious*, *co-existent*, and *mutually dependent;* that health and weakness, strength and disease, are incompatibilities ; that as health and disease are also incompatible, so the most efficient principle of treatment that medical science could adopt is one which would most effectually build up, nourish, and invigorate the vital powers as an antidote to the disease ; to oppose and to thwart the antagonism of debility by the administration of such means as give tone, and vigour, and health, and strength to the system. This ought to be the broad basis of medical treatment throughout the whole range of disease, the true philosophy of medical science."—(P. 32.)

Mr Skey is a surgeon by profession. His observation of the maladies lying in the physician's province is incidental, and consequently his knowledge of the entire range of deviations from health is the reverse of profound. He tells us, indeed, that for some twelve years he has been engaged in prescribing for a third portion of the out-patients of a large institution, so that, " during that long period he has been employed in the management of many, many thousands of persons." We understand by this statement that his experience in medicine, properly so called, has consisted in prescribing for such trifling maladies as permitted the patients to walk through the streets of London to seek advice ; and as to management, we should like to know what proportion of the patients returned a second time to shew themselves, and how many finally came back to say they were really cured. We almost suspect that our author never saw a case of simple acute inflammation in a vital organ. Throughout these Lectures, what he says is often very much to the purpose as respects chronic diseases, but then he is all the while speaking of the acute. There was nothing so likely to create such a fundamental error as his exclusive out-patient practice.

The poverty of Mr Skey's knowledge of practical medicine we track through almost every page. He continually reminds us of some parish Lady Bountiful, who looks with contempt on the village Doctor, for he sometimes loses patients—she never. So must it ever be with amateur physicians like Mr Skey. Into the reality of a sick-chamber they seldom get admission. In the acute and actually dangerous diseases of vital organs, they would make quick work with their bottles of brandy and tumbler upon tumbler of mulled port.

Our author comes forward avowedly to correct the erroneous practice of the day, and the first lecture is specially devoted to shewing the folly of the detraction of blood in inflammatory diseases. Among his other discoveries, Mr Skey has found out a new mode of dialectics. His argument exhibits a mode of reasoning hitherto unknown. We shall only characterize it at present as being the reverse of the *à fortiori*.

But let us first glance at the state of things which he assails. There are certain acute inflammatory diseases which in past times have been very generally admitted to be beyond other such diseases, for the most part, under subjection in particular to bloodletting, while there are not a few other such diseases which, from the earliest records of medicine, have been observed to be, at all times, more under the control of other remedies, sometimes of an opposite description, and only at occasional periods to be at all compatible with actively depletive treatment.

Now, if Mr Skey had not disdained to follow the established rules of argument, when the magnanimous idea arose in his mind to put the medical world right as to the folly of bloodletting as a general remedy in inflammation, he would have sought evidence of its inefficacy in those inflammations which by almost universal consent have been conceived to be especially under subjection to that remedy. The plan he actually adopts is quite the contrary. He selects those inflammatory diseases which are always capable of being treated advantageously without bloodletting, being only at certain periods of a character to admit of that remedy at all, and having shewn that sometimes these diseases do not require bloodletting, sometimes are better treated by an opposite description of remedies, he triumphantly concludes that, " the blood is notoriously the great agent of life,—without it life would cease ; therefore, by removing or diminishing the quantity of blood, we are really abstracting the first element of life."—(P. 14.)

Our readers may fairly suspect us of misrepresenting our author, but we appeal to the book itself. We assure them in the meantime that, in an argument against bloodletting, no mention is made of pneumonia, pleuritis, pleuro-pneumonia, the diseases generally believed to be particularly under subjection to bloodletting in their earliest stages, nor even of acute hepatitis or acute enteritis, in which the effect of bloodletting is somewhat less striking ; while he dwells on the inefficacy of bloodletting in erysipelas, in the diffused inflammation consequent on puncture in dissection, in phlebitis in ophthalmia, in hysteric pains, and in cephalalgia. Now nobody of any authority disputes the fact that bloodletting is very generally inapplicable in all these last diseases. His conclusion extends to all inflammations without any exception, yet his instances do not include any of those inflammations which lie exclusively in the physician's department. Under this new mode of reasoning, we do not know what extravagance can fail of proof. Because the diseases under the head B are unnecessarily treated by bloodletting, therefore the diseases under the head A, as well as those under the head B, are unskilfully treated when bloodletting is employed. Mr Skey doubtless, with the new light which he possesses, will reply,—the diseases under the head A are termed inflammations as well as those under the head B, and therefore it correctly follows, on the proof as to the inefficacy of depletion in those under B, that it is inefficacious also in those under the head A. In reply we simply ask him to consult the masters of reasoning in our schools, if what is true of one order be necessarily true of every other order in the class to which it belongs.

We have said our author makes no mention in his argument of the diseases lying specially in the physician's province, though included in his conclusion; but as we find the term pneumonia occurs in a footnote, a word of explanation is necessary. Our author seems for a moment to doubt if erysipelas be a good type of internal inflammation in general, and even hesitates to condemn the lancet in puerperal peritonitis; but quickly getting over this qualm, he ascribes the present rarity of peritoneal inflammation after surgical operations in the London Hospitals to the more sparing use of the lancet. Then comes the footnote in question, which we quote entire, not feeling our belief in the efficacy of bloodletting in active uncomplicated inflammation of the lungs in the least endangered by the admission that severe hemorrhage, after an operation, might be among the concurrent causes both of the disease and of the fatal event in a subsequent attack of pneumonia.

> "The observation of Mr Guthrie points in the same direction. His cases of pneumonia, treated by large bleeding, were always more protracted and more unsatisfactory than those in which the abstraction of blood was moderate. A man's leg was amputated above the knee at St Bartholomew's Hospital. He progressed favourably for three weeks, but the stump did not progress. He then had an attack of hemorrhage and lost a considerable quantity of blood. Within five

days he died of *pneumonia.* The peculiar features of this case may be striking, but the principle is universal."—(Footnote, pp. 12, 13.)

We cannot guess from what sources Mr Skey draws his belief as to the practice of the day, for while we admit that there are, and always have been, men in the profession apt to run away with the prevalent fashion, we positively deny that the prevalent fashion at present is to carry bloodletting to excess. We are sure the reverse is the case at this moment. In short, we believe there hardly ever was a time when there was less disposition in the medical profession at large to push bloodletting to extreme than at present. There never was a time when Mr Skey's two lectures were less called for. By coming forward at such a period to speak authoritatively from the professorial chair of the College of Surgeons against depletion, he proves himself to be one of those short-sighted mortals, numerous in all departments of life, who cannot let well alone, but who must be always rushing from one extreme to another, incapable of keeping the middle way.

That bloodletting has at various periods been carried too far in the treatment of diseases is indisputable, but of what remedy can the same thing not be said with too much truth? And who are the men so disposed to push the remedy of the day to a hurtful extreme? Who but those who like Mr Skey despise the wisdom to be gathered from the history of medicine; who, instead of carefully studying the recorded precepts of the men in all past times possessed of the medical mind in its most exquisite form, as the only true key to the interpretation of the perplexities of clinical observation, impiously set up their own small judgment, exercised within some most limited scale, as the universal standard of medical truth. Mr Skey's twelve years' experience among the third part of the out-cases of a London institution is a ludicrous example of the kind of foundation on which self-sufficient rationalists rest their claims to overturn all that has grown up, under the directive influence of men plainly born to be physicians, out of the ever-varying character of human maladies, throughout long ages, and over all the accessible regions of the globe.

The utility of bloodletting in certain circumstances of acute inflammation does not rest on reasoning but on experience. These circumstances respect its seat, its acuteness, its extent, and the attendant constitutional excitement, and when several of these circumstances concur, ample experience proves it to be both safe and useful.

Our author will say he has not witnessed this in his dispensary practice.

Mr Skey, and those who join him in this answer, of course reject the belief so well founded, and so universally received by the great fathers of medicine, that the type of diseases, particularly as respects the violence of their reaction, when acute, and consequently their subjection to bloodletting, is liable to variation, each variation sometimes extending over long periods of years. It is easy to refuse this belief; but is it equally easy to shew its improbability? If it be true, it is a great first principle in the treatment of diseases. If it be not true, the past history of medicine can afford no practical lesson, and the treatment of acute diseases has no better claim to stability of principle than the present cut of our coats has to being the universal standard of taste in dress.

We admit that acute inflammations of vital organs often recover perfectly without bloodletting, but we do not admit that no mischief is ever done in our days by the so prevalent abstinence from bloodletting. We maintain that the history of medicine shews clearly that cases of such inflammations as were enumerated above have often been successfully treated by copious bloodletting. But we neither deny that it was often carried too far by the over-zeal of less judicious practitioners, nor do we bind ourselves to retract our judgment of its effects in past times, even were it ascertained that bloodletting is, in all time coming, to be confined within much narrower limits than of old. Judging of the future from the past, we feel assured that the time will come round when acute diseases will shew a tendency to higher constitutional excitement than is common at present. Nevertheless, we believe there are remedies now known which supply in no small measure the place of bloodletting; and farther, we are not unwilling to think that in the civilized parts of the world the tendency to high excitement under acute diseases has from a variety of influences become permanently diminished, so that the causes on which periodical tendencies to high excitement are dependent may be thereby counteracted to a degree unknown in former times.

In short, while we condemn the terms in which Mr Skey speaks of the practice of a past age, as implying unmerited censure on our predecessors, we see reason to predict that bloodletting will never again be carried to such an extent as at some earlier periods in the history of medicine. Mr Skey's persuasion, however, it should be explained, is, that he is reviling, not a practice of a bygone period, but one of the present day. Our readers, we are sure, will agree with us that it is long since English practice in reference to bloodletting ceased to merit such a remark as the following; the remark stands under bloodletting.

> "There is hardly a nation throughout the world that has not a sneer at our English practice; and I doubt whether I am not right in affirming that the lavement and ptisan system of the French school, inefficient as it is in the treatment of active disease, has led to one tithe of the mischief which may be attributed to the universal system of depletion adopted by the school of England."—(P. 30.)

No one will suspect that the author of this paragraph ever heard of the *coup sur coup* practice of M. Bouillaud, and of the large quantities of blood drawn by some other distinguished French physicians, any more than he can learn from these Lectures that in proportion as excessive bloodletting has declined on this side of the channel, it has gained ground in France.

We will not enter upon Mr Skey's observations on depletion by purgatives, farther than to say that we believe such remedies have often been carried too far, and that we think this remark had been made by many before these Lectures appeared. We are convinced that at present the excessive use of purgatives is very generally on the decline, and that the judicious use of these important remedial agents is the rule and not the exception in the profession. In so large a body as the medical profession, we know there must be no small number of less judicious practitioners; but we have a better opinion of our brethren than Mr Skey. We are satisfied that the great body of medical men successfully endeavour to adapt their remedies to the varying character of diseases and constitutions in the cases coming under their care; and we judge of the practice of the day, not as our author seems to do, by the number of errors committed by the injudicious, but by the multitude of instances, which every person who takes the trouble to open his eyes may discover, continually coming to light of men in every grade of medicine, applying the resources of the art with judgment guided by a large acquaintance with diseases and remedies.

Of his boasted tonic treatment of acute diseases, we will only say in the proverbial words ascribed to a late distinguished physiologist, that here "what is new is not true, and what is true is not new."

We will not deny that there are some good observations scattered through these two Lectures, but these do not redeem the book from our condemnation. It is a bad book; it is unmedical in its spirit.

PART III.

SUMMARY OF THE PROGRESS OF MEDICAL SCIENCE.

MIDWIFERY.

Case of Pregnancy in a rudimentary horn of the uterus, with probable advance of the ovum from the right ovary into the left horn of the uterus. By Professor SCANZONI.

In his handbook of Pathological Anatomy, Professor Rokitansky describes an unique preparation, in the Viennese anatomical collection, exhibiting a pregnancy in a rudimentary uterine horn. The example of Professor Scanzoni is an interesting addition to this extraordinary class of cases.

The woman generally enjoyed good health, and was regularly menstruated. She was married in her 28th year—had a miscarriage of twins five months afterwards, and subsequently bore three children, which still live. In her last two pregnancies she tried by violent exercise to induce premature labour. During the first half of all her pregnancies she suffered much from vomiting, toothache, heartburn, and œdema of the lower extremities.

In July 1852, she conceived for a fifth time, and again used every means to induce premature labour. No disorder of general health occurred in this pregnancy. On the 21st Nov. she had a quarrel with her husband. The same evening, and also on the next, she complained of slight colicky pains in the left hypogastric region, which however did not prevent her from going about. But she soon became very weak and had to be carried to bed. This was at ten o'clock forenoon. At midnight Dr Scanzoni visited her and diagnosed an extra-uterine pregnancy with profuse internal hemorrhage in consequence of the rupture of the containing sac. She died almost immediately afterwards.

At the post-mortem examination, there was found in the lower half of the abdomen a great quantity of variously altered blood. After its removal there was observed on the left side a round swelling of about the diameter of 3½ inches, with a laceration on its outer border. In it were contained the membranes and placenta of the fœtus, along with coagula of blood. The cord was six inches long. The corpus luteum was in the right ovary. Both Rokitansky and Scanzoni, on first viewing their cases, thought they had to do with tubal pregnancy, and only on further examination discovered the true nature of the cases. Such cases are described by Rokitansky as an intermediate link between uterine and tubal pregnancy.

Scanzoni's case is interesting physiologically, as belonging to a series where the corpus luteum is formed on the ovary of the side not corresponding to the uterine horn or tube which is pregnant. Such cases at once suggest the idea that the ovum must have performed a long journey from the ovary of the one side, through the uterus, and into the passages belonging to quite the other side. Scanzoni states that many analogous observations have been made in the lower animals, but he is mistaken in supposing that his own is the first in the human subject.—*Verhandlungen der Phys. Med.; Gesellschaft in Wurzburg*, iv. bd. 1853.

The Inner Surface of the Uterus after Delivery. By Dr Matthews Duncan.

It has hitherto been generally taught that in parturition there passes away with the expelled ovum, or after it, the entire decidual membrane, leaving the muscular or proper structure of the uterus bare over the entire inner surface of its cavity; and, in contradiction at once to observation and analogy, that over this surface a false membrane is formed, beneath which there springs up an entirely new mucous membrane. But the investigations of the author have convinced him that in the healthy female the muscular tissue of the uterus is at no time laid bare. After parturition the remains of the uterine decidua are left covering this surface. At the site of the placental insertion this membrane appears thicker than elsewhere, and presents numerous elevations and depressions, and also the open mouths of the utero-placental vessels, which have been as it were cut across by the separation of the afterbirth.

This membrane is distinguished from the muscular tissue by its softness, its colour being different in a cross section, and its microscopical characters.

As the uterus diminishes in size in the progress of its natural involution this membrane increases greatly in thickness, and becomes more and more easy of demonstration. At the same time the muscular tissue underlying it diminishes rapidly in thickness. The large veins become obliterated, and in five or six weeks the womb has resumed its unimpregnated condition.—*Brit. and For. Quarterly Rev., October* 1853.

Phlegmasia Dolens in a Virgin. By Dr M'Clintock.

This case differs from most such cases, in being purely idiopathic, the female having had no attack of metritis, suppression of the menses, or any other disease of the uterus. It was that of a young lady, aged eighteen, recently suffering from scanty menstruation and tendency to chlorosis, consequent on a change of residence from the country to the town. The attack was caused by exposure to cold in standing for two hours upon damp grass. Treatment was delayed for three days, at the end of which time the symptoms were found fully developed. Leeches were then applied over the femoral vessels. The entire limb was constantly stuped, and she was enjoined rest and low diet. Under this management the acute symptoms subsided in a week. One relapse took place, and the limb was many months of regaining its ordinary condition or usefulness.—*Dublin Quarterly Journal, November* 1853.

The Secretion of Milk in Newly Born Infants. By Dr N. Guillot.

Although the existence of this secretion in infants has long been known, and has, in fact, been interwoven with some old and absurd theories of the mode of nourishment of the intra-uterine fœtus, yet our author believes that the subject has been lost sight of, or very incorrectly viewed by most physicians.

After birth the mammary gland of both male and female children takes on an extraordinary activity, which is occasionally seen again at puberty in both sexes, but never again occurs except in the female after parturition. At all these times the phenomena are identical in character. The glands swell and secrete a peculiar fluid.

Newly born children are divided, in regard to this subject, into two groups. The first includes all those which furnish milk when their mammæ are compressed. The second includes all those who do not.

All healthy children, born of robust and well-fed parents, generally secrete milk a few days after birth. Children born of unhealthy, syphilitic, tubercular parents, or which are themselves ill-nourished or unhealthy, present sometimes traces of this secretion, but generally not even this.

The secretion presents the same general characters in the child as in the adult. But it does not continue long in the child, and disappears whenever it gets into bad general health or loses weight. It generally commences after the separation of the cord, and lasts from seven to twelve days.

Our author states five interesting cases of abscess of the gland in connection with the miniature lactation. From them it appears that from the twelfth to the fifteenth day of life the mamma is liable to become inflamed. This inflammation is generally preceded or accompanied by suppression of the secretion of milk on the affected side. After this come the ordinary symptoms of suppuration in the gland. Then there come to be felt one or several points of fluctuation in the substance of the gland. These separate abscesses either open at different points, or unite together and are discharged by one opening.—*Archives Generales de Med. Novembre* 1853.

2. SURGERY.

Treatment of Aneurism by Compression.

In the "Medical Times and Gazette" of October 29th, and November 5th and 12th, a very interesting series of cases of external aneurism is given, which have been recently subjected to surgical treatment in the metropolitan and some of the provincial hospitals. These cases amount to twenty-five in number, and are nearly all well and accurately reported. The details of the cases furnish much information, which is very desirable for a prôper estimation of the comparative value of the modes of treatment by ligature and by compression, as well as in determining the cases in which the one or other mode of treatment is preferable.

The list comprises 23 cases of aneurism of spontaneous and 2 of traumatic origin. The seats of the aneurisms of spontaneous origin were as follows:—Popliteal artery, 19; femoral, 3; radial, 1. The traumatic aneurisms were situated in the femoral and in the anterior tibial arteries. In one case a popliteal aneurism existed in each limb; and in one, double femoral aneurism existed in one artery. One case only (popliteal) occurred in the female sex.

Of these 25 cases, 23 were submitted to treatment by compression, which succeeded in curing the disease in 14 and failed in 9. Ligature of the artery without previous compression, was followed in only one case, and with success. In the cases in which compression proved successful, the artery was compressed during periods varying from three days to upwards of five months. The duration of the compression till the tumour was solidified in the different cases was as follows:—3 days, one case; 4 days, one; 8 days, three; 11 days, one; 15 days, one; 21 days, one; a month, one; 6½ weeks, one; 10 weeks, two; 15 weeks, one; and 23 weeks, one. In the case of double popliteal aneurism, the cures were effected respectively after 4 and 8 days' compression.

The treatment by compression was abandoned in six cases of aneurism of the popliteal, in two of the femoral, and in one of the radial artery. The causes which led to the abandonment of the compression were nearly the same in all these cases, pain and constitutional disturbance, with œdematous or erysipelatous swelling of the limb, and increasing size of the tumour. The result of the ligature in these nine cases was that six were cured, one (a case attended by unusual peculiarities) remained *in statu quo*, and two died, one from subsequent suppuration of the sac and of the knee-joint, the other from gangrene, from the femoral vein having been wounded in the operation.

In one of the cases of traumatic aneurism (that of the anterior tibial artery) compression was successful in curing the disease; in the other, (that of the femoral artery, alluded to as a case of a peculiar kind) neither compression nor the ligature effected any change on the condition of the tumour.

This short abstract proves, in the first place, the very general confidence which now appears to be placed in the treatment by compression, or at least the general anxiety on the part of surgeons to test its merits. Very great attention and perseverance appear to have been bestowed on the treatment in nearly every case. The apparatus employed in Dublin (Carte's) was that almost invariably applied, alternated in some cases with the employment of manual pressure, or of a weight placed on the artery. The pressure was variously borne, some of the patients complaining little of its irksomeness, others passing sleepless days and nights and insisting on occasional intervals of relief from all pressure. The treatment by compression was not attended by any fatal result, nor, so far as we can perceive, by any decidedly mischievous consequence. The number of cases in which it was found necessary to abandon compression and to resort to the use of the ligature is considerable (about 40 per cent.); but the proportion of cases in which the cure was effected after a very short period of compression is greater, we think, than in any series of cases hitherto recorded,—and it is worthy of remark that the tumour was of comparatively small size in all the cases in which a few days of compression sufficed for the cure.

Death followed the application of the ligature in two cases of the nine in which the vessel was tied. In one of these suppuration of the sac and of the knee-joint led to the fatal result. The case does not appear to tell against the use of the ligature; for the operation was performed after the artery had been subjected to pressure for a fortnight, during which time the tumour had increased in size, the thigh had become œdematous and the patient had suffered much pain. The advocates of the use of the ligature might tell us, and with some justice, that the ligature of the artery in this case at first would probably have saved the patient, and that the result was due to the unfavourable state of the limb produced by the compression, and to the progress which the tumour had been allowed to make during the period of its employment. The second fatal result was attributable to the vein being wounded in the operation, and we think that the possible occurrence of this accident (which has many times happened) forms to a certain extent an element in the question of preference of the one or other method.

The perusal of these cases certainly tends to increase our confidence in the treatment by compression, as a means of cure which should, in the majority of cases, supersede the use of the ligature in the treatment of popliteal and femoral aneurism. Their history, too, confirms our opinion as to the class of cases in which compression is especially applicable, viz., those where the tumour is small, where it contains a considerable quantity of solid matter, and when it is not making rapid increase in size. It is true that compression has effected a cure in many cases in which these favourable conditions were not present; but we think that the general result of such cases does not present sufficiently favourable evidence to establish the superiority of the treatment by compression over that by ligature, except in the class of cases we have mentioned.

Intra-Cranial Suppuration following Scalp Wounds.

We extract the following excellent observations from a paper by Mr Prescott Hewett (published in the *Medico-Chirurgical Transactions*, vol. xxxvi.), consisting of an "Analysis of the cases of Injuries of the Head, examined after death in St George's Hospital, from January 1841 to January 1851."

"Suppuration between the bone and the dura mater appears not only to have been much more frequent, but also very much more commonly confined altogether to this situation formerly than it is in the present day. In the second section of 'Injuries of the Head,' Pott gives twelve cases of scalp wound followed by subsequent mischief beneath the bone, in seven of which the suppuration was altogether confined to the outer surface of the dura mater. . . . . In every one of the eight cases of suppuration between the bone and the dura mater occurring in this decennium, puriform effusion was found within the cavity of the arachnoid, and appeared to have spread from the external to the internal parts. That this difference does not depend upon locality is evinced by the results of similar cases in different places and in different countries. Suppuration between the bone and dura mater naturally leads to a practical question of the utmost importance, I mean the application of the trephine. Formerly, and especially in Pott's hands, it appears in such cases to have been one of the most successful operations. . . . . It has never yet fallen to my lot to see a single instance in which the application of the trephine has, under such circumstances, had a successful issue. In every case in which I have seen the operation performed, the patient, notwithstanding the evacuation of the matter, has died of diffuse inflammation of the membranes. . . . . We consider the application of the trephine altogether useless in cases of intra-cranial suppuration, where the symptoms are those of acute inflammation of a diffuse kind, in which the signs of compression are generally but slightly marked. . . . . . . Purulent infection was observed in fourteen out of the twenty-three fatal cases of scalp wound, and, although developed in other injuries, in none is it more frequently so than in those of the head, and that too in cases where the injury has apparently been of a trivial nature. The well-known fact that this disease is found especially in injuries involving the osseous system, will serve to explain the frequency of the development of this most formidable complication in accidents about the head, where the bones are not only abundantly supplied with cancellous tissue, but where are also found venous canals much larger and much more numerous than in any other part of the skeleton."

We have had frequent opportunities of verifying the facts contained in these observations. Mr Hewett's admirable analysis will have the effect of increasing still further the growing distrust of surgeons in the efficacy of the trephine as a remedy in such cases as Mr Pott describes to have been so successfully treated in his hands by perforation of the cranium.

Minie Rifle Ball Wounds.

The new era in the history of warfare produced by the introduction into general use of the deadly Minie rifle will doubtless give rise to renewed discussions among military surgeons on the *quæstio vexata* of the characters of the wounds of entrance and of exit. We should, *a priori*, expect that the wound produced by the conical rifle-ball would be of a less formidable character than that of the round musket-ball.

The war in the East promises to elicit pretty extensive information on this subject; but we have the first instalment in some "Contributions to Clinical Surgery and Medicine," in the *Dublin Quarterly Journal of Medicine* of November 1853. A lad aged seventeen received a wound on the dorsum of the foot from one of these conical balls, discharged, we believe, at a distance of several hundred yards by one of a regimental practising party. A single wound existed, but it was thought the ball might have bounded back through this opening! "been rejected by the elasticity of the metatarsal bones!" A fulness, however, existed in the sole of the foot a little behind the ball of the great toe. "The treatment consisted in the action of the water battery (?) on the foot, and the administration of the anti-tetanic *pile.*" (?) . . . "consisting of a grain of calomel, one or two grains of James's powder, one grain of aloes, one of ipecacuanha, and an eighth or a quarter of a grain of opium." This formula was suggested by Mr Peile, and is said to be "a most valuable medicine in lacerated, contused, and punctured wounds." What is the meaning of *the action* of the water *battery*?" We presume the simple application of cold water dressing probably by means of the syphon. But what is the meaning of "the anti-tetanic *pile?*" Is it a misprint for *pill?* or is the term used in deference to the gentleman who suggested the remedy? or is it intended to be suggestive of the hemorrhoidal tendency of the calomel, antimony, and aloes, which form the *pile?*

Let us follow the treatment of the case, however; it was resigned to the care of a very expert military surgeon (Mr Tufnell), who adopted the more certain anti-tetanic treatment of cutting out the ball by an incision in the sole of the foot.

The author prefaces the account of the case by some remarks on the character of the entrance and exit wounds, on which we hoped the communication would have enlightened us to some extent, but we are only tantalized by the concluding suggestive question to which no answer is vouchsafed, "*Had it traversed the foot* what *would have been* the form and appearance of the exit?" The smallest "contribution," however, is thankfully received, and we have to record the fact, that the entrance wound "presented more the character of what I may term a clean lacerated slit about one and a half inch long."

3. PATHOLOGY AND PRACTICE OF MEDICINE.

Case in which Sarcina Ventriculi was found in the fluid removed from the Ventricles of the Human Brain. By Dr JENNER.

The peculiar microscopic object first described by Professor Goodsir, and by him named sarcina, has been found not only in adults and children, but also in the lower animals, in the rabbit, dog, and tortoise. The most common circumstances under which the sarcinæ have been detected are in the matters ejected from the human stomach and intestinal canal. Haller and Mackay have detected sarcinæ in the urine, and by Virchow and Zencker they have been found in the lungs; cases upon which some doubt has been from time to time thrown, but whose reality is confirmed by the recent observations of the first-named author. Dr Jenner's case is thus succinctly recorded.

A boy aged four years was admitted under my care into the hospital for sick children, June 24th, 1852; he died July 3d. The child was of fair complexion, well made, and moderately stout. His illness was suspected to have commenced on June 17th with pain in the head. He did not keep to bed till the 23d. After his admission to hospital the most prominent symptoms were drowsiness, talking in sleep, and irregularly diffused scarlet rash on the skin, redness and swelling of the tonsils, with white patch on the left, frequent pulse, sordes about teeth, some dryness and brownness of tongue, almost incessant grinding of the teeth. Between the 30th of June and the 1st of July, the day of death, restlessness, strabismus, inequality of the pupils, and redness of the conjunctivæ. Convulsions preceded and accompanied dissolution. The body was carefully examined ten and a half hours after death, and next day the fluid removed from the lateral ventricles of the brain was made the subject of particular microscopic research. The following characters were presented by the fluid :—It turned turmeric paper brown ; its odour was sickly, brain-like, not ammoniacal ; it seemed as if just beginning to be decomposed ; there was a little sediment at the bottom of the vessel. A drop containing some of the sediment was examined with a magnifying power of about 200 diameters ; the following objects, and these only, were contained in it :—

1. A considerable number of spherical bodies, $\frac{1}{3000}$th of an inch in diameter, of a pale-yellowish colour (blood discs altered in form ?)

2. Square bodies, each of which measured $\frac{1}{2000}$th of an inch, some a little larger than this, others a little smaller ; the surface of each body was divided by cross lines into four equally-sized compartments, and each of these quarters was again divided into four (sarcinæ).

3. Oval bodies about $\frac{1}{5000}$th of an inch in breadth, and $\frac{1}{2500}$th of an inch in length, distinctly divided by a transverse line into two equal parts ; each of these two parts being very obscurely divided by a longitudinal line into two other parts.

The following questions suggest themselves :—Were the sarcinæ in this case developed after the fluid in which they were found was taken from the cerebral ventricles ? Were they developed after death but within the body ? Did they exist in the ventricles during the life of the child ? To whichever of these questions the affirmative is answered, the occurrence of sarcinæ under the condition mentioned is remarkable.—*British and Foreign Medico-Chirurgical Review.*

On the Characters of Urine depositing Oxalate of Lime. By Dr Douglas Maclagan.

1. The mean density of all the specimens of urine referred to in this interesting paper is 1024·4 ; the specific gravity is, therefore, somewhat above the natural standard. The observed difference of density between the morning and evening urines is confirmed by Dr Maclagan's experience, and has reference to the fact that the presence of this deposit in the urine is commonly connected with a disorder, more or less important, of the digestive and assimilative processes.

2. Concerning colour—as a general rule it was paler than the average of healthy urine.

3. Odour—sweet-brier smell frequently; in some instances more or less fetid, never ammoniacal; only a very few times even urinous.
4. Reaction—in general strongly acid.
5. Saline and other concomitants.—In 37 specimens.

	Morning.	Evening.
Oxalates unmixed, . .	16	14
Oxalates with other saline deposits,	16	20
Not examined, . .	5	3
	37	37

Of the saline concomitants the lithates alone occurred frequently; next to them, but comparatively rarely, the uric acid, oxalurates, and amorphous phosphates; and again in almost solitary instances, triple phosphate, cystine, xanthein, pus, sugar, blood, and in six instances a more or less notable reaction of purpurine. Epithelium in the 37 specimens.

	Morning.	Evening.
Copious, . .	12	10
Trifling, . .	20	24
Not examined, .	5	3

As a general rule, the amount of oxalate is much greater in the evening than in the morning urine. Such is the experience of the author in regard to the characters of oxalic urine; for many excellent remarks upon the pathological importance of the subject we must refer to the paper itself.—*Monthly Journal of Medical Science, December* 1853.

Cholera successfully communicated to the Lower Animals. By Dr Lauder Lindsay.

This gentleman, the resident physician to the Cholera Hospital in Surgeon Square, has during the last few months been engaged in conducting a series of experiments on the propagation of cholera from the human subject to the dog; and has at length succeeded in producing the disease simultaneously in three dogs and a cat. Two of the dogs died rapidly with all the symptoms of cholera, and examination of their bodies after death revealed the characteristic appearances of the disease. The interesting narrative of Dr Lindsay's researches, at present unavoidably postponed, will be presented to our readers in the next number of the Journal.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

1st APRIL 1854.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Illustrations of Gout and the Gouty Diathesis.* By James Begbie, M.D., F.R.S.E., Fellow of the Royal College of Physicians, Physician in Ordinary to the Queen in Scotland. (Continued from vol. lxxxi., p. 21.)

> " Knots upon his gouty joints appear,
> And chalk is in his crippled fingers found."—Dryden.

Heberden begins and ends one of his admired commentaries (chap. xxviii.) as follows:—" What are those little hard knobs, about the size of a small pea, which are frequently seen upon the fingers, particularly a little below the top, near the joint? They have no connection with the gout, being found in persons who never had it; they continue for life; and being hardly ever attended with pain, or disposed to become sores, are rather unsightly than inconvenient, though they must be some little hindrance to the free use of the fingers."

Many years have passed since Heberden suggested and answered the question; and few, we believe, of those who have since perused his writings have felt themselves disposed to differ with him in opinion, and propose a different answer. Nevertheless, the question, if we mistake not, admits of such a reply; and those little knobs, which physicians in all ages must have noticed and considered, are in reality intimately connected with gout, though they are often found in those who have never experienced a fit of the disease. They are the product of the gouty diathesis; they are the deposits of the gouty blood; and are not less certain indications of that peculiar habit than the enlarged cervical glands, and irregular cicatrices, which mark the deposit and progress of tubercle, are of the strumous constitution. Both are guides and landmarks to the inquiring and observing physician; both are eminently suggestive as points of diagnosis, and as indications of treatment, not only as regards the individuals themselves in whom these manifestations appear, but as respects the many who may be related to them by ties of kindred blood.

We have again and again traced the rise and progress of these little knobs to their full development; we have seen that progress slow and almost imperceptible from year to year; and we have seen it equally rapid and observable during the course of as many months. They are occasionally the result of an inflammatory affection of the fingers, more or less acute in its character, and attended by the same constitutional disturbance which marks the fit of gout; but more commonly they are the consequences of a slow and chronic disorder, in which dyspeptic derangement has been chiefly noticed, while occasional twitches of pain in the fingers, and a sense of fulness and stiffness there, have been the only forerunners of those morbid products which are destined to last through life. They are deposits in the sheaths of tendons, or in the bursæ or synovial membrane of the joints. They are found chiefly among the upper classes, or among the luxurious and well-fed of their dependents, and are seldom or never found disfiguring the hands of the industrious labourer or of the hard-working mechanic. We have never seen these knobs but in connection with the gouty diathesis; and their presence in the fingers of a patient, or of a blood relation, has often led us to a right diagnosis of some obscure affection, or been the means of throwing light on the nature of some anomalous symptoms which had baffled all our attempts to unravel.[1]

CASE XVI.—A lady aged thirty-five, the eldest daughter of a numerous and healthy family, where death had not yet entered, had for many years suffered from severe and lasting headache, which had resisted the skill of more than one practitioner, and defied the power of many and diverse remedies. She was of full habit of body, and florid complexion; active in household affairs, which chiefly devolved upon her, and accustomed to regular exercise in the open air. Her appearance was continuously that of health, and all the functions were said to be discharged in correspondence therewith. Nevertheless her long-continued headache was of a severe and intractable character; and it required all the efforts of a vigorous and well-regulated mind to sustain her under her sufferings. Among the varieties of headache which have been observed and described by authors, it was difficult to fix on one which bore the characters of that which she presented. It was not nervous or hysteric; it was not inflammatory or congestive; it was not anemic; it was not dyspeptic. It could not be traced to cerebral disease; it was not of neuralgic or rheumatic character. It was not periostitic; it was not periodic. It was not, according to observation, sympathetic of uterine derangement or renal disorder: though a careful scrutiny of the urinary secretion time after time detected the oxalate of lime alternating with the lithates, often to great excess; and the menstrual period, though regular, was often accompanied by the sufferings of dysmenorrhœa. Thus an approach was made towards ascertaining the character of the headache under which the patient laboured. Her father displayed his hands, and his fingers presented the little knobs of Heberden; and farther inquiry led to the confession that he had long suffered from the cardiac uneasiness, and flatulent distension, and acid eructation of the gouty. Her paternal grandfather had been the victim of gout, and died suddenly of heart disease. Has light arisen amid the obscurity which has so long concealed the true nature of this perplexing headache? And can we hope that after bloodletting in various forms, and purging by diverse medicaments, and alteratives of every kind, and counter-irritants, and vaunted specifics, have all miserably failed, there remains any thing to be accomplished by an anti-arthritic treatment? If there was one remedy less unavailing than another in the long catalogue which had been exhausted, that remedy was arsenic. Under its persistent employment for weeks and months relief to some extent was certainly obtained; but the most effectual means were at length secured in the steady employment of saline diuretics combined with the preparations of colchicum. Arsenic appears to possess but a limited influence over chronic affections originating in the gouty diathesis, while over those of a kindred nature connected with the rheumatic constitution its powers are most remarkable. Colchicum, on the other hand appears to obtain a doubtful effect over the purely rheumatic habit, while over that of gout it exercises a striking control. Under the persistent employment of a combination of the powder of colchicum with the nitrate and carbonate of potash, aided by those regulations of diet and exercise which are suitable to the gouty habit, the severe headaches were relieved and ultimately removed; but, unhappily, by discontinuing the remedy too soon, and neglecting hygienic rules, in consequence of severe family affliction, they have again renewed their attack, but in a less severe form, and are, there is reason to believe, soon to yield by a return to the former treatment.

---

[1] These tophaceous deposits to which Heberden alludes differ somewhat from the ordinary gouty chalk stone, though they are essentially of the same family. They are neither so soft and fluctuating in the early stage, nor so hard and earthy-like in the more advanced. They do not acquire the same size which the chalky concretions sometimes attain, neither do they appear to lie so immediately beneath the skin, nor do they ever burst or discharge the thin mortar or gritty chalk of the ordinary deposit.

Let us look a little farther into the workings of the gouty diathesis in other members of the family to which the lady whose history has now been briefly related belonged. Her father, we have seen, inherited the disease, and had long been familiar with the dyspeptic and cardiac derangements which manifest its presence. He was often anxious and dejected, and required to be assured, and reassured, that the palpitations he so frequently suffered, and the cardiac pangs he so much endured, were not symptomatic of organic disease. The irregularity of the heart's action was at times very great, and it continued so for days together, but an interval succeeded, in which in force and rhythm it was perfectly normal. The digestive functions were liable to frequent disorder, particularly after an indiscretion in regard to diet. Flatulence and acidity were the chief symptoms, and along with these appeared the more copious deposit of the lithates, alternating or co-existing with the oxalate of lime. The salts of potash and the preparations of colchicum were the remedies from which he always derived benefit, during the gastric and cardiac disorder, and in the interval the preparations of iron and the nitro-muriatic acid appeared to be beneficial.

During the course of my attendance on this gentleman, while suffering from one of his attacks, I was consulted in the case of his eldest son, of the age of thirty-five, who had been suddenly seized with violent pain in the region of the kidney, with sickness and vomiting, and frequent desire to pass urine. This nephralgic attack yielded to anodynes and warm applications, and was followed on the morrow by the passage of a small calculus, which was found to consist of oxalate of lime. The urine in his case was generally loaded with the lithates, and he presented other features of the gouty diathesis, to the full development of which his habits and mode of life were not unfavourable. On another occasion my attention was directed to the case of a younger daughter of the family, a girl of fifteen, who had been greatly annoyed by a plentiful eruption of an eczematous character over the face and neck, with considerable constitutional disorder. In her the disease yielded when acute to the alkaline treatment in connection with colchicum; and in its more chronic form, to small doses of arsenic.

Again, and all within the course of a few years, I was consulted in the case of an elder sister, who, after a chill caught in attending church, when her clothing had been wet by a passing shower of rain, was seized with acute pain in the right side, with difficulty of breathing, and all the general and physical signs of acute pleurisy. Here, too, after topical bleeding, the alkaline carbonates, with the wine of colchicum, were found effectual in removing the effused fluid, and in rapidly restoring the patient to health. Lastly, in order to estimate aright the influence of the maternal constitution on the health of the offspring, we are called on to remark that the mother of this family, consisting of ten children, had early in life suffered from two severe attacks of rheumatic fever, in which the endocardium was involved, as demonstrated by a distinct systolic murmur over the aortic valves,—a damage, however, which never impaired her health, or was productive of suffering, or danger, till many years had elapsed, when in addition to the diseased left ventricle, now greatly hypertrophied, there existed the signs and symptoms of dilatation of the right chambers, to which might be traced an attack of an apoplectic character, followed by palsy of one side, and soon after by enlargement of the liver and œdema of the extremities. Latterly, when at the age of sixty, a helpless paralytic, with progressive damage of the circulating and respiratory organs, she made a wonderful rally from gangrene of the toes; and died in twelve months afterwards from an attack of pulmonary apoplexy.

In reviewing the events of this family history, so far as they have come under our own immediate notice, and much must have been overlooked and concealed, we are led to conclude that the prevailing diathesis of the family, and the proneness to diseases connected therewith, was inherited chiefly from the father's—the gouty side of the house; the mother, though the victim of the rheumatic constitution, appearing, so far as we have seen, to have given no predisposition to her offspring, to any of the ailments under which we have seen them labour, if we except the pleuritic attack of the daughter —a disease, however, which is intimately connected with gout, though its more ordinary source is rheumatism.

Case XVII.—Mrs N. is now seventy-five years of age. She is descended from a long line of gouty ancestors, and carries with her the marks and indications of the slow, progressive, and silent poison which by inheritance has been secured to herself and family. It is now more than twenty years since I first saw her on a sick-bed, suffering from gastric, hepatic, and cardiac derangement; the latter gave her the greatest distress; and, as usual with such sufferers, raised the dread and apprehension of fatal heart-disease. She has outlived her fears; and now that functional disorder has given place to organic change, she is less concerned about the state and condition of the great organ of life, than the neuralgic pains and crippling nodosities which embitter and embarrass her declining years. She has had no regular fit of gout; but the little knobs upon the fingers, the flying pains in the hands and feet, the irritable stomach, the tumid liver, the over-charged kidney, the fluttering, palpitating heart, and other symptoms, have told for many years past that she has not escaped the fate of her progenitors. She labours now under valvular disease; she has occasional threatenings of an apoplectic character; and she has frequently suffered from the distress and anguish occasioned by the impeded transit or impaction of biliary calculi, and the jaundice which accompanies this condition. She has experienced in many of her attacks the benefits of the salts of potash, and the preparations of colchicum, in combination with iron or quinine; and she has profited largely, especially in regard to her crippling and nodosities, by one of the best remedial agents in chronic gout—cod-liver oil.

Mrs N. is the mother of a large family, whose history in regard to health and disease, as influenced by their parent, it would be interesting to trace; but the pursuits of life exercised, in more instances than one, in distant lands, precludes the full investigation. Four sons and four daughters have grown up to manhood. Of the eldest of the males it is known that after foreign service he returned home the victim of liver disease, and sank under it in combination with pneumonia of the right lung; of the eldest daughter I might record the history of a long train of illnesses originating in marked gouty habit, demonstrated by the little knobs of Heberden, and manifested by the regular fit in the great toe and instep, in the inflammatory affections of many synovial membranes, in the frequent occurrence of more than one variety of cutaneous eruption, and especially in congestion of the liver and functional disorder of the heart; and latterly in constant returns of jaundice in connection with the passage of biliary calculi, during one attack of which, or rather during her convalescence from it, she was seized with epidemic typhus, and died on the tenth day, presenting the true picture of yellow fever. Examination of the body discovered an enlarged and waxy liver, and a distended gall-bladder, in the duct of which was found a biliary calculus of considerable size, while three or four more occupied, along with dark viscid bile, the gall-bladder itself. The heart was pale, and its cavities dilated, and the coats of the aorta, also dilated, presented here and there thin scales of atheromatous degeneration.

A younger daughter of Mrs N. is the subject of a short narrative in a former part of this paper, and whose attacks of iritis and subsequent blindness we have ascribed to the gouty diathesis of her family, and who has since experienced in no unequivocal manner other manifestations of the arthritic poison, frequently, however set aside by a vicarious discharge in the form of hemorrhage from the bowels. The pallid and cachetic appearance of this lady has long been remarked to be influenced by no remedy so much as by that which has proved so useful to her mother—the cod-liver oil in conjunction with iron. In this family history it is worthy of remark that the females, contrary to what is generally noted, inherit more strongly than the males the predisposition to gout, both in its regular and irregular forms.

CASE XVIII.—A wealthy citizen, long a member of the civic corporation, and a frequent guest at the banquets which thirty years ago so often graced their board, was the victim of gout—if not by inheritance, at least by conquest, and after exhibiting for many years its acquisition in various forms of gastric, hepatic, and cardiac disorder, followed by indications of interrupted portal circulation, of dilatation of the chambers of the heart, and of serous effusion into the sac of the pleura, died soon after his great climacteric of these not unwonted complications of visceral disease, the common termination of the gouty. The mother of his children survived him for several years, but died ere she reached the age of seventy, with long-standing but obscure indications of gout, with signs of mitral-valve disease, with enlarged liver, obstructed gall-bladder, incurable jaundice, and general dropsy. What legacy have they left to their offspring? We know not the amount bequeathed to each, but the eldest son, long ere he reached the prime of life, gave unequivocal tokens of the possession of a large share of family honours and misfortune. He had frequent fits of pure, undisguised gout—the true podagra, and by and by of gout affecting various functions. and implicating many organs, and terminating not life, but hope, in cerebral disease and mental alienation. Another son partakes richly in the same inheritance; he is early the votary of sensual indulgence, and cannot escape the fate on which he rushes. He has fit upon fit; but still he pursues his course, till having reached his fiftieth year, he is struck down by an apoplectic seizure, and recovers only to drag out the remains of life, a broken and palsied cripple. A sister at the age of thirty has seen many physicians. She is considered dyspeptic by one, and hysterical by another; and after a long routine of unavailing practice, she is forced to forsake legitimate treatment, and to seek in hydropathy and other resources the relief she had sought in vain from the regular physician. She is only the victim, however, of disappointed expectation, and delusive hopes, and returns home to test once more the efficacy or the worthlessness of physic. She had become pale; she complained of oppression of the chest, and of frequent faintness; she had much dyspnœa, and great flatulent distension of the stomach, to allay which she had sought the too frequent use of stimulants,—of lavender, and sal-volatile and brandy. Her pulse was small and weak; the impulse of the heart was feeble, but there was no abnormal sound. The liver was tumid; the alvine excretions were pale; the urine was dark and loaded; and the skin assumed a jaundiced hue. She lost all relish for food; she had a craving desire for wine and brandy. She began to wander in her talk; she moaned from headache; she manifested great confusion of thought; she became highly delirious; she sunk into coma, and died; her death apparently caused by the non-elimination of bile or urea. Many months before these last fatal symptoms revealed themselves, she had presented the signs of gout in the tender and swollen finger-joints, and had more than once obtained a partial mitigation by a copious menorrhagia, and a more permanent relief after the means employed to overcome an attack of peritonitis in connection with a fibrous tumour of the uterus, under which she had long laboured. Dissection disclosed a small and fatty heart; a greatly enlarged and greasy liver, and kidneys whose muscular fibre was fast degenerating into the same morbid condition. The uterus was small and shrunk; and in its muscular substance was found imbedded the remains of a fibrous tumour of the size of a nutmeg.

Lastly, in connection with this family history we may record in passing, that a cousin by the mother's side, a gentleman of fifty years of age, had long laboured under gout in its severest form, and manifested in his distorted fingers and crippled toes the miseries of his sad inheritance. His son, too, while a youth of twenty, was the victim of an intractable dyspepsia which was intimately associated with the gouty habit, and which submitted to no alleviation save that which was obtained from remedies suitable to that disease. The father had tested perseveringly the efficacy of all gout remedies during the fit; but like many others refused to submit to any restriction on the pleasures of the table during the interval; and had thus joined the outcry against colchicum—of which he had acquired a great tolerance—and all rational treatment of the disease.

CASE XIX.—A gentleman, aged sixty-three, long resident in India, returned to his native country several years ago, having suffered little from disease during his absence. He had hereditary claims to gout, and though never affected by its ordinary local manifestation, had perceived indications of its presence in various functional derangements, and in vernal and autumnal attacks of congestion of the liver, accompanied by copious secretion of ropy mucus from the lining membrane of the pharynx and œsophagus, accompanied by great gastric tenderness, depressing sickness and vomiting, and a torpid, inactive state of bowels. These illnesses were generally of fourteen days' duration, and were accompanied by severe neuralgic pains over the limbs, by burning pains in the feet and hands, and by a copious efflorescence of erythema. The hands were stiff and painful, and some of the joints presented the little knobs we have so often referred to. Since his return to this country it has been remarked that two sisters, one of whom resides at a distance, and the other along with himself, are almost invariably affected at same time with himself, with illness, though of different character to that under which he labours. The sister in the country suffers from a peculiar form of bronchitis, while the other residing in his own house is affected with severe headache, or by dyspeptic derangement, accompanied by distressing flatulence, by tenderness and tumefaction over the liver, with the usual signs of disturbed renal secretion. To these have been added, on the last occasion, the more unequivocal demonstrations of gout in the foot. The coincidence of these illnesses, generally twice a-year, and their progress, *pari passu*, have often been matter of surprise, not only to the patients themselves, but to their medical attendants.

CASE XX.—Mr C. has not reached sixty years. He has been long a martyr to neuralgia, chiefly affecting the nerves of the lower extremities; and has patiently submitted to heroic and expectant treatment, but with very partial or doubtful benefit. He is now affected with the paraplegic condition described by Dr Graves, and to add to his miseries he has suffered from the distressing symptoms of angina pectoris, and from those depressing hypochondriacal feelings which afflict the gouty. His fingers display the little knobs of Heberden, and his urine deposits largely the octohedral crystals of the oxalate of lime. There are no signs of organic disease of the heart. He has benefited greatly, of late, by the persistent use of nitro-muriatic acid, and by small doses of the extract of nux-vomica. His cardiac and nervous symptoms have disappeared; his palsied and crippled limbs are now testing the efficacy of cod-liver oil, in conjunction with the extract of colchicum and the citrate of iron. Mr C. does not admit that he ever had gout, or that he possesses any right of inheritance.

CASE XXI.—A lady aged fifty-two, inheriting a predisposition to gout derived from her mother, whose family, as well as herself, had long manifested its possession, both in external form and by internal disorder, had suffered much from severe neuralgia of the limbs, and in various parts of the trunk, and been subject to otitis in a severe form, became gradually dull and desponding, and required the treatment and discipline of a confirmed melancholic. The urine was loaded with the oxalate of lime; and biliary and renal disorder prevailed. No treatment seemed to alleviate her mental condition. She has lately, however, been suffering from pain and swelling of the last joints of the fingers of both hands, which have resulted in the formation of little tender nodosities. The mind is now less dejected; and there has arisen a hope that depurants suited to the gouty diathesis may not be altogether unavailing in promoting the amendment which has lately taken place.

The connection of gout with hypochondriasis and melancholia, and their relation to the oxalic diathesis, is a subject worthy of patient investigation.

CASE XXII.—A gentleman aged fifty-six, descended from gouty ancestors, has during the last twenty years suffered from gout in various forms and modifications. After many years of dyspeptic suffering and nervous depression, he was seized with a fit in the extremities; and for a time the system appeared relieved. After a while the stomach again laboured, the liver became tumid, the feet and hands became swollen and tender, the bowels became irritable, and hemorrhoidal swelling and discharge commenced. A partial and temporary relief was again obtained, and by and by followed by symptoms of venous congestion and varicosity of the veins, and by painful sensations referred to the region of the heart. The pulse was found weak and compressible, the præcordial dulness extended, and the impulse of the heart feeble. Sensations of anguish of considerable duration from time to time occurred, and the mind became gloomy and apprehensive. While in this state, without any exposure to cold, an attack of tonsillitis was ushered in by a smart shivering fit, and followed by bronchial irritation and cough, and by a scanty expectoration of viscid phlegm. The cardiac symptoms were relieved; and on the subsidence of the bronchial irritation, the middle joints of the fingers of the right hand began to be swollen and painful. There have been in this gentleman's case long intervals of good health, alternating with fits of marked disorder of the liver, and followed at one time by inflammatory affections of the synovial tissues, or by smart febrile attacks, accompanied by irritation of the mucous membrane of the throat, bronchial tubes, or bowels, and generally terminating by profuse perspiration or remarkable diuresis. Temperance and active exercise control and check these seizures, while any approach to excess, or any indulgence in sedentary pursuits, never fails to foster and excite them. We regard them as indications of the gouty diathesis. The fingers exhibit the little knobs of Heberden, and the urine is generally loaded with the oxalate of lime.

Case XXIII.—Captain G., long resident in India, returned to Europe in unbroken health, at the age of sixty. He did not acknowledge gout, but his fingers gave evidence of its existence in little knobs and chalky concretions. Soon after reaching his seventieth year he sustained a paralytic seizure, and in five years afterwards an apoplectic stroke which proved fatal. His wife had died, at the age of thirty-seven, of a pulmonary disease; and several of his children died in early youth. Two attained adult age—a son and daughter. The former is strong and healthy, and shews no indication either of the gout or strumous diathesis. The latter, now at the age of forty-seven, has been delicate during the greater part of her catamenial period, and has suffered from neuralgia and other nervous disorders. She has also had periodical attacks of hemoptysis alternating or co-existing with functional derangement of the heart. The earlier attacks of hemoptysis naturally roused the suspicion that there might exist tubercular disease of the lung, inherited from her mother. The blood, however, was not that which we are accustomed to see from such a source. It was pale and thin, and bore the character of an exudation from a mucous surface; while no stethoscopic examination could detect the presence of diseased lung. The functional derangement of the heart, and the spitting of blood, have both yielded to an attack of gout in the fingers and toes, the tenderness and swelling of which have been followed by the deposition of the morbid material, and the formation of "those little knobs about the size of a small pea, which are frequently found upon the fingers, particularly a little below the top, near the point." She has since been subject to tonsillitis.

It would be easy to extend these illustrations, and adduce additional examples of the gouty diathesis in individuals or in family groups, so as to elucidate its natural history. I shall only record, however, one other case of a character which is very common, but in regard to which physicians are by no means agreed.

Case XXIV.—On the 5th of September last Mr C. called to consult me regarding his health, which for upwards of a twelvemonth had been to himself and friends the source of much anxiety. I had been previously acquainted with him, though I had never prescribed for him in any illness; and was much struck with the expression of his countenance, and general appearance of disorder which he manifested. Mr C. is sixty years of age, tall and well built, and particularly large-chested. He had spent the early part of his life in the army, and had always lived generously, though accustomed latterly to lead an active country life. Some years ago he consulted an eminent physician here regarding symptoms which were attributed to the heart; but was informed that that organ was quite sound. Subsequently he had seen other medical men in different parts of the country, and lately had returned from a lengthened sojourn at the German spas. The stomach, the liver, the heart, were each in its turn considered the seat of disease, and the source of his sufferings. These sufferings at present consisted in severe and lasting fits of dyspnœa, often to the extent of orthopnœa, with great sense of debility and faintness. The pulse was weak, rapid, and irregular. The impulse of the heart was feeble; and a soft bellows murmur accompanied the first sound, heard over the region of the mitral valve, and not transmitted into the large arteries. There was extended præcordial dulness. The chest anteriorly gave no abnormal sound; but on the posterior and lower part, particularly of the left side, there were indications of œdema. The liver was enlarged and tumid, and could be readily felt a considerable way below the margin of the ribs. The urine was reported scanty, the bowels irregular, though generally relaxed; and the feet and ankles were found slightly œdematous. This group of symptoms forced on my mind the impression of dilatation of the heart, and mitral-valve disease, in connection with impeded circulation through the lungs, and consequent congestion of the liver and kidney. The question remained, Has Mr C. had gout? It was answered in the affirmative. The last attack occurred a little more than a year ago, and much of the disordered health which the patient has experienced has been consequent on that illness. A somewhat more favourable prognosis is now required; and while the heart is admitted to be the seat both of functional and organic disease, the prospect is held out that life may be prolonged, and all the present sufferings allayed, by careful regimen and judicious treatment. Small doses of mercury to act upon the liver; small doses of colchicum and squill to act upon the kidney; small doses of iron to act upon the weakened heart and circulation; and a simple generous diet, with a moderate addition of wine, were prescribed. All active exercise was forbidden; but recreation in the open air, in a carriage, or on horseback, was allowed.

In the course of ten days I was requested to meet Mr Otto, Mr C.'s ordinary attendant, in the country, and found our patient much worse. The dyspnœa was urgent. The right lung had become congested, hemoptysis to a small extent had taken place; the liver was more enlarged, and the feet and legs greatly swollen. The diuretics had not begun to act. A slight attack of gout had seized the great toe. No change of treatment was allowed, beyond the application of a few leeches over the region of the congested part of the lung. In the course of another week the mercury and diuretics acted favourably, the breathing was relieved, the heart beat more forcibly and regularly, the tumefaction of the liver had subsided, and the swelling of the feet had greatly diminished. In ten days more Mr C. could lie down in bed; the action of the heart was tranquil; the mitral murmur soft and difficult to detect; and all the symptoms had declined. A bitter infusion, with a saline diuretic, were recommended. The amendment was progressive for several weeks, but about the middle of October, without any assignable cause, there occurred a renewal of the chief part of the former history,—the dyspnœa, at first not urgent, but gradually amounting to orthopnœa, the feeble, rapid, irregular pulse, the tumid liver, the scanty urine, and the dropsical swelling of the limbs. Dr Alison, at the patient's desire, was conjoined in consultation. The diuretics, to which digitalis was added, were increased, and mercury in the form of the proto-ioduret was given as an alterative. A moderate allowance of wine and animal food was still granted. Two or three weeks elapsed before these remedies had the desired effect, but about that time the symptoms again began to decline simultaneously with the increase of the renal secretion; and on the 24th of November, when I last saw Mr C., he might be pronounced a fair convalescent. The action of the heart was normal in number and rhythm, the impulse feeble, the mitral murmur scarcely perceptible, the breathing free, the decubitus easy, the swelling of the liver had entirely disappeared, and there was not the least trace of œdema. About the beginning of January last there was a recurrence of the same class of symptoms, but they have again yielded to the same plan of treatment, including iron, and now (February 1), Mr Otto informs me that his patient is again a hopeful convalescent. The physician, however, who has seen much of such cases, will not readily pronounce a too favourable prognosis. He knows that the whole series of phenomena is apt to recur again and again; and though certainly under the influence of medicine, so long as the kidney does not refuse its office, yet the functional will gradually assume more and more the character of organic disease, and the heart, and lungs, and liver become involved in irreparable injury. The question will arise, which is the first link in this chain of morbid processes? One will say the lungs, another the liver, and a third the heart. We reply with the last. The heart is first affected, but it is affected only functionally through gout. The muscular power of the left ventricle is weakened, and by and by, its cavity dilated, and its walls thinned, interrupted pulmonary and portal circulation succeed, and dropsy follows in consequence; all produced by and aggravating the original lesion, which however is still capable of repair, so long as the kidney, which is seldom organically affected, is able to carry off the effused fluid, and relieve the circulation of the complicated causes of its interruption.

We are quite prepared to find objections taken to these cases, and indeed to many others we have adduced, as illustrations of gout, and to have the following sentiments of a late reviewer in the *Monthly Journal* quoted in support of such a position:—"In Dr Forbes' Bibliography we reckon more than 200 works on gout, not one of which has issued from a Scottish press, while London teems with them. This produces a great peculiarity in the views and practice of our southern brethren. They trace gout in almost every complaint to which the human body is liable, not only in its congeners of biliary and urinary complaints, but in a great number of others,—diseases of the heart, liver, hydrothorax, dropsies, &c.,—while we, in our northern sphere, meeting with the same diseases unconnected with gout, which is extremely rare, consider them as separate entities, however allied."[1] In our turn we demur to the whole drift of this passage. We cannot persuade ourselves that gout is less frequent among the luxurious and well-fed of the north than among their fellows of the south, nor are we inclined to believe that hereditary taint or the exciting causes of the disease are less in operation in the one quarter than the other. Neither are we prepared to admit that there is any ground whatever for a great peculiarity of view and practice in the English mind. He who has attentively set himself to study the history of gout, and is possessed of opportunities and means of doing so, and can profit by observation and experience, will certainly find that in the north as well as in the south it disturbs many functions and originates many diseases, and that "the strumous is not more frequent than the gouty habit." Dr Gairdner, who ventured this opinion in the first edition of his excellent treatise on Gout, informs us in a second that he has been much struck by the unanimity of assent which it has received. "But I have been still more pleased" (he says) "to find that friends around me, whom the high opinion of the public has placed in the most conspicuous stations in practice, and whose judgment may therefore be relied upon, have also pointed to it as eminently truthful, and coinciding exactly with their own experience. Sir Benjamin Brodie has taken the trouble, indeed, of writing me—'I entirely agree with what you say as to the very frequent occurrence of the gouty diathesis. A large proportion of the persons that come to me with what are esteemed to be local diseases, are, in reality, suffering from the influence of the gouty poison in the system, though they may have nothing which would commonly pass for gout.' "[2] And to shew that the writer, whose opinion is entitled to the greatest weight, is not one of those who "trace gout in almost every complaint to which the human body is liable," let us turn to one of his most esteemed works—that on Diseases of the Joints. We quote from page 57 of the fourth edition. "The following case," says Sir Benjamin, "furnishes an example of a disease which, as far as I know, has not been described by any pathological or surgical writer. A gentleman, forty-five years of age, in the middle of June 1817, became affected with symptoms resembling those of gonorrhœa. There was a purulent discharge from the urethra, with ardor urinæ and chordee. On the 23d of June he first experienced some degree of pain in his feet. On the 24th the pain in the feet was rather increased, but not in a sufficient degree to prevent his walking four miles. There was some appearance of inflammation of his eyes. June 25th, the pain in his feet was more severe; the tunicæ conjunctivæ of his eyes were much inflamed, with a profuse discharge of pus. These symptoms increased in violence, the pulse varying from 80 to 90 in a minute, the tongue being furred, and the patient being restless and uncomfortable during the night. The whole of each foot became swollen; there was inflammation of the synovial membranes of the ankles; and it appeared to me that the affection of the feet themselves arose from inflammation of the snyovial membranes belonging to the joints of the tarsus, metatarsus, and toes. He said he could compare the pain which he experienced to nothing else than that which might be supposed to arise from the feet being squeezed in a vice. On the 27th of June the left knee became painful, and on the following day the synovial membrane of this joint was found exceedingly distended with synovia. He was now completely crippled; compelled to keep his bed, and scarcely able to vary his position in the smallest degree without assistance. The inflammation of the eyes and urethra was somewhat abated. June 30, the inflammation of the eyes and urethra had much subsided, and the purulent discharge was diminished. The pains of his joints were less severe; and the feet were less swollen. On the following day the knee was less swollen also. He continued to mend, and on the 10th of July the swelling of the feet was still further diminished, and that of the knee had almost wholly disappeared. His pulse continued to vary from 80 to 90 in a minute, and his tongue was still furred. He had pain in the feet and knee, but less severe than formerly, and he was restless at night. July 13, he complained of pain in the right knee, and on the following day there was pain also of the right elbow and shoulder. The right knee afterwards became swollen from fluid within the cavity of the synovial membrane, but not in the same degree with the other knee, and the swelling soon subsided. There was never any perceptible swelling of the shoulder and elbow. August 1, all his pains were abated. The eye and urethra were nearly free from inflammation, and the purulent discharge was scarcely perceptible. August 5 he was free from pain, except on motion; the joints which had been affected were stiff; but he was able to move about on crutches. From this time he progressively mended. The stiffness of the joints diminished very slowly; but he was free from all uneasiness. He was longer in recovering the use of the shoulder than that of the other joints.

---

[1] No. for January 1850, page 42.

[2] Gairdner on Gout, 2d Ed., p. 4.

In the following December he had another attack of the complaint. The symptoms were the same as formerly, taking place in the same order, and pursuing the same course, but with a much less degree of violence. This second attack lasted about six weeks; and left him again considerably crippled.

In March 1818 he became affected with an ophthalmia, but of a different nature from that which he laboured under in the preceding summer. The inflammation was seated in the proper tissues of the eye; and it appeared probable that it would speedily have terminated in adhesions of the iris, and destruction of the powers of vision, if its progress had not been arrested by repeated bloodlettings, and the use of mercury. He had another attack of ophthalmia of the same kind four years afterwards."

The treatment consisted of leeches and blisters to the knee; liniments rubbed on the knee and shoulders; and fomentations where there was severe pain. "Of the various medicines which were exhibited (adds Sir Benjamin), none seemed to be productive of benefit, with the exception of the *vinum colchici*. It was under the use of this medicine that not only the pains and swellings of the joints, but that even the purulent inflammation of the eyes and urethra first began to subside; and I am, on the whole, inclined to believe that my patient was indebted to it for a much more speedy recovery than he would have obtained otherwise."

Whoever peruses this history, and that of several subsequent cases in the work referred to, will, we believe, be convinced that there is no desire in the South to ascribe to gout an undue or exaggerated influence. The only doubt that remains in our mind as to the real nature of the disease arises from the circumstance of its not having at once been perceived by the enlightened and accomplished author who has detailed these histories.

The illustrations which we have rather hurriedly thrown together in this and a former paper, and the study and observation of gout in its more ordinary manifestations, lead, we think, to the following conclusions:—

1st, That gout is a constitutional disorder, originating in mal-assimilation, and in the secretion and non-elimination of a morbid product, which by accumulation in the blood, and transmission through the ordinary channels of the circulation, disturbs many functions, and engenders many diseases.

2d, That it is a hereditary disease, chiefly through the contaminated blood transmitted from parent to offspring; but that it may also descend from ancestor to heir, through a peculiarity of the textures prone to be affected by the morbid material.

3d, That it may be acquired by habits of indulgence, calculated to injure and deprave the digestive and assimilative processes, and produce the peculiar contamination of the circulating fluids in which the essence of the disease consists.

4th, That the condition of the system, or diathesis, thus transmitted by hereditary descent, or acquired by mal-assimilation, or by both, is intimately connected with the relations of urea and uric acid, and their compounds in the system, as manifested by the presence in the blood, by the deposition in the parts affected, and by the elimination through the several emunctories, of these materials, during the prevalence of the disorder, or on the occurrence of a fit of gout.[1]

5th, That there are various stages of this diathesis, of longer or shorter duration, of sthenic or asthenic character, according to age, sex, temperament, habit, and mode of life, and other circumstances.

6th, That the primary effects of the gouty diathesis are those of a poison, often slow and insidious, disturbing many functions, chiefly those of the digestive organs, and of the nervous centres; but sometimes more speedy, through its rapid accumulation in the system, and engendering active disease in various tissues and organs of the body, the synovial, the mucous, and serous membranes being peculiarly prone to be affected by it; while the heart, the liver, and kidney, participate largely in the general disorder.

7th, That the development of gout in the hand or foot, or as a febrile disorder, or an inflammatory affection of any tissue or organ for which it has an affinity, appears to free the system for a time, from the symptoms and sufferings of the diathesis, through the elimination of the morbid material by the usual emunctories during the progress, or at the crisis, of the disease.

---

[1] Dr Garrod has demonstrated the presence of urate of soda in the blood of gouty patients: the chalk stones of the gouty are known to be composed of the same material; and Dr Prout has seen it so copious in the urine, as to impede its passage through the urethra.

8th, That the altered or contaminated condition of the blood in the gouty habit has subsequently an injurious effect, either vitally or mechanically, on the muscular structure of the heart and bloodvessels, by weakening their power, and producing dilatation and attenuation of their cavities and trunks, and leading to venous congestion and obstruction, and to their consequences in hemorrhagies, dropsical effusions, and similar affections.[1]

9th, That ultimately the heart and bloodvessels, through the continued prevalence of the diathesis, undergo structural changes by the deposition of earthy matter in their coats; and thus gout is intimately allied with palsy, apoplexy, and other cerebral diseases, and with angina, syncope, and rupture and other fatal cardiac affections.

These views of the nature and progress of the gouty diathesis will be found available in directing our mode of treatment in the different aspects in which we are called to combat the disease.

It has been wisely remarked that, in the earlier stages, gout is curable, if not through physic, yet through the physician; and the remark might have been extended so as to include the patient, whose unwillingness to submit to hygienic regulations is too often the great bar to successful treatment. Some who have been happily apprised at an early stage of the nature of their inheritance, or of the pernicious tendency of a life of indulgence, have had the fortitude and perseverance to overcome and resist both the habit and the temptation, and by a life of active exertion, and temperate enjoyment, have reached the confines of extreme old age, without having witnessed more than the precursory symptoms of a disease which, under other circumstances, would have proved inveterate. But the examples of a virtue thus exercised, and a reward thus reaped, are but the exceptions to the rule of vicious indulgence, and painful retribution, which everywhere prevails.

Temperance—strict temperance, in the gratification of every appetite, is demanded of every subject of the gouty diathesis. Exercise systematically pursued in the open air; attention to the healthy state of the skin, and to the different excretions, in short, the strict observance of all the well-known laws for the preservation of health, may, without the assistance of medicine, ward off a coming fit of gout, or check the progress of the gouty diathesis. But the physician is seldom called to offer advice in such circumstances, and it is only when the gouty poison has accumulated in the blood, and disturbed more or less some of the great functions of life, or lighted on some texture, and raised up inflammatory action and febrile disturbance, that his aid is for the first time sought and obtained. Is it a fit of the gout, as described by Sydenham? Let the sufferer rest in bed; let the foot be elevated, and enveloped in a warm-water dressing covered with oiled silk, and let the strict antiphlogistic regimen be carefully enjoined. The functions of the liver and bowels will be found so disordered, that a full dose of a laxative, such as rhubarb, with the bicarbonate of potash, is required; and the action of the kidney will be found so impeded, and the secretion so disordered, that saline diuretics, those especially of the salts of potash, are demanded in full and continued doses. Colchicum, in conjunction with these means, will be found highly useful; without them its operation is uncertain, and its efficacy doubtful. Administered alone, and in large doses, as is too often the case, the remedy may bring a fit of gout rapidly to a close, either by a powerful cathartic or diuretic effect; but the system is not relieved, though the local affection may appear to be subdued. It is by securing the eliminating property of the drug, along with the depurating effect of the saline diuretic, that we obtain the full benefit of colchicum in gout; and this is best accomplished by small doses of the remedy. In acute articular gout, or in any of the inflammatory affections which are allied to it, fifteen or twenty drops of the wine prepared from the seeds, in combination with scruple doses of the acetate or nitrate of potash, or still larger of the bicarbonate, administered three times a day, and continued for a week or longer, will be found an eligible mode of prescription; and when the more urgent symptoms are overcome, the remedy may be continued still longer in the form of the acetous extract in combination with a laxative, a bitter, or a chalybeate once or twice a day, care being taken not to press the drug to the production of its sensible physiological effects. We believe that it is by the known power which colchicum possesses of favouring the elimination of urea from the system, that it acts so beneficially in gout, and that this effect is best produced when it acts silently and slowly, without depressing the nervous system, or exciting, in a marked manner, the secreting organs. In cases of gouty inflammation affecting an organ essential to life, or of a structure so delicate that the slightest lesion proves injurious to its functions, for example, the pericardium, or the iris, it may be necessary to employ bloodletting in some form, and to seek the aid of mercury to prevent the deposition of lymph; but even in those cases colchicum should be steadily employed in the manner we have pointed out, and its beneficial effects will be best secured after these other remedies have told on the system. No time, however, should be lost in seeking its aid, nor should it hastily be abandoned, from disappointment of its promised virtues. The exhibition of colchicum, with such depurants as we have referred to, will be found availing as a means of cure in all the various forms of disease connected with the gouty habit, not only in such inflammatory affections as ophthalmia, otitis, tonsillitis, pleuritis, bronchitis, and the like, but in disorders of the nervous system, as headache, toothache, lumbago, sciatica, &c.; nor will its efficacy be less conspicuous in those cases of rapid effusion into the synovial capsules, and in the gonorrhœa and leucorrhœa dependent on a similar cause. Its power over the affections of the skin connected with the gouty habit is very striking; urticaria, erythema, eczema, and the like, are remarkably subject to its control; indeed, the remark applies more or less to all the disorders which prevail during the earlier stages of the diathesis, and before the heart and bloodvessels, and the liver, kidney, and other viscera have sustained abiding lesion.

---

[1] The effect produced by the altered condition of the blood of the gouty may be somewhat analogous to that produced by the impoverished blood of the anemic. In both we find dilatation of the chambers of the heart, and enlargement of the great vessels. The resisting power of the muscular fibre is diminished, and thus the dilatation of the cavities results as a purely mechanical effect of over-distension. The gouty have naturally thin-walled hearts, and thus are more prone than others to dilatation. See Hope on Diseases of the Heart, page 293, 3d edition.

Nor even then is colchicum, prudently and judiciously employed, altogether valueless. The combination of colchicum with some of the preparations of iron will be found useful when the heart is found feeble and dilated. The combination of colchicum with mercury will be useful when the liver is enlarged or torpid; and the same preparation, with the addition of digitalis or squill, will be found to act beneficially when dropsy in any of its forms results from organic lesion of these or other viscera; and throughout the course of the diathesis, whatever remedy may appear requisite to fulfil some indication of treatment will, in general, receive aid from "the great specific."

In the chronic and asthenic forms of gout, while simple nutriment and a moderate use of wine are requisite, no remedy has appeared to us so useful as the combination of colchicum with quinine, bebeerine, or iron; but the dose of the colchicum ought to be small, generally in the form of extract, and administered at long intervals, and continued over a long period.

In the recent works of Drs Todd and Gairdner, and in the Notes and Reflections of Sir Henry Holland, will be found many suggestive and practical remarks on this part of the subject, and to them we must refer, having already exhausted our space; we cannot close, however, without referring to the singular benefit which may be derived from cod-liver oil, in some of the morbid conditions of the gouty. In the chronic and asthenic forms, generally, and in the more acute, after the severity of the illness is overcome; in almost all the chronic ailments connected with the gouty habit, and in the cachetic condition of the offspring of gouty parents, we have found it to be possessed of wonderful renovating powers. Nor have we been less agreeably surprised to find how potent, after persistent use, it is, in the least promising of the gouty sequelæ which can be reached by medicine: we mean the nodosities of the fingers and toes, and the crippling, lameness, and deformity occasioned thereby. The consumptive and the rheumatic have profited largely by the remedy which Dr Bennett, after it had long been laid aside, has had the merit to restore to the list of our therapeutic agents; and its reintroduction into this country, we are not without hope, may yet remove, in some measure, both the truth and the reproach of the classic sentiment:—

> "Tollere nodosam nescit medicina podagram."—OVID.

ART. II.—*On Diverticula from the Small Intestine, anatomically and pathologically considered.* By JOHN STRUTHERS, F.R.C.S., Lecturer on Anatomy, Edinburgh.

Having met with a considerable number of instances of the diverticulum from the small intestine, and with several in which it was concerned in producing death, I have thought it might interest others were I to record the facts with which my investigation of this subject has made me acquainted.

It is well known to anatomists that, as a rare variety, a diverticulum or cæcal appendage may be found projecting from the small intestine; and various opinions have been expressed regarding the origin or nature of this process.

My object in this paper is chiefly to state anatomical and pathological facts in connection with the cases I have met with, but it is well that I should first briefly glance at the different views, so as to elicit the bearing of the details to be furnished.

Monro[1] has observed, "There are two kinds of diverticula, original malformations, and those formed consequent upon the protrusion of the intestines. The preternatural appendices occasionally found within the sac of a hernia are generally formed as stated by my grandfather, viz. by the gradual elongation of the side of the intestine, and probably also by the addition of new matter." His view seems to be that, by an adhesion forming, a portion of bowel is tied down and pulled upon, and thus becomes "gradually elongated into a cul-de-sac of some length.' And, although this remark is applied only to their production in a hernial sac, the view may be extended to the intestine generally. Monro farther remarks—"There is also a species of appendix different from any of the preceding; it has a much thinner covering, is of a globular form, and is shorter and wider. It seems to be formed by a protrusion of the mucous membrane, through the other coats of the intestines."

This view is strengthened by the comparison to the sacculi, or herniæ, which are known occasionally to occur from other parts, as the gullet, urinary bladder, and gall-bladder, and by the consideration that more than one may be found on the intestine of the same subject.

The other view is, admitting the occasional occurrence of acquired protrusions on the small intestine, as on other hollow viscera, that the diverticulum is a congenital or original formation, to which is farther to be added Meckel's explanation, that it results from the early connection between the umbilical vesicle and the intestine of the embryo, remaining partially unclosed and growing as the intestine grows.

Some, admitting the original or congenital nature of the true diverticulum, objected to Meckel's view on the ground that he could not demonstrate an actual communication from the umbilical vesicle into the intestine in the human embryo; and there was the hypothesis of Oken, admitting the communication, but that it took place not with the ileum but the cæcum, and that the appendix vermiformis was the remains of this early communication.

The latter view, however, was a hypothesis, not an inference, and is set aside by the demonstrable fact that the cæcum is seen forming or formed at some distance from the point where the vitelline duct, or pedicle, is seen to be connected with the small intestine.

---

[1] The Morbid Anatomy of the Gullet, Stomach, and Intestines. By Alex. Monro. Edinburgh, 1830, p. 93.

For an able discussion of the whole question, with a statement of the facts and arguments on either side, I may refer to Meckel's excellent work on anatomy,[1] and shall here only state briefly the chief arguments advanced by Meckel as the foundation of his view, although he laboured at that time under the disadvantage of being unable actually to demonstrate a continuity of canal between the intestine and the umbilical vesicle in the human embryo. His chief arguments are—That their characters indicate them to be primitive formations, having the same structure as, and continuity with, the intestinal coats; that they occur almost always at the same part of the intestine, and that the connection between the umbilical vesicle and the bowel occurs at this same part —viz. on the ileum near the cæcum; that "we sometimes perceive in very young fœtuses a canal which goes across the umbilical sheath, from the vesicle to the abdomen, and by which we can at pleasure empty the vesicle of this fluid, and fill it again;" that "it is not unfrequent, proportionally speaking, to find in the full-grown fœtus a canal which extends from the intestine to the umbilicus, which opens in this latter place, and is always attended by the omphalo-mesenteric vessels." And lastly, he alludes to "the analogy with birds, reptiles, and cartilaginous fishes, in which it is proved that the communication in dispute exists at all periods of fœtal existence." He admits of course that protrusions or herniæ may occur here as elsewhere, and thus account for the occasional existence of a diverticulum or pouch on the jejunum, and for the existence occasionally of more than one.

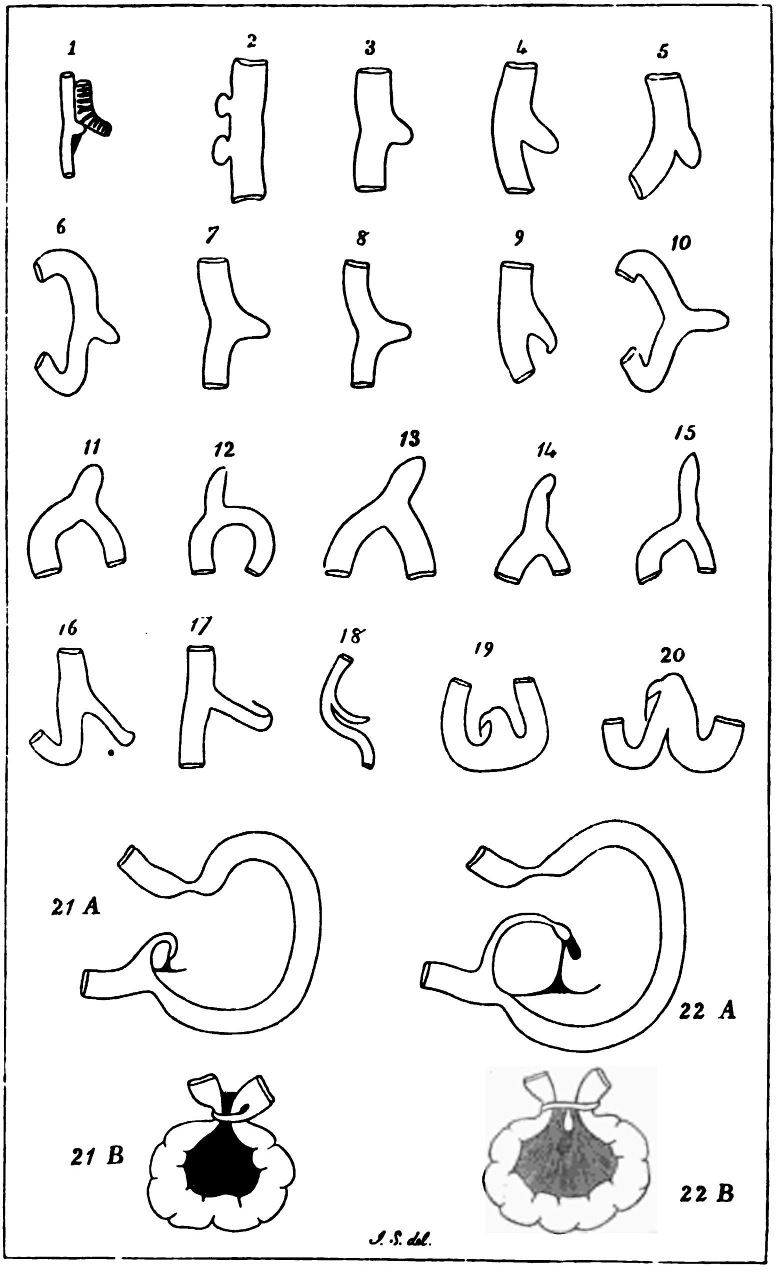
It is necessary therefore to distinguish diverticula into two kinds, the true and false. On this M. Cruveilhier[2] remarks, "These diverticula, into the composition of which all the tunics of the intestine enter, are very different from the hernia of the mucous membrane through the muscular membrane, a hernia of which I have seen an example in the duodenum, and several in the rest of the small intestine. In a subject recently submitted to my observation, the small intestine presented fifty spheroidal tumours, of unequal volume, all situated along the mesenteric border of the intestine, and formed by the hernia of the mucous membrane, through the muscular fibres."

Having now drawn attention to the points of interest in connection with these diverticula I shall next give an account

---

[1] Manual of Descriptive and Pathological Anatomy. By J. F. Meckel, Professor of Anatomy at Halle. London edition, 1838, pp. 375–386.

[2] Traité d'Anatomie Descriptive. Paris, 1852, p. 351.


1
2
3
4
5
6
7
8
9
10
11
12
13
14
15
16
17
18
19
20
21 A
22 A
21 B
22 B
J. S. del.

of the various cases I have met with and examined, considering them first anatomically and then pathologically. These are twenty in number, of the true diverticulum ilei, and two of the false diverticulum, one on the jejunum, the other on the œsophagus, which will serve to contrast with the former or true diverticula.

In the woodcut is given an outline of each. These sketches I made with care from the preparations, shewing in each a sufficient portion of intestine, and representing accurately the proportion and the various points in the anatomy of each diverticulum. They are arranged in a gradual series; and as the numbers of the figures correspond to the same numbers in the descriptive notes, each may be referred to from the notes.

I have deemed it best, at the risk of being thought tedious, to give all the facts in connection with each case, and, for brevity's sake, in the form of notes, which were made with the preparations before me. Of the twenty-two preparations, eight (Nos. 1, 3, 6, 9, 16, 17, 19, 21) are in my own collection; for the others I am indebted to the Edinburgh Museums; eight (Nos. 2, 4, 7, 10, 12, 13, 20, 22) are in the University Museum; and the remaining six (Nos. 5, 8, 11, 14, 15, 18) in the Museum of the College of Surgeons.

Descriptive Notes of Cases of Diverticula, &c.

1. Pouch on œsophagus. Dried preparation, without history. Projects forwards from œsophagus at bifurcation of trachea, and occupies the recess between and behind the bronchi. It is a blunt knob-like projection, measuring in depth, length, and thickness, ⅝ inch. It begins gradually and without any constriction, occupying the entire breadth of the œsophagus, the measurement of which, and of the base of the pouch, is ⅞ inch. The œsophagus at this point is slightly enlarged laterally, and is if anything larger below the pouch than above it. Pouch terminates in a smooth rounded extremity. The muscular fibres which run longitudinally on the gullet are seen distinctly to be continued halfway along the pouch, splitting above and below to incase it. Circular fibres are visible on the gullet, but not on the pouch. The pouch is slightly more transparent than the gullet.

2. Dried preparation, marked " portion of jejunum, with two small diverticula." The smaller pouch projects directly outwards, like a nut. It measures one inch across and ¾ inch vertically. The commencement is abrupt, and marked by a sharp constriction on all sides. The larger pouch is situated ¾ inch from the other, measures 1½ inch from above down, and

1¼ across. It also begins by a sharp circular constriction, and on one side it is considerably bulged or prolonged. The portion of intestine is plainly marked with valvulæ conniventes; but as it is stuffed, it is impossible to say whether the walls of these pouches differ in thickness from those of the bowel.

3. Dried preparation; portion of small intestine, with diverticulum. Diverticulum one inch in length. Has funnel-shaped origin from whole breadth of bowel. Passes somewhat obliquely from bowel, which is considerably larger on the side of the obtuse angle. Forms a blunt cone, terminating in a smooth rounded extremity, without any appearance of ligament or mesentery. It proceeds from the bowel nearer to the mesentery on one side than the other. No valvulæ conniventes, but a small crescentic fold at commencement of diverticulum on side of acute angle. There is an evident continuity of tissue between the walls of the bowel and diverticulum.

4. Dried preparation, without history. Diverticulum two inches in length and 1¼ in diameter. Arises from whole breadth of bowel, abruptly, and marked by a slight constriction, except on one side, where they are gradually continuous. Direction, oblique from the bowel, which is considerably smaller on the side next the acute angle. The body is straight and very little tapering, and terminates in a smooth and rounded extremity, without any appearance of a ligament or mesentery. It is situated on the bowel much nearer the mesentery on one side than the other. The intestine presents no valvulæ conniventes, but there is a small crescentic fold at the acute angle. Both the longitudinal and circular muscular fibres are distinctly visible on the diverticulum, and continuous with those of the intestine.

5. Wet preparation; "small intestine having a cul-de-sac, from the body of a person in which no other morbid appearance was observed." Diverticulum 1¾ inch in length. Begins gradually from entire breadth of convexity of bowel. Directed obliquely, and bowel considerably smaller on the side next the acute angle. Terminates in a rounded, slightly conical extremity, without appearance of a ligament. It is somewhat bent on the side of the acute angle, and along this edge of it lies a narrow mesentery.

6. Dried preparation, without history. Diverticulum two inches long. Arises gradually from whole breadth of bowel. Is directed obliquely from bowel, which is larger on the side of the obtuse angle than on the other. It is conical, and terminates in a smooth rounded apex, without any appearance of a ligament. It proceeds from one side of the bowel close

to the mesentery, and, continued from the latter, rising up at right angles from it, is a small mesenteric fold, which reaches ⅓ of the length of the diverticulum. On the concavity of this curved piece of intestine, crescentic folds, like small valvulæ conniventes, are seen, and one more distinct at the acute angle where the diverticulum begins, but there are no folds on the convexity of the intestine.

7. Dried preparation. Diverticulum two inches in length. Begins by funnel-shaped origin on both sides, from whole breadth of the bowel, and from that part of it which is directly opposite from the apparent former attachment of the mesentery. Is directed with very little obliquity, and the intestine is considerably larger on the side of the more obtuse angle, being in diameter 1½ inch on one side, and one inch on the other In form it is conical, the depth considerably exceeding the thickness or breadth, and terminates by a smooth rounded end, without any appearance of ligament or mesentery. The intestine presents no valvulæ conniventes.

8. Dried preparation. Diverticulum two inches in length, and conical in form. Has a funnel-shaped origin from the whole breadth of the bowel, opposite from the traces of the former mesenteric attachment. Has a slightly oblique direction, and the bowel is largest on the side of the obtuse angle. Extremity smooth and rounded, and no appearance of ligament or mesentery. No valvulæ conniventes on the intestine.

9. Dried preparation. Diverticulum two inches in length, conical and curved. Has a gradual origin from about half the breadth of the bowel. It is directed obliquely, and the bowel is much larger on the side of the obtuse angle than on the other. Termination like point of fore-finger, with a little pea-shaped prolongation towards the concave side. It is connected with the bowel on one side, close to the mesentery, which sends a small mesenteric fold along the concavity of the diverticulum. No valvulæ conniventes, but there is a crescentic fold at the acute angle, a little below the entrance into the diverticulum.

10. Dried preparation. Diverticulum two inches in length. Origin funnel-shaped from whole breadth of bowel, and nearer the mesentery on one side than the other. Direction horizontal, and bowel apparently of equal size on the two sides. Form somewhat conical, the last half-inch tapering to a blunt apex, which is smooth and rounded without any appearance of a ligament. There is a small mesentery for ⅔ds of its length, prolonged from the mesentery of the bowel,

on that side which is nearest the mesentery. No valvulæ conniventes on intestine. The longitudinal and circular muscular fibres are distinct on the diverticulum, and seen to be continuous with those on the bowel.

11. Dried preparation. Diverticulum two inches in length and conical. Origin funnel-shaped, from entire breadth of bowel on side opposite from mesentery, but nearer to it on one side than the other. Direction somewhat oblique, and bowel considerably larger on the side of the obtuse angle. Termination smooth and rounded, without appearance of ligament or mesentery. Intestine without valvulæ conniventes.

12. Dried preparation. Diverticulum two inches in length. Origin gradual, or without constriction, from convexity of bowel, nearly equi-distant on both sides from mesentery. Direction nearly horizontal, but is as it were continued from one end of the bowel, and this is a little larger than the other. Termination smooth and rounded, without ligament or mesentery. Both sets of muscular fibres are plainly visible on the diverticulum, though less so than on the bowel, and they are scarcely distinguishable towards the termination of the diverticulum. Intestine has no valvulæ conniventes.

13. Dried preparation. Diverticulum 2½ inches in length. Origin, from whole breadth of bowel, funnel-shaped on one side, and somewhat constricted on the other ; and a little nearer the mesentery on one side than the other. It is more in the direction of the bowel on one side than the other, and the end from which it seems more continued is here the smaller of the two, being that next the obtuse angle. Termination smooth and rounded, without appearance of ligament or mesentery. Intestine has no valvulæ conniventes.

14. Dried preparation. Diverticulum 3½ inches in length. Origin gradual, from whole breadth of convexity of bowel, but nearer mesentery on one side. It is shaped somewhat like the fore-finger, but twice as large, and is slightly bent to one side; and that end of the bowel which opens more directly into it is considerably larger than the other end. Termination smooth and rounded; the bloodvessels are injected, and are seen to cross over the bowel on both sides, and run along both the convexity and concavity of the diverticulum. On the concavity, an inch from the blind extremity, is a depression and puckering, and convergence of bloodvessels, as if a cord had passed off here. Small valvulæ conniventes are seen in the interior of the bowel, and a more distinct crescentic fold at the angle in the centre of the concavity of the portion of bowel.

15. Dried preparation. Diverticulum four inches in length. Origin somewhat abruptly from nearly whole breadth of bowel, and nearer to mesentery on one side. Bowel much larger at one end than the other. Body of diverticulum wider than the commencement, increasing to within 1½ inch from the extremity, towards which it now tapers to a distinct apex, without appearance of ligament. Vessels are seen to run along that surface which lies nearest the mesentery of the intestine. Intestine destitute of valvulæ conniventes.

16. Dried preparation. Diverticulum three inches in length. Origin gradual, from about half the breadth of the intestine, and nearer the mesentery on one side. Is directed obliquely from the bowel, which is largest on the side next the obtuse angle, and somewhat bulged or uniformly dilated opposite the commencement of the diverticulum. Body of diverticulum is curved, with convexity on side nearest the mesentery, and has uniform diameter of ¾ inch, dilating in its last half-inch to a termination abruptly bulged on one side, giving it a transverse diameter of 1¼ inch. No appearance of mesentery or ligament. Intestine presents no appearance of valvulæ conniventes.

17. This specimen I examined in the recent state. Diverticulum three inches in length. Origin, gradual or somewhat funnel-shaped, from about ¾ of the breadth of the intestine, the diameter of the latter being 1¼ inch, and that of the diverticulm one inch. Direction oblique from the intestine, which is somewhat larger on the side of the obtuse angle. Body of diverticulum is round, and increases somewhat in diameter towards the end, where it is bulged or enlarged to one side. The termination is smooth and rounded, except to the side where it is bulged. Here there is a pointed extremity giving off a ligamentous cord, which was two inches in length, and blended with the mesentery.

I regret that this was not more exactly noted at the time, but from my notes made then, and the appearance of the dried preparation now, I believe I am correct in stating that there was a narrow mesentery posteriorly in which this terminal ligament was contained. The diverticulum is attached to the bowel nearer the mesentery on one side, that on which the mesentery of the diverticulum has been situated. The diverticulum was at once seen hanging free down in the abdomen, not tied up upon the mesentery of the bowels. The line indicating the attachment of its former mesenteric fold, is on one surface near that border of the diverticulum which is continued from the obtuse angle, a little posterior to where the ligament is seen in the illustration, fig. 17.

The intestine is entirely destitute of valvulæ conniventes, and the diverticulum was situated on it about three feet from the cæcum. The longitudinal and circular fibres are obscurely visible on it, but as much so as on the bowel, the coats being evidently continued from one to the other. The ligament at the extremity appears to arise or be continued gradually from the walls or tissue of the pointed portion of the diverticulum which runs towards it.

18. Dried preparation of the small intestine of a child, with diverticulum 1½ inch in length. Arises somewhat abruptly from nearly entire breadth of intestine, and near mesentery on one side. Body of diverticulum is oblique and curved, and enlarges as it passes outwards, at middle having a diameter very little less than that of bowel, which is half an inch. Towards the extremity it again contracts, and from the apex is continued a narrow pointed portion, doubled upon the rest, in the preparation. There is no appearance of a mesentery or ligament.

Intestine destitute of valvulæ conniventes.

19. I removed this specimen at a *post-mortem* examination of a male aged 71, who died much emaciated, but without any apparent disease. Diverticulum 1½ inch in length. Origin gradual from whole breadth of bowel, which is somewhat enlarged from before backwards opposite the diverticulum. It arose from the concavity of the intestine close to the mesentery, and passed up resting on and attached to the mesentery. It is somewhat conical, passing up to a rounded extremity, the diameter of the body being 1¼ inch, and that of the extremity 1 inch. Proceeding from its posterior aspect near the extremity, is a *secondary diverticulum*, forming a curved conical process, ¾ inch in length. This secondary diverticulum arises abruptly from the parent diverticulum at its back part near the extremity, a circular constriction passing round, across which some ligamentous fibres pass. It now curves downwards and backwards into the mesentery. It is at first somewhat dilated, being here the size of the little finger, half an inch in diameter, and then tapers to a small rounded apex, from which a ligament passed. From the upper and fore part near the apex of the parent diverticulum, there projects a small pouch, like a tubercle or split pea, which has much the appearance of a hernia of part of the coats. It begins gradually at one side, and two-thirds round its base is a slight constriction.

The parent diverticulum is evidently continuous with the structure of the intestine, both longitudinal and circular fibres being plainly continued from the one to the other. The circular fibres form a uniform layer, and at the base run obliquely across the bowel as its circular fibres, whilst the longitudinal fibres of the bowel turn and run up longitudinally, especially on the interior surface. The walls of the diverticulum and of the intestine have the same thickness. The secondary diverticulum is thicker and less transparent, but the circular fibres are obscurely visible. The little secondary hernia-like pouch is more transparent, and no fibres can be distinguished on its walls, as if it had passed through the muscular coat. In the dried preparation the apex of the secondary diverticulum is smooth and rounded, as if no ligament had been attached to it; this, along with the mesentery, having been removed before drying the preparation.

The intestine is wholly destitute of valvulæ conniventes. The diverticulum was situated on the bowel a distance of eighteen inches from the cæcum. In the illustration, fig. 19, to shew the position and form of the secondary diverticulum, I have drawn it as if passing from the side; naturally it passes backwards, and is not seen in a front view.

20. Dried preparation. Diverticulum three inches in length and very wide, with secondary diverticulum an inch in length. Origin gradual, from concavity of intestine, close to mesentery. Diverticulum, at its base or origin, measures two inches in breadth, and 1½ in thickness, being considerably larger than the largest side of the intestine. This on one side is twice as large as on the other. Diverticulum forms a wide blunt cone, the extremity smooth and rounded, but from its posterior aspect, near to the extremity, passes off a secondary diverticulum, resembling, but larger than that in the last preparation. This secondary pouch is a curved cone, bent backwards upon the parent diverticulum. It begins by a constriction, is one inch in length, and at the middle ¾ inch in diameter. From its apex passes a ligament or band, forming the thick free edge of a narrow mesentery, 1½ in length, which lies along the corresponding surface of the parent diverticulum, and is continued from the mesentery of the bowel. The circular fibres are plainly visible on the diverticulum, becoming gradually thinner towards the extremity, but they are more distinct on the intestine, the longitudinal fibres of which are likewise visible.

The intestine is destitute of valvulæ conniventes.

21. This specimen I examined in the recent condition, and it is now preserved as a wet preparation.

The diverticulum is 3½ inches in length, intestiniform, curved, and bound down at the extremity, by the attachment of a terminal ligament. Origin funnel-shaped, from the convexity of the intestine, but nearer the mesentery on one side. The diameter of the intestine here is 1¼ inch. The diverticulum, at its commencement, has a diameter of one inch. The body of the diverticulum may be divided into three stages. The first inch is funnel-shaped, contracting at the end to a diameter of half an inch. The second inch continues to have this diameter, and the last 1¼ or 1½ inch is contracted to a diameter of ¼ inch, the entire length of the tube being from 3¼ to 3½ inches. From the narrow but rounded apex proceeds a ligament ¾ inch in length, which is inserted into or continuous with the mesentery, half an inch from the mesenteric margin of the bowel, at the part where the diverticulum arises. This ligament is formed, as it were, by the serous coat of the diverticulum continued from its extremity. It is at first a narrow flattened band, but expands triangularly as it joins the mesentery, with the serous surface of which it is continuous upwards and downwards, and it consists of two layers of serous membrane, containing some fat and cellular tissue. There is no mesentery along the diverticulum. There is no appearance of effused lymph or morbid adhesion.

The extremity of the diverticulum being thus bound down to the mesentery, a ring or oval space is formed, bounded chiefly by the diverticulum, partly by the ligament, by half an inch of the surface of the mesentery, and by the side of the bowel from this to the origin of the diverticulum. This ring has a diameter of about an inch, and admits the points of three fingers. Through this, as I shall afterwards relate, a loop of intestine had passed, and, becoming strangulated, was the occasion of death. As I had only a mass of intestines sent me for examination, I am unable to say how far from the cæcum the diverticulum was situated, but, as there are valvulæ conniventes moderately marked near the place, I suppose it to be about the beginning of the last third of the small intestine.

22. Wet preparation. Diverticulum six inches in length, vermiform, and at the extremity tied down to the mesentery by a ligament.

The ring thus formed was likewise the cause of death, and the constriction near the extremity, and the dilatation beyond, seem to have been acquired or caused by the peculiar position of the diverticulum in the strangulated mass, as I shall subsequently describe.

It arises by a funnel-shaped commencement from the bowel, which has here a diameter of one inch, whilst that of the diverticulum is about half an inch. Its diameter is pretty uniform until near the extremity, 1½ inch after its origin there is a slight twist upon it, and at this part it is rather narrower than before and after. The last portion forms an abrupt oval expansion, one inch in length, and ¾ inch in diameter at the middle. This is separated from the rest by a narrow constriction, ¼ inch in length and diameter, and feels thick as if from the gathering together of the coats. On rubbing the coats between the finger, those of the diverticulum feel thicker than those of the bowel, and the dilated extremity of the diverticulum feels thicker still, though not so much so as the constricted portion. This constriction corresponds to where the diverticulum itself was embraced in the strangulating ring.

Attached to the extremity of the diverticulum, there is a peculiar pendulous body, soft, solid, and dark coloured. It is covered by serous membrane, and, on division, is seen to be composed apparently of fat and cellular tissue, very like a soft substance lying in considerable quantity between the two layers of the mesentery of the preparation and is therefore apparently analogous to one of the appendices epiploicæ on the great intestine of fat subjects.

Passing from the point where this pendulous body joins the diverticulum, is a ligamentous cord. It is round, strong, and fibrous-looking; is two inches in length, and then joins the mesentery by a triangular expansion ¾ inch in length, the serous surface of the mesentery running up each of its sides. The point where it becomes continuous with the mesentery is two inches up from the mesenteric border of the bowel. There is a narrow mesentery, ⅛ inch in breadth, running along the concavity of the diverticulum, as far as the constricted point, within an inch of the extremity, and arising below from the bowel, not from the mesentery of the bowel. The diverticulum is placed on the bowel, one-half nearer the mesentery on one side than the other. The bowel at this part presents no valvulæ conniventes, but there is a distinct trace of them about eighteen inches higher up the canal.

In this case then, it will be observed, there is formed a large ring, bounded for six inches by the diverticulum, for two inches by its terminal ligament, and for other two inches by the surface of the mesentery. Its diameter is about three inches, and it admits the whole hand.

Remarks on the preceding Cases.

Reviewing these notes of cases of diverticulum from the intestine, it is evident that they must be considered as original or congenital formations, and not of the nature of herniæ or yieldings of one or all of the intestinal coats. Figure 2, representing two false diverticula from the jejunum, presents a striking contrast in respect to the form of the projection, beginning by a constriction, and then bulging outwards, aneurism-like, in the mode in which a pouch formed by a dilating force from within usually enlarges. Here we find two pouches, and notice that they occur on the jejunum; but no instance has been recorded of the occurrence of two processes having the form or structure of the true diverticulum ilei. As long as the pouch was short or small, I would not regard a constriction at the commencement as a necessary character, as illustrated by the pouch on the œsophagus represented in fig. 1, and which, though almost as natural looking as the true diverticulum ilei represented in fig. 3, must still be regarded as a pouch formed by partial rupture or sudden yielding of the œsophageal walls into the recess between the bronchi, as we do not know of any other explanation, from development or otherwise, which could be given for the existence of such pouches.

Dilatations or pouches formed by rupture or yielding of one or all of the œsophageal coats occur, according to Meckel, only at the commencement of the œsophagus, as the pharynx contracts somewhat rapidly at this point, behind the cricoid cartilage, to the proper commencement of the œsophagus. A well-marked case in point, taken from William Hunter's collection, is related by Baillie,[1] in which a large pendulous pouch projected downwards and backwards from the commencement of the gullet. A cherry stone had rested there for a few days and formed a recess, which, during five years, became gradually dilated by the entrance of the food, and at last occasioned death by preventing the food passing to the stomach.

According to Rokitanski these cylindrical or conical dilatations or pouches on the œsophagus may form at all parts, but are most common near the bifurcation of the trachea. In fig. 1 it will be seen that the pouch exists precisely in this situation. Above this point the œsophagus is supported by the contact of the soft but still resisting membranous back of the trachea; but at the bifurcation of the latter it suddenly loses this support, and, although in all its course the œsophagus is as little supported on each side as it suddenly here becomes in front, it is still interesting to notice this occasional position of the pouch in the recess between the bronchi. This recess is naturally filled up by a mass of bronchial glands, usually of a dark colour; and in connection with this circumstance may be mentioned the case referred to by Rokitanski, "in which the mucous membrane of the œsophagus was dragged out in consequence of the shrivelling of an adherent tracheal gland."

---

[1] Morbid Anat. of the Human Body, London, 1799; Part 3, Plate I., fig. 2.

The diverticula represented from fig. 3 to fig. 22, inclusive, are, however, evidently original pouches or processes, continuous with the tissue of the intestine. They appear all to have been situated near, or towards, the lower or colic extremity of the ileum. Of the two of which I had the opportunity of ascertaining the exact situation on the length of the canal of the small intestine, one, fig. 19, was eighteen inches, and the other, fig. 17, twice that distance upwards from the cæcum. As to the situation of the diverticulum in the other cases, I can judge only by the valvulæ conniventes, as I had only separate portions of the intestine. We can, however, form a very accurate opinion usually as to the situation of a portion of bowel whether it was from the upper or lower end, or middle, of the small intestine, by the size or absence of these valvulæ. Very large and close together from about the entrance of the bile duct, downwards through the upper third, they gradually diminish in size and number in the middle third, and in the lower third are at first few and slight, and then altogether wanting. A portion of jejunum has thick coats, and when held up to the light, presents dark rings, and when everted or laid open the valvulæ are seen. A portion of ileum at the lower end again has thin coats, is more transparent, and no dark rings or valvulæ are visible. The same distinction is easily made although the preparation is dried. When held up to the light the jejunum presents numerous dark characteristic rings, and the lower portion of the ileum is thin, transparent, and destitute of rings. If, then, a portion of small intestine is presented to us, either wet or dried, in which there are no valvulæ or dark rings, we can with certainty pronounce it to be from the lower end of the ileum. The statements of anatomists differ as to how much of the ileum is wanting in valvulæ conniventes. It may vary in different cases, but M. Cruveilhier's statement, that "sometimes they are altogether wanting in the last two or three feet of the bowel," is, I think, incorrect, as representing them to exist too low down. My own observation would lead me to say that generally they are wanting in the lower third of the small intestine, that is for about six feet upwards from the cæcum, allowing twenty feet as the average length of the small intestines. In one instance in which the small intestine was twenty-four feet in length, the lower half was entirely destitute of valvulæ conniventes. At the commencement of the lower third, six feet from the cæcum, they are very small and scattered, or can scarcely be said to exist, and soon altogether disappear, so that (while I do not deny that sometimes they may exist nearer the cæcum) at least in the last four or five feet of ileum there are usually no valvulæ conniventes; and therefore a portion of bowel bearing a diverticulum, and free from these valvulæ, or dark rings, may be safely pronounced to have been taken from within this distance from the cæcum.

Now this conclusion embraces all the specimens of true diverticulum, twenty in number, above described and figured. In one or two there were traces of valvulæ where the diverticulum occurred, indicating that the situation was not more than five or six feet above the cæcum, whilst in all the others the intestine presented no valvulæ.

For this remarkable fact, that the true diverticulum occurs constantly near the cæcum (whilst the false diverticula occur indiscriminately at any part of the alimentary canal, and may be one or more in number), taken in connection with its evident original nature and continuity with the intestine, there must be a reason; and the view of Meckel that this explanation is afforded by the early connection between the vitelline duct and the intestine, appears a very satisfactory one, when it is further considered that the vitelline duct joins the canal at the same part where the diverticulum occurs, viz., at the lower part of the small intestine; and seeing that it is not a point capable of proof from actual demonstration, I am not aware of any objection that can be advanced against the conclusion, that Meckel's explanation is a very probable one, and a sound deduction from the consideration of all the facts of the case.

The occurrence of a cord or ligament passing from the extremity of the diverticulum is a point of considerable interest. This cord, which may be called the *terminal ligament*, may be looked upon as the remains either of the early vitelline canal, or of the more persistent pedicle of the vitelline sac, and of the omphalo-mesenteric vessels which passed along it. Its presence was distinctly noted in only five out of the twenty cases of true diverticulum above recorded. But I may observe that it may have been present in the cases of the dried preparations, and removed in the dissection without leaving any trace. This has been done in one of the cases in which I dissected it myself when recent, and now the dried preparation presents no trace of a terminal ligament, but a smooth apex. In all the cases in which the specimens were examined in the wet condition—Nos. 17, 19, 21, 22,—there was a cord or terminal ligament. In case 21, as already observed, this ligament was short and might pass for a serous adhesion, but this view of its nature was improbable, and in case No. 22 this ligament is a true cord, fibrous, rounded, and smoothly covered by serous membrane.

On several of the specimens there are what I have called *secondary diverticula*. In Nos. 19 and 20 these are very distinct. Also in No. 18 there is a narrower portion from the end; and in Nos. 16 and 17 there is a projecting corner or portion to one side, from which in No. 17 the terminal ligament passed off. These appear to be portions of the original diverticulum which had not undergone the same growth or enlargement as the rest. The form, as seen in figs. 19 and 20, seems to render it likely that the original diverticulum had been curved down, and that the part at the turn had become dilated, or that beyond it had ceased to grow.

The existence of a mesentery to the diverticulum does not appear to be constant. In the above dried preparations it may have existed, although no traces are now visible. In several there was a narrow mesentery running along one side. A mesentery is not necessary for such a process as it is for the intestine, where it is necessary both for the retention of the bowel and the conveyance of the vessels and nerves. In one of the cases, No. 15, the vessels were seen to run along one side, that nearest to the mesentery of the bowel; and in another, No. 14, they are seen to cross directly over the bowel on both aspects, and run along the diverticulum both on its concavity and convexity. That the diverticulum does not necessarily require a mesentery, as far as a conveyance for the blood-vessels is concerned, is also seen in a case figured by Cloquet.[1] "The appendix was three inches long. Its serous membrane was perfectly smooth. The mesenteric artery furnished a principal branch, which descended upon one surface of the appendix, giving off branches to the right and left; the other side was only supplied by some fine, irregular, vascular ramifications."

With regard to their relation to the intestine and mesentery, it will be noticed that one or two came off at the mesenteric attachment, and ascended, adhering to the mesentery,—that one or two came off from the middle or nearly the middle of the bowel,—that is, equidistant from the mesenteric border;

---

[1] Surgical Pathology; by Jules Cloquet. With Plates. London edition, 1832. Plate XVI., fig. 1.

but that the great majority were placed upon the bowel nearer the mesentery on one side than on the other, having usually about two-thirds of the circumference of the bowel on one side, and one-third on the other. Whether this situation was anterior or posterior could not at any rate be determined from the examination merely of a portion of intestine; and in reality, when we consider how the convolutions of the intestines naturally lie, the terms anterior and posterior become inapplicable. We commonly speak of the intestine presenting two borders and two surfaces—a mesenteric or attached border, and a free or convex border (although this is not a *border* in the proper sense of that term), commonly designated by the obscure expression of the part " opposite the mesentery," meaning thereby farthest from the mesentery. But the surfaces have no constant position; what is anterior at one time may become posterior at another, according as the convolutions change; what is the front of a convolution passing from left to right, may, by the turning over of that convolution, become posterior, and then the bowel is passing from right to left, as, it is clear, in the arrangement or retention of the bowels, there must be a turn to the left side for every one that exists to the right, except the last, to join the cæcum. Nor has the mesentery itself, except at and towards its root, any fixed anterior or posterior surface. It rather lies laterally, like the ribs of a partially opened fan, that surface which is anterior joined to a convolution passing from left to right, becoming posterior on the next convolution passing from right to left; and these, together with the convolutions, change about in regard to convexity and concavity, so that, of a sigmoid portion of bowel and mesentery, what was convex may be changed to concave. It is therefore deceptive to speak of the moveable intestine having an anterior or posterior surface, and in expressing any relation or attachment, as that of a diverticulum, we can only say that it was placed on the bowel nearer the mesentery on one side than on the other.

Lastly, it will be observed of these diverticula, that the great majority have a more or less oblique direction from the bowel, and that the bowel on one side is larger, usually considerably larger than on the other. Whether the larger side was the colic or gastric side in the above cases I am unable to say with certainty, but am inclined to the view that the upper was the gastric end, which would give the conclusion that the diverticulum was usually prolonged from the bowel obliquely downwards, *i. e.*, in the direction in which the chyle or intestinal contents were moving, and it is conceivable that the obliquity had been determined in this way, although I do not propose this as more than a suggestion. There is usually no valve or other means to prevent a free entrance to the cavity of the diverticulum. When the acute angle is well marked, there is a small crescentic fold at this part, and again in figs. 11, 13, 14, 15, and 20, there is a small crescentic fold at the angle where the two sides of the doubled-in piece of intestine meet over against the mouth of the diverticulum, like the septum of the bronchi facing the trachea; but such a crescentic fold will always be produced by bending up any portion of intestine at the sharp point of the bend, even although the interior was previously quite smooth; and any such small crescentic fold seen on one side, or near the mouth of the diverticulum, is not to be regarded as a valvular obstruction, or in any way involving this principle. The mouth of the diverticulum is usually funnel-shaped and smoothly open, so that the intestinal fluids must freely enter the recess, and again leave it by its own contraction.

Diverticula from the small intestine pathologically considered.

These diverticula may be considered pathologically under four heads. 1. As a cause of strangulation of the bowels. 2. As part, or the whole, of the contents of a hernial sac. 3. As predisposing to the lodgement and accumulation of foreign matters; and, 4. As a part liable to disease.

1. Under the first of these heads are included two of the preceding list of cases, being the two at the end, Nos. 21 and 22, and one or two others have been recorded by authors.

CASE I.—This is No. 21 in the notes and figures. The case occurred in the practice of Dr Archibald of St Andrews, and the intestines were put into my hands for examination by my friend, Dr Keiller. The patient, a young woman, was seized with symptoms of strangulation, and died after a short illness. The mass of intestines which I received was much gorged with blood, and there was a peculiar loop, embracing and strangulating a portion of the intestine, which was in a gangrenous condition, and easily gave way in the centre during the examination. On careful examination, in the empty state, and also by inflation of the bowels, I made out the arrangement already described in the notes, No. 21, and seen in fig. 21 A, and also the following particulars regarding the strangulation, as represented in fig. 21 B.

The diverticulum was, as I have described, bent over and fixed down to the mesentery by a short terminal ligament. Whether this ligament existed originally, or is of the nature of an elongated adhesion, I cannot positively say, only there is no appearance of adhesion or old inflammation elsewhere and it has all the appearance of a natural serous ligament, or double fold of peritoneum connecting a viscus. A loop or *ring* was thus formed. When the bowel and diverticulum are inflated, it has the appearance I have drawn, fig. 21 A. The ring is oval, and, when rounded, an inch or little more in diameter. It is chiefly formed by the diverticulum, the rest being completed by the ligament, and a small extent of the mesentery and side of the bowel. Through this fatal ring, lying loose and open among the convolutions of the intestine, a convolution had passed. It is now easily drawn out of the ring, and the part embraced is marked off by two constrictions; the first is immediately above the origin of the diverticulum, and is but slightly marked; the second is fifteen to eighteen inches (according as it is measured along the concavity or the convexity), farther up the canal, and is well marked, being for an inch in length contracted to one-half the diameter of the bowel. The narrower constriction appears to be higher up the canal, *i.e.*, on the gastric side of the other. These constrictions continue, notwithstanding the distention of the intestine with air. When the strangulated portion of bowel is replaced, and the intestine distended, forcible strangulation is then apparent. This will be readily understood when it is recollected that the size of the ring is not greater than the ordinary diameter of a moderately distended single piece of small intestine, whereas, besides two portions, there was of course also through it, the mesentery passing to the strangulated portion.

CASE II.—This is No. 22 in the notes and figures. It is a wet preparation in the Monro collection, and in the printed catalogue is designated, "Process from the ileum which formed a knot round a fold of the intestine, causing strangulation of the included portion of the bowel; the process near its extremity is attached to a ligamentous band."

On careful examination of the preparation, I made out the anatomy described in No. 22, and the following additional points connected with the strangulation. We have seen that there was formed a large ring, first, by the diverticulum, six inches in length; secondly, by its strong terminal ligament, two inches in length; and, thirdly, by two inches of the surface of the mesentery, besides a part of the side of the bowel; the ring being three inches in diameter, and sufficient to allow the whole hand to pass within it. Through this ring, a portion of small intestine, eighteen inches in length, had passed; and, on withdrawing it from the ring, which is easily done, the strangulated portion is still plainly marked off by constrictions. One of these, moderately marked, is immediately above the origin of the diverticulum, where the bowel had turned round and passed through the ring close below it; the other is situated eighteen inches higher up the intestine, and is well marked, being, for an inch in length, as narrow as the little finger, whilst at the lower constriction the bowel is contracted for a third only of its diameter, for the distance of an inch and a half.

Eighteen inches then of bowel, with its mesentery, which is thick and loaded with fat, had passed through the ring; but this as yet had not caused strangulation, as, after distending the bowel on both sides, there is ample room for it, in the three-inch diameter ring, to move freely in and out, without obstruction or bending round the edge of the ring. The loose end of the diverticulum itself has turned round and passed through between the two pieces of intestine, and has thus tightened itself round one side of the bowel. Through the fatal ring is seen to have passed, as shewn in the diagram, fig. 22 B—first, the two sides of the knuckle of the intestine, with the accompanying mesentery included between them; and, secondly, between these and in front of their mesentery, the pendulous body, the last inch of the diverticulum, and the contiguous portion of the terminal ligament.

But for the end of the diverticulum also passing through the ring, there would have been no strangulation with this amount of intestine and mesentery, as the ring is wide and loose, but, by the end of the diverticulum passing through, its first portion became tightened and the ring narrowed. There is first that side of the bowel where the diverticulum arises, strangulated by the diverticulum drawn across its front, the mesentery compressing it behind, then the other side of the strangulated bowel is crossed in front by the diverticulum, which then turns round behind it, and then forwards through the ring, thus going completely round this end of the bowel. The farther the diverticulum made its way through the ring, the tighter it would draw the ring and constrict especially that end of the bowel which it embraced on all sides. On the latter, accordingly, the stricture is now best marked. The narrow constriction on the diverticulum itself, within an inch of its end, is now seen to be the part where it is compressed by the ring, and the dilated last inch is the strangulated part beyond the ring. The ligament is not concerned in producing the strangulation, although primarily it formed part of the large ring. It has, as seen in the sketch, where it is represented by a dotted line, also passed through the ring, but it is not tight, and would act rather in preventing the diverticulum from going further through, if that were possible.

The great resemblance between these two cases will be rendered more apparent by reference to the figures 21 and 22. These, it will be understood, are merely outline sketches. They were made with the preparations before me, and convey a more accurate idea of the anatomy and arrangement, than could have otherwise been done. Fig. 21, A, represents the diverticulum and the fatal ring formed by it, and its terminal ligament, and the surface of the mesentery between the ligament and the bowel; the moderate constriction on the bowel close above the diverticulum, and the second constriction which was eighteen inches higher up. Fig. 21 B shews the manner in which the ring embraced the portion of intestine and mesentery. Figs. 22 A and B are corresponding views of the preparation in the second case, shewing the parts as already described. This preparation is figured by Monro. Of this I was not aware until after the above description and my two sketches of it were in the hands of the printer and engraver. Monro's figure (Plate III., p. 179, *op. cit.*) shews the portion of bowel strangulated by the diverticulum encircling it, and the latter itself through the loop. No farther information is furnished, either as to the anatomy of the diverticulum, or the history of the case, than that above given in the extract from the printed catalogue.

A case remarkably similar to the two preceding, in its principle features, has been recorded by Professor Pirrie of Aberdeen.[1]

A lad seventeen years of age, and in good health, was suddenly seized with symptoms of strangulation, and died in sixty hours from the commencement of the attack. On dissection Dr Pirrie found a portion of intestine, about twelve inches in length, strangulated through a loop formed by a diverticulum adhering to the mesentery by a ligament, the included twelve inches of bowel being greatly distended and in a state approaching gangrene. There was no adhesion between the loop and the included bowel, as the latter could be drawn out of the loop.

Two constrictions are seen on the drawing given by Dr Pirrie, one close above the origin of the diverticulum and moderately marked, the other twelve inches farther up, deeper and more abrupt, contracting the bowel to about half its diameter. The diverticulum was 1½ inch in length, and the terminal ligament being figured as of the same length, and as attached to the mesentery two to three inches from the edge of the bowel, the ring must have had a diameter of about two inches.

" The diverticulum," says Dr Pirrie, " at its entrance into the intestine, was furnished with a semi-lunar valve, formed by a doubling of the mucous membrane; but the diverticulum communicated with the intestine, and contained intestinal matters. It was 1½ inch in length, and terminated in a slightly dilated cul-de-sac; from the extremity of which a membranous band was sent off, one extremity of which was evidently continuous with the serous coat of the diverticulum, and the other as evidently not merely attached to, but becoming contiguous with, the anterior lamella of the mesentery." In the drawing, it is seen to leave the intestine near the mesentery on one side by a funnel-shaped origin. It is directed with very little obliquity, and has an average diameter of ½ to ⅓ inch, enlarges somewhat towards the extremity, and presents two slight constrictions, which however appear to be accounted for in the drawings by their position as they present themselves at different parts of the strangulating loop. It is not said how far from the cæcum the diverticulum was placed on the intestine, but from the statement, in the description of the pathological appearances, that "the lower third of the *ileum*," was collapsed, I may infer that it was about four feet above the cæcum.

---

[1] Monthly Journal of Medical Science. July 1849. " A remarkable case of strangulation, caused by a diverticulum."

I am also indebted to Dr Pirrie's paper for a reference to the catalogues of the Museums of St Bartholomew's Hospital and the College of Surgeons of Ireland.

The catalogue of the collection at Bartholomew's contains the following reference to a preparation which appears very closely to resemble the three preceding ones. "Portion of small intestine, from which a diverticulum is continued. The extremity of the diverticulum is adherent to the contiguous part of the mesentery, so as to form a circular aperture or ring. Through this aperture a portion of intestine, twelve inches long, passed and became strangulated. The patient, a lad subject to constipation, died four days after signs of strangulation of the intestine.

Referring probably to this case among others, Mr Lawrence[1] observes regarding internal strangulation, "Membranous cords forming adhesions frequently cause strangulation. They may be attached to any part of the cavity, or of its contents. The appendix vermiformis, the Fallopian tube, the omentum, and diverticula of the small intestine, when fixed at their loose extremities to some neighbouring part by such adhesions, may soon cause death in this way. I have several examples of such occurrences." Also Rokitanski mentions in his Manual of Pathological Anatomy, among the causes of internal strangulation, "an intestinal deverticulum (*verum*), which is directly or indirectly, by means of an obsolete vascular cord, attached to a certain portion of the peritoneum;" but he does not refer to any case.

---

[1] Treatise on Ruptures. London, 1838.

A more dangerous condition than such a ring lying loose and open among the coils of the small intestine could scarcely occur; yet, unless we adopt the improbable view that the ligamentous cords passing from the extremity of the diverticula were of the nature of elongated morbid adhesions, and therefore comparatively recent, we see the patients had obtained nearly the adult state before the accident of the fatal passage of the intestine though the opening had actually occurred.

The only natural ring or opening in the abdominal cavity is the foramen or canal of Winslow, and it has happened that a coil of intestine has passed through this opening, and become strangulated. Mr Lawrence refers to M. Jobert as an authority for this occurrence, and Rokitanski mentions that he "once found a large portion of small intestine strangulated in the fissure of Winslow."

We may say it is well that the foramen of Winslow exists above and not below the transverse meso-colon. Before the small intestines can reach the entrance of this passage between the greater and lesser sacs, they would require to be displaced over the transverse colon. It is interesting to observe how the moveable or loose intestines are kept together in the lower region of the abdomen. Encircled by the great bowel, and kept down by the transverse meso-colon and colon, from the latter of which the great omentum hangs down in front of them; the transverse meso-colon lying horizontally above them, and forming what I am in the habit of calling the great floating partition of the abdomen, separating the stomach, spleen, and liver above, from the coils of small intestine below, and moving up or down according as the hollow viscera below or above are distended or empty. It is evident, the foramen of Winslow would be a dangerous aperture indeed if the floating coils of small intestine were permitted to ascend to its situation.

2. A diverticulum may pass down and form part or the whole of the contents of a hernia. The only writer who refers to this, so far as I am aware, is Monro, whose attention appears to have been drawn to this point. He observes,[1] "The symptoms occasioned by such diverticula included within the hernial sac are in some instances somewhat different from those occasioned by stricture made upon a protruded turn of intestine. My grandfather has observed,—'that, instead of an entire piece of intestine being thrust out, one side of a gut has been stretched out into an appendix

---

[1] Op. cit., p. 94.

cæca. When this happens, the ingesta will not be stopped in their passage towards the anus, and the patient will go to stool, even though a strangulation of the hernia should come on.' But when the diverticulum only is contained within a hernial sac, it sometimes happens that a very violent inflammation spreads from the diverticulum to the contiguous intestine, and proves fatal. The unnatural process does not constitute an essential part of the intestinal canal, and has been cut off when in the sac of a strangulated hernia. Dr Wardrop did so in a case of this description, and after the operation the patient went to stool, and had a complete recovery.'' No reference is made to cases as a foundation for the above general remarks.

3. When we consider the position of these diverticula, we would expect them to be very liable to receive foreign bodies, and be the seat of the accumulation of intestinal matters. A glance at the sketches in the woodcut will shew that the intestinal contents must pass into them, almost as readily as past them into the canal below. When intestinal fluids are making their way actively down the canal, they must fill these diverticula, and they will remain full until, the intestine above having ceased to fill them, they are enabled by their own muscular power to empty themselves again. We would expect, then, that any foreign matters descending in the fluids would be very liable to pass into a diverticulum. On the other hand, it may be observed that they in this respect only resemble the normal cæcum, which is in this sense a diverticulum from the onward direction of the alimentary canal; and into it most foreign bodies and matters must go for a time, as soon as they pass the ileo-colic aperture, yet the lodgement and accumulation of such matters here is but a rare occurrence, even although, in the erect and sitting postures, the caput cæcum coli is a depending recess.

However, a case of such accumulation within a diverticulum ilei is referred to in the catalogue of the Museum of the College of Surgeons of Ireland. The preparation is thus described, vol i. p. 38, "A diverticulum on the human intestine. This preparation was taken from a young woman who died of fever; it forms a circumscribed bag about the size of a turkey's egg, and opens into the ileum at a point about three feet from the cæcum, by a passage sufficiently large to admit the end of the little finger; it is filled with hard brittle matter, apparently the solid residue of fæces stained with bile. The omentum and intestine in the neighbourhood were closely joined to the tumour by adhesions, the result of a former inflammatory attack; this woman had complained for many years before death of occasional very severe pains in the abdomen."

4. In conclusion, I have to add one case in which a diverticulum verum ilei was the seat of disease, without any apparent cause. This is the same case as No 17 in the notes, and fig 17. I found it in an adult male subject, and on inquiry learned that the patient, previously in good health, died after a few hour's illness, with symptoms of ileus or gangrene, but that the bowels had continued to move. The diverticulum was in a state of grangrene or approaching gangrene, healthy at its origin, as was also the intestine itself, but the rest of the diverticulum was gangrenous-looking. There was no strangulation or constriction at any part, nor was any other morbid appearance found. The diverticulum was carefully examined also by eversion, but inside there was no appearance of ulceration or irregularity, and it contained air alone when first examined. The diverticulum was, without any ascertainable cause, simply in a state of gangrene.

ART. III.—*On the so-called Fatty Degeneration of the Placenta.* By JAMES M. COWAN, M.D., Edinburgh.

The subject of fatty degeneration, ever since the microscope came to be extensively employed in pathological investigations, is one which has much engaged the attention of the morbid anatomist. We have had described, and no doubt very properly, fatty degeneration of the heart, kidneys, and liver; not to speak of those instances met with in the arteries, effused lymph, and extravasated blood, with which every pathologist must be familiar. At one time, indeed, there seemed to be a general tendency to refer every lesion displayed by the scalpel of the anatomist to the category of fatty degeneration—so much did this seem to be the case, that the phrase became a sort of by-word, and served as a synonym for every morbid condition that seemed mysterious, or was found difficult of explanation.

In the course of time, however, as a result of the conjoined labours of the microscopist and chemist, certain tests were discovered easy in their application, and, as some imagine, of unimpeachable accuracy, the mere application of which, if followed by certain effects, was held to determine whether the lesion under dispute should be regarded as an instance of fatty degeneration or not.

Thus the microscopist detected the highly light-refracting quality of the fatty granule, and its invariable simplicity of form and structure—characters, deemed by him almost sufficient of themselves to demonstrate its pathological significance. To these, however, the chemist added a third, which the former by way of confirmation almost habitually employs, although some—and it would seem with reason—have doubts as to its real importance and value. I of course allude to the solvent action of sulphuric ether on all bodies into whose composition fat enters as an important element. It may be added that if the mere appearance presented to the naked eye be trusted to as a criterion of the existence of this lesion, it will, with very few exceptions, even in the hands of experienced pathologists, almost infallibly lead into error. Fat, as is well known, is met with in the human system, both as a normal and abnormal element. In the shape of adipose tissue it is extensively diffused throughout the body, and serves many most important purposes. This amount of natural fat must not, however, exceed certain limits—if it be excessive, or present itself in situations where normally comparatively little fatty tissue is to be found, then it becomes a diseased condition, known to pathologists under the title of "fatty deposition." Of this we have a striking example in the case of the heart. Again, certain of the tissues of the healthy body, as muscular fibre, sometimes undergo a peculiar transformation, in whose ultimate composition fat can be recognised both microscopically and chemically. Seeing this transformation is attended by certain morbid phenomena, it is not unreasonable to consider it as a degeneration; hence the phrase "fatty degeneration." Other distinctions exist between these, somewhat allied and often confounded conditions; thus the one can be detected by the naked eye, the other needs for its demonstration the assistance of the microscope—the one presents an arrangement of minute vesicles lodged in the meshes of areolar tissue, the other an aggregation of corpuscular forms, not individually defined. It is necessary to bear these distinctions in memory, for the two phrases have been used synonymously;[1] and as the placenta is an organ in whose intimate composition fat cannot be detected, even by the refined processes of chemical analysis,[2] we perceive at once it cannot be the subject of fatty deposition, provided the limitation just stated to that phrase be admitted as correct.

One other remark I wish to preface, which seems to be called for by the vagueness with which some pathologists express their ideas, and it is this,—that in applying the phrase "fatty degeneration," to a morbid condition of any organ, it is intended to represent that the true and essential component structures of that organ and not the product of any morbid action that may have been accidentally excited, as exudation or extravasation, have been transformed into fat. In saying the heart and bloodvessels are in such a state, it is usually understood the muscular fibrillæ in the one, and the true component tissues of the walls of the other, and not the blood or nerves supplied to those organs, have undergone such a degeneration. So, in speaking of "fatty degeneration of the placenta," it is to be presumed to signify, the villi, that is the true and essential structures of the organ, have undergone such a morbid process.

---

[1] See Report of London Medico-Chirurgical Society (*Lancet*, May 24, 1851.)

[2] According to Mr Hassall's statement.

It is with a view of precision in thought and expression, to avoid misunderstanding, I have entered into the above few explanations, and shall now proceed to consider the so-called fatty degeneration of the placenta, with the object of pointing out what seems to me to be its true pathology. Having had occasion two years ago to peruse a communication read by Dr Barnes to the London Medico-Chirurgical Society on this subject, my attention has been for some time applied to its investigation; and as, after a rather extensive examination of specimens, and careful inquiry into the general facts of each case, I have been led to a different conclusion from that arrived at by Dr Barnes,—and the subject seems invested with a great pathological, not to speak of therapeutic importance,—I venture to lay before the profession the results of my investigation.

In Dr Barnes' paper, as reported in the *Lancet*[1] (May 24, 1851), two cases of this lesion are given, with an account of the symptoms experienced by the mother during utero-gestation, and of the examination of the diseased placentæ after they had been expelled from the uterus. Dr Barnes then, in some comments upon the cases, goes on to state "that he regards the occurrence of this change of structure in the placenta as highly interesting, both to the pathologist and obstetric practitioner. . . . . . The conversion of portions of placentæ into solid unyielding structure, and the consequent imperfect attachment of these portions and the surrounding healthy structure to the womb, give rise to hemorrhage, and premature labour may occur possibly during the life of the child." From this statement we are led to infer that Dr Barnes regards the essence of this lesion to consist in a degeneration of the proper tissues of the placenta; that is, of the villi into a fatty abnormal matter. What I wish to point out is, that the appearances presented by this morbid condition, and described by Dr Barnes, in most respects accurately, are capable of a totally different explanation; in short, it is not the villi of the placenta, but some of the constituents of blood which have become extravasated into its substance, that have undergone this so-called fatty degeneration; and that, consequently, we have to do with the results of a hemorrhage into the placenta.

---

[1] This Paper is now published in the Society's Transactions, vol. xxxiv.

The facts upon which this opinion is based will be found to be derived from two distinct sources:—

1*st*, From the general and minute examination of the diseased structure; and,

2*d*, From a consideration of the causes of the lesion, and the symptoms experienced by the mother previous to the expulsion of the placenta from the uterus.

It may be here stated I have heard it objected to accepting such evidence as the narration of symptoms experienced by a patient, and the account of her general state as certified by a physician, in support of a pathological question. But I need hardly say, considering the intimate and interesting relation the placenta stands in to the female economy, it would be a most anomalous circumstance indeed in the history of medicine, to find any great disease of this organ without some corresponding constitutional derangement.

3*d*, After stating the facts under each of these divisions, I shall discuss the views that have been taken of the pathology of this lesion, and hope to shew that that of the fatty degeneration of the constituents of extravasated blood is the only explanation comprehensive enough to account for the whole phenomena, and as such is entitled to our support.

I.—*The general and minute examination of the diseased structure.*

In my own experience I have met with ten cases of this lesion. If to these be added the two reported by Dr Barnes, and one mentioned by Professor Kilian of Bonn, an abstract of which appeared in the *Medico-Chirurgical Review* for 1851, we collect together thirteen cases.[1]

Let us first examine a placenta which affords a well-marked and advanced specimen of this lesion. The first glance of it will suffice to shew two very distinguishing characters,—a diminution in size, as compared with the organ in a healthy state, and an excessively bloodless appearance. The former holds true both as regards the thickness and breadth of tissue. I have seen specimens which did not exceed in size the half of the normal placenta, and which were expelled from the uterus at the full term of utero-gestation. It not only, however, appears of diminished proportions, but also much more compact than normally; in place of its spongy, loose, and soft appearance in health, it seems contracted and condensed in bulk, as if atrophied. When taken into the hand these characters are more distinctly brought out. In some points it feels very condensed and indurated, like a piece of boiled liver—to use the simile employed by Ruysch;—hence from the anatomists of the last century it received the appellation of "scirrhous." Its colour is yellowish-white; not a trace of the usual dark red, congested appearance of the healthy organ is visible. From this it is evident very little blood can have circulated or been contained within its vessels. Both the fœtal and maternal aspects of the organ present an unevenness of surface, and appear studded over with little projections, separated by sulci; some flattened, others more or less conical,—perhaps most distinctly so underneath the amnion; hence it has been described as "tuberculated." If the disease be principally situated between the chorion and decidua—for it is met with in one or both of two positions, either amid the true tissue of the placenta, or between the chorion and decidua—then the uterine surface will be comparatively smooth, for the blood coagulating has filled up the sulci. The characters of condensation and induration I consider owing to the extravasated blood having undergone coagulation, and the anæmic appearance to the effect of these coagula pressing upon the bloodvessels, and so obliterating the circulation. The degree of condensation and anæmia will be in proportion to the extent of the extravasation. I have seen it confined to one spot not exceeding one inch in diameter, and again so extensively diffused that it became impossible to find a remnant of structure presenting the usual colour and degree of consistence of the healthy tissue of the placenta. It is likewise to be anticipated, that the longer the placenta has remained within the uterus, after the occurrence of extravasation, the greater will be the degree of induration, for the farther advanced is the process of coagulation. Frequently, and most especially in those cases in which more than one extravasation has happened, we find the uterine surface covered with thin layers of coagulated blood, which can be easily separated the one from the other, like the coats of an onion. When a section is made through a thick deposit of these layers, the same appearance is presented as in a section of any aneurismal tumour in which some coagulation has taken place.

---

[1] Ten additional cases have been cited in a recent paper by Dr Barnes, in the 30th vol. of *London Medico-Chirurgical Society's Transactions.*

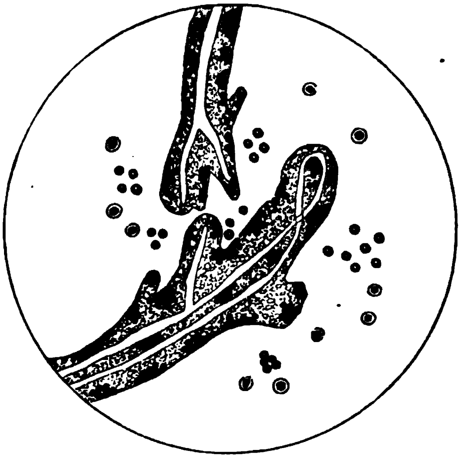
These laminæ are found to extend even between the membranes and internal surface of the uterus, particularly in those cases in which hemorrhage *per vaginam* has occurred during utero-gestation. It is found impossible to inject placentæ so affected; the bloodvessels are so compressed they cannot allow of the penetration of the matter of the injection, and on the employment of the slightest amount of force their walls speedily rupture.

On making a vertical section through the diseased portion, a sensation is imparted to the hand as if the knife were being made to penetrate something hard and resisting like a fibrous tumour. On looking at the surface of the section with the naked eye, we perceive here and there spots of a yellow colour and granular in appearance, by scraping which gently with the point of the knife we disclose the villi of the placenta, very much condensed and altered in appearance, but still preserving their true form, and not at all broken up. In the neighbourhood of these spots we find little deposits of a much harder consistence, not very accurately defined in outline, of a pale yellow colour, and not at all granular, but smooth in aspect. On breaking up one of these masses, we find in the centre bodies presenting quite the appearance of those already described,—only the villi are in much fewer numbers, present their usual form, and appear even more compressed than in the former situation. Offshoots are sent from these deposits in all directions, surrounding and compressing the villi. In certain specimens we are enabled to trace the gradual steps of coagulation the blood has gone through from a recent fluid state, presenting its usual dark scarlet colour, up to the transformation into a yellow deposit of no inconsiderable consistence. In one part of a placenta we meet with a deposit of quite recent fluid blood; in another some of the colouring matter has been absorbed, but it is still soft and not much altered in appearance; in a third we find a kind of encysted clot consisting of a nucleus of yellow fibrine surrounded by clear serous fluid; while lastly—and this is the most advanced stage in the process—we find a hard yellow mass of semi-cartilaginous density. It is rare to meet with true encysted coagula; according to my experience they are seen in one situation only, that is, between the chorion and decidua: the clot had so pressed upon the villi as to form a little cup-shaped depression; in which it was deposited and lined by an apparently serous structure. In some cases of very partial occurrence of this lesion, we meet with small rounded masses imbedded in the substance of the organ, quite hard and isolated to touch, surrounded by healthy soft compressible tissues, which on section present no cyst-wall, but still shew a distinct and pointed line of demarcation separating them from the rest of the placenta. The tissue of the organ is so lacerable and easy of distension, that the blood, when extravasated, finds no difficulty in insinuating itself amid the intra-villous spaces. This anatomical condition seems a sufficient explanation of the rarity of encysted coagula as a result of hemorrhage into the placenta.

Another appearance sometimes seen in placentæ affected with this lesion, and which occurs in connection with the other appearances already described, is that of a rounded melanotic mass, quite granular in section, and of the same consistence as the contents of a steatomatous tumour. We have twice seen this degeneration, never however very extensive. In the Edinburgh University Museum[1] is a preparation shewing little rounded masses of a melanotic aspect, scattered over the uterine surface of a placenta—no particular description however is given. Dr Simpson[2] regards this as a regular step in the process before the stage of yellow hepatization—if we may be permitted the expression—is reached; but from the rarity of its occurrence I am inclined to regard it as due to some special conditions not existing in every case.

The microscopic appearances presented by well-marked specimens of this lesion are very distinct and highly characteristic—indeed they are sufficiently diagnostic to distinguish it from all other diseased conditions to which the placenta is liable. Under a power of 240 linear diameters, the villi do not appear much altered from their normal condition. What has been remarked with the naked eye, in respect of the bloodlessness of the tissue is

Fig. 1.



Appearances presented by the villi and bloodvessels in a case of simple sanguineous extravasation.—(Case "Kelly.") Mag. 210 lin. diam.

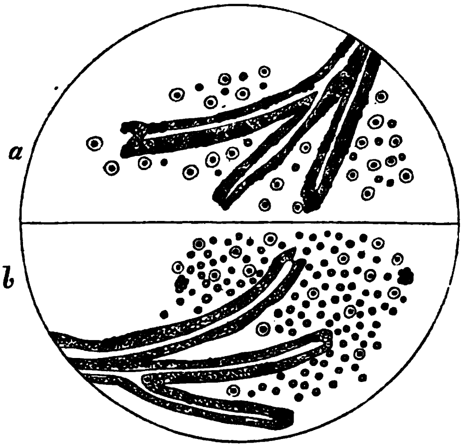
---

[1] *Vide* Catalogue of Museum, p. 256.

[2] Edinburgh Medical and Surgical Journal, 1836, vol. xlv.

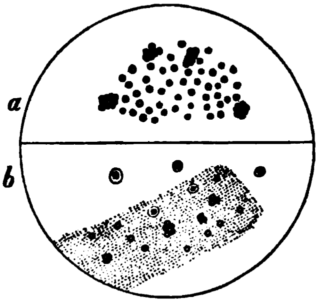
now quite confirmed; scarcely a red blood-corpuscle can be distinguished within the walls of a vessel—the villi in the vicinity of the extravasation seem compressed, and do not present their usual extent of surface—with the exception of a slightly more defined, because thickened external wall, their organization shews nothing abnormal. Those villi which are found imbedded amid the coagulated blood do not only present a compressed appearance with thickening of the external wall, but are also covered with numerous rounded granules which strongly refract light and contain no nuclei (*vide* fig. 1). They measure in size less than the half of an ordinary red blood-corpuscle (*vide* fig. 3 *a*). These bodies are usually collected in little groups, and are situated most thickly near the bases of the villi. Under the action of water, they swim loose in the field of the microscope (*vide* fig. 2 *b*), while on the addition of sulphuric ether they entirely disappear. The appearances presented again by the coagula which are deposited in layers and laminæ, are simply those of molecules and granules, arranged in a linear form, and which present the same behaviour on the addition of chemical agents as those bodies already described (*vide* fig. 3 *b*). The dark melanotic masses present nothing under the microscope but a collection of

Fig. 2.



a. Villi in a case of sanguineous extravasation.
b. Same preparation when water has been added.

Fig. 3.



a. Appearance presented by coagula when situated amid the tissue of the placenta or sub-amniotic. (Case "Kelly.")
b. Same when in contact with uterus or surface of chorion.

amorphous granules, which on the addition of nitric acid are completely dissolved. In the specimens, again, of this lesion in which we can trace with the naked eye the changes the blood undergoes, and of which a delineation has been given by Cruveilhier,[1] the microscope in one part detects the ordinary red blood-corpuscle of its usual form and size; in another spot these become less numerous, and shew a more corrugated external wall, and are mixed up with other cellular bodies of an irregular form, containing neither nuclei nor colouring matter; in a third place, these last are seen occurring in connection with oleo-albuminous granules, which, in a fourth portion of the diseased structure, form the only object in the field of the microscope.

Such is the result of the general and minute examination of placentæ affected with this lesion. From it I would deduce the following conclusions:—

1*st*, That this is a morbid condition denoted by a peculiar condensation and consequent atrophy of the true placental tissue; 2*d*, By a degree of anæmia—characters explicable on mechanical grounds. 3*d*, Under the microscope certain bodies are seen which can be traced as transformations of certain of the constituents of extravasated blood, and which present a characteristic behaviour under the action of chemical tests,—oleo-albuminous granules, in short, which are met with in no other morbid state of this organ with which the pathologist is as yet familiar.

II.—*The causes of this diseased condition, and the symptoms observed during utero-gestation.*

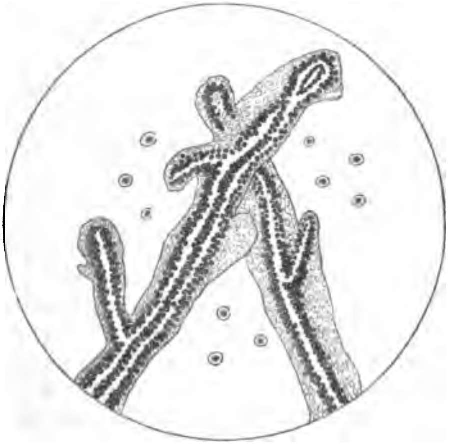
In eight out of the ten cases which fell under my observation, external injury could be clearly, with one exception, traced as the exciting cause. As a consequence of this, external hemorrhage happened in seven of these cases almost immediately upon the reception of the injury, so that there can be no doubt as to the connection between this cause and the effect. In the remaining one no direct injury could be traced, and it seemed probable the hemorrhage *per vaginam* which occurred during utero-gestation was due rather to some constitutional morbid condition. However it is matter of conjecture, because as the patient was a woman of abandoned habits it seemed not at all improbable that she did sustain such injury, although not to her knowledge. The part of the body on which the injury is inflicted may vary in each case. In four the injury was direct, that is to say, inflicted on the abdominal region. In three it was indirect, that is, received on other parts of the body not in immediate relation with the uterus. The effect of the injury is to produce a sudden and abrupt contraction of the uterine fibres, which causes a rupture of those vessels which form the bonds of connection between the uterus and placenta; the hemorrhage of course will be in proportion to the extent of the laceration. If this explanation be admitted we at once get rid of the difficulty Dr Simpson experienced in assigning the particular source of hemorrhage in what Cruvelhier has called "apoplexy of the placenta." Such external injury, if this be its mode of operation, can have no effect on the umbilical vessels.[1] In the normal contractions of the uterus the pains gradually increase in intensity up to a certain point, at which they attain a maximum, and from which they gradually decline. I apprehend it is in consequence of this arrangement that in natural labour laceration of the utero-placental vessels takes place so rarely. That such an occurrence does sometimes however occur is undoubted. In the North of England Medical and Surgical Journal is reported a case in which a woman died suddenly during parturition, and on the *post-mortem* examination a coagulum of blood was found between the placenta and the fundus of the uterus, which weighed 18 oz.

---

[1] Anatomie Pathologique, tom i., liv. 16.

In the remaining two cases of the ten the lesion seemed to owe its existence to a condition of the bloodvessels ramifying in the villi of the placenta, which has never yet been, so far as I know, described, and which is identical in appearance with that observed in the vessels of the pia mater in some cases of apoplectic extravasation. The bloodvessels were completely and equally coated over all their extent with oleo-albuminous granules (*vide* figs. 4 and 5). That these granules were part and parcel of the walls of the vessels, and did not result from any exudation exterior to the villi, is proved by their remaining unaffect-

Fig. 4.



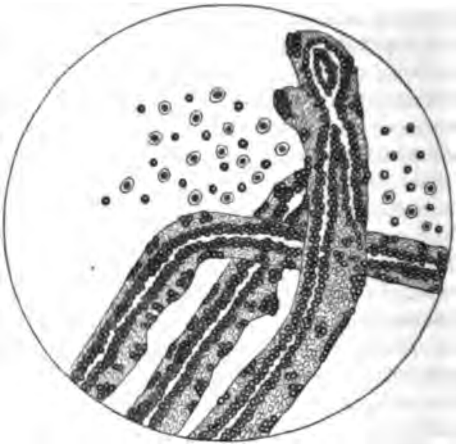
Appearance of simple fatty degeneration of the vessels of the villi.—(Case "Henderson.")

---

[1] M. Murat, in the *Dict. de Médecine*, states, laceration of the placenta may be produced by external violence or shocks communicated to the system of the mother, and of this lesion hemorrhage is a frequent occurrence.

ed on the addition of water, and not dissolving when sulphuric ether was added. In both cases the patients suffered previous miscarriages, the one two, the other three. The exact appearance presented by the placenta on each of these occasions could not for obvious reasons be ascertained. Most probably, however, as the patients were of a leucophlegmatic habit of body, the cause of abortion was the same in each particular pregnancy. In neither did external hemorrhage occur previous to labour, and in both the extravasation was limited, and in some parts quite recent.

Fig. 5.



Appearance of villi in a specimen of both lesions of sanguineous extravasation and fatty degeneration.—(Case " Wilkinson.") Mag. 260 lin. diam.

In addition to an exciting cause, there must be also some pre-disposition, for it would be too much to say every woman who sustained an injury during pregnancy would suffer from this lesion. It is very liable to occur in women who have previously aborted,—like the fœtus, the placenta has a strong tendency to the perpetuation of a particular type. In four of the cases that came under my observation the patients had miscarried more than once previously; and, it is to be presumed, from the same cause,—although from the circumstance of dispensary patients being rarely attended in two accouchements by the same medical man, and not unfrequently by women only,—it becomes impossible to ascertain this point with any degree of accuracy. Dr Lee of London mentioned, in the course of the debate which followed the reading of Dr Barnes' paper, that he had attended a lady who sustained eight abortions in succession, the pathological cause of which, according to his statement, was, "a change in the villi of the chorion, which had the effect of obliterating the circulation." Schulzius met with a woman who had twenty successive miscarriages—the state of the placenta he does not particularly mention. Dr Williams thinks an altered state of the blood, or some defect in nutrition, may have some influence in producing fatty degeneration of the placenta. This lesion, again, Dr Quain is inclined to regard as having its origin in some modification of the nutritive and circulatory apparatus of the organ by which the albuminous and fibrinous elements of the blood are retained within the vessels, or extravasated, and then subjected to the process referred to.[1] It may be stated, in a general way, all circumstances tending to weaken the vascular connection between the uterus and placenta, whether these be constitutional or local, must have an influence on the production of this lesion.

External hemorrhage, or the discharge of blood *per vaginam*, was noted as a symptom in six out of the seven cases in which injury was received. In the remaining case, in which external hemorrhage did not occur previous to labour, the disease was very partial, and confined to the centre of the placenta,—it did not exceed an inch in diameter. The patient remembered distinctly having received a blow upon the abdomen from a man during the sixth month of utero-gestation, after which she felt sick and languid, and was seized with rigors; in the course of two or three days these entirely disappeared, and she carried her child to the full term, when it was born alive, though rather emaciated, and of smaller size than normally. It is only in certain circumstances we can look for the presence of this symptom—only when the seat of laceration of the vessels is situated at the circumference of the placenta, or the extent of rupture so great as nearly to dissever the whole organ from the interior of the uterus. The hemorrhage may occur only once after the reception of the injury, or, as was observed in two cases, it may occur every month during the remainder of utero-gestation. It is curious to remark, that the hemorrhage in these cases occurred at periods corresponding exactly to the ordinary monthly molimina,—to the time at which the patient, had she not been pregnant, would have expected to menstruate. Perhaps the matter extravasated acted as an excitor of reflex action on the interior of the uterus, and that the uterus was thrown into action at those periods at which normally it is in its most irritable state. One of these patients, in whom repeated hemorrhages occurred, was a strong healthy woman æt. forty-five, who was confined of her fifth child, and never had a previous miscarriage. At the fifth month of pregnancy following a blow upon the belly she perceived a red discharge *per vaginam*, somewhat abundant, on which she applied to a medical man, who ordered rest and cooling drinks, which had the effect of checking it. Soon it became diminished in quantity, and at last disappeared. She saw no more of it for four weeks, when it again appeared at the time at which, in her unimpregnated state, she would have expected the catamenial discharge. It lasted on this occasion five days, and again gradually disappeared. She did not now consult any medical man, not feeling alarmed about it, although she was affected for some time with nausea, loss of appetite, and rigors. Twice more the same phenomenon presented itself at an interval of three weeks and a few days, until on the ninth month she was delivered of a male putrid child. The other case was that of a woman in her third pregnancy, who never had any miscarriage, and who appeared free of any constitutional taint. At the fourth month of utero-gestation, consequent on a hurt she received on the left haunch-bone, she sustained a hemorrhage *per vaginam*. Labour supervened at the end of the seventh month; and on three different occasions, corresponding exactly to her ordinary menstrual molimina, the same discharge returned in no very abundant quantity. In both of these cases, the placentæ presented characteristic specimens of this morbid condition; in both they appeared to bear a normal proportion to the size of the fœtus,—they felt hard and indurated. Overlapping the surface of the chorion in one, and penetrating down between the intra-cotyledonous spaces, in some places even to the amnion, was found a compact, tough, membranous substance, of greatest thickness in the centre, and becoming gradually thinner as we approached the circumference; it presented a yellowish-white colour, and on section was found to consist of layers which could be easily separated the one from the other, thus shewing it to be composed of successive deposits of coagulated blood.

---

[1] *Vide* Report of Meeting of London Medico-Chirurgical Society (*op. cit.*)

The general symptoms manifested after the receipt of the injury were very similar in all of the cases,—for the most part the patients complained of pain in the abdomen and back, nausea, anorexia, tendency to fainting, dizziness in the head and coldness of the extremities, and rigors. From the presence of this last symptom, some have inferred the lesion to be of inflammatory origin. No man at all conversant with uterine disorders need be told in how many instances shivering is a prominent symptom, although not the slightest trace of inflammation can be detected. The sympathy existing between the uterus and the general female economy is so intense, that the slightest disturbance of the former gives rise to a severe commotion of the latter.

According to Dr Barnes, in one case, flooding occurred twice without obvious cause, and unaccompanied by pain, at the third month, and again at the seventh, when labour followed. In the second case there was no hemorrhage previous to delivery. In neither is any idea given as to the relative extent of the disease; in both death of the child had occurred some time before delivery, from which it is to be presumed the extravasation must have been considerable in amount.

External injury, then, seems the great exciting cause of this lesion, which, when produced, if at all extensive, must inevitably lead to the death of the child.[1] Indeed, from my own experience—which is perhaps as yet rather too limited—and from what I have heard in conversation with older practitioners, I am inclined to believe—and beg to draw the attention of obstetricians to the point—that it is extravasation of blood into the placenta, arising from rupture of the utero-placental vessels, and leading to the death of the fœtus, and not "inflammation of the womb"—or of the placenta" or "rheumatic stitches of the uterine parietes," or even premature parturition itself, that is most to be dreaded from a woman considerably advanced in pregnancy receiving any bodily injury, especially in the lower parts of the body. The practical precept I should draw from this fact is, the necessity, on being summoned to such a case of careful auscultation, and watching the state of the fœtal heart; if pregnancy should have reached the seventh or eighth month, and the fœtal pulse tends to become slower and more feeble, the obligation the medical man is laid under of inducing premature labour, with the view of saving the life of the fœtus in utero. I am quite convinced many lives might be saved by practice based on such principles. Indeed, I had occasion myself, when a medical student, to witness such an occurrence happening before my eyes. It was the case of the wife of a gamekeeper in a distant part of the country, who, in the eighth month of pregnancy received accidentally a rather forcible blow upon the abdomen. Immediately thereafter she felt squeamish and sick, had rigors, slight pains in the back, not very severe nor regular in their time of accession, and was obliged to lie down in bed. I saw her in the course of an hour after the accident. I found her with a feeble pulse, cold surface, and tendency to syncope. On examination *per vaginam* there was no hemorrhage, the os uteri was occluded, the vagina was cool and moist. I then proceeded to auscultate the abdominal tumour, and found the "placental souffle" very limited, but quite distinct over a portion of the uterus, midway between the umbilicus and the right anterior superior spinous process of the ilium. After a good deal of attentive examination I heard the pulsations of the fœtal heart; they were faint and feeble in character, and numbered 100 per minute. The patient told me most distinctly that for several days previous to the accident she was quite conscious of violent intra-abdominal movements, and that since the accident she had felt no motion at all. Not possessing any legal qualification, and being rather timid of inducing premature labour on my own responsibility, without the concurrence of another medical man, I thought the more prudent course of treatment, especially as it was the patient's first pregnancy, was to adopt "la médecine expectante," and use those measures which I considered the best calculated to check the extravasation which I presumed was taking place into the substance of the placenta. Next day, nothing remarkable having occurred in the interval, after the most attentive and searching examination over every point of the uterine tumour, and in all kinds of position of the patient, I failed in detecting the pulses of the fœtal heart, not only at the place where I formerly had heard them, and which was marked with ink, but at nowhere else. A week afterwards, having in the meantime felt no movement in her abdomen, she began to complain of a strange sensation of something rolling in her belly whenever she moved in bed; she was seized with loss of appetite, depression of spirits, and vertigo; the mammæ, formerly large and well developed, became small and flaccid; in short, it became evident death of the fœtus in utero had occurred. At the end of six weeks the uterus threw itself into action, and expelled a dead male child, quite putrid, in all other respects well developed. The placenta presented quite the appearance I anticipated—the disease was very extensive, and principally confined to the central portions of the organ. I remain to this hour convinced that that woman might have been delivered of a living and viable child, had I had the courage to act on the principles I have been attempting to enunciate.

---

[1] It may be remarked, causes which might operate quite unobserved by the patient herself, are sufficient to produce a sudden contraction of the uterus and consequent severance of the utero-placental vessels.

The mode in which this lesion proves fatal need not be a matter of conjecture. By obstructing, and in some cases completely preventing, the circulation of the blood through the placenta, it must lead to one or both of the following results, as regards the fœtus. It must interfere with the due oxygenation of the fœtal blood, thus tending to produce death by asphyxia—and this undoubtedly happens in cases of sudden and extensive extravasation; or by interfering with the proper assimilative function of the placenta, of abstracting from the maternal blood substances necessary to the maintenance of nutrition and growth in the fœtus, will likely prove fatal by inanition: this will explain the great degree of emaciation presented by the fœtus in some instances of partial extravasation. Indeed it is matter of surprise how very small a portion of healthy placenta seems sufficient for the maintenance of life in, and carrying on of the processes of respiration and nutrition in the fœtus. A case happened in M. Dubois' ward, in Paris, in which a placenta fully two-thirds diseased was expelled from a woman, having been preceded by the birth of a child at the full time, perfectly well developed, and not a whit emaciated. That, and such cases, are doubtless exceptional; and I cannot see how such a lesion, if at all extensive, can exist with no prejudicial effect on the condition of the child. As regards the mother too, this morbid condition is not altogether devoid of danger. If coagulation does not take place, and a portion of the placenta remain adherent (supposing the present doctrines respecting the source of hemorrhage in placenta prævia to be correct), there is a risk of the mother dying from loss of blood. It may be sometimes fortunate for the mother, if coagulation does take place; although, as respects the child, it must always be attended with disastrous consequences. Might not compression exercised over the spot where the placental murmur is heard, be attended with advantage in such cases?

III.—*The pathological anatomy of the so-called fatty degeneration of the placenta.*

Two views in particular have been taken of the pathology of this lesion; the one regarding it as a type of inflammation, the other as a true fatty deposition in, or degeneration of, the tissue proper of the placenta. The former was that maintained by the anatomists of the last century—thus, Ruysch has described, and caused to be delineated a placenta, the whole of whose fœtal surface is studded with numerous " tubercles " of a consistence so great as to deserve the epithet[1] of "scirrhous." He considers it the consequence of inflammatory action; on section, he adds, the tubercles displayed a homogenous appearance, and presented no trace of organization. Wrisberg has described a case in which he found the uterine surface of a placenta covered with a layer of membrane 2½ lines thick in the intralobular spaces, and from 1½ to 2 lines in thickness where it overlapped the lobes. This membrane was white in colour, and of a hard and compact consistence. Its structure was cellular and nearly tendinous. To inflammation he imputes its origin. It may be added, the placenta was non-adherent to the uterus. Albrechtus writes "he has seen small tumours scattered throughout the substance of the placenta, of a whitish colour, and closely resembling scirrhous glands. And Mauriceau, in his book on Midwifery, gives several cases of placentæ he calls "scirrhous," or "affected with chronic inflammation."

---

[1] From this hardened or hepatized state of the placenta, the distinguished pathologist Rokitansky has been led to regard this lesion as a result of placentitis.—(*Patholog. Anatomy*, vol. i., p. 142—Sydenham Society's edition.)

As to the signification to be attached to the term "*scirrhous*," as used by these authors in their description of morbid conditions of the placenta, it may be stated that, primitively and etymologically, nothing is intended to be implied by the term, beyond a certain physical quality, viz. hardness. It was only as pathological science advanced, hardness being found a constant and most striking property of certain morbid growths which were coincident with a cachectic state of the system, and shewed a peculiar tendency to invade all neighbouring tissues, that "scirrhous" came to be applied to and associated in every person's mind with a growth of a *mali moris* or malignant character. Now-a-days, when one speaks of a scirrhous tumour, he usually means cancer. Formerly every tumour, provided it was possessed of a certain degree of hardness, received the same appellation. This opinion seems to derive some support from the experience of the present day, for we know that if there is any organ of the body more than another exempt from malignant disease, it is the placenta. I believe it possible to affirm, without the slightest risk of error, that cancer of the placenta is a lesion never met with, at all events so rare as to be unworthy of consideration. In Dupuytren's Museum at Paris, there is a preparation labelled "cancerous tumour of the placenta." No account is given of any examination this tumour has been submitted to, and to me I confess it seemed nothing else but a considerably hypertrophied placenta deprived of its blood by long maceration in spirits.

Pathologists of more modern times, and some even of the present day, have attempted to defend the inflammatory view of this lesion on the grounds of the microscopic appearances; the oleo-albuminous granules they regard as arising from an exudation of liquor sanguinis from the interior of the bloodvessels; these bodies in short they consider as diagnostic of an inflammatory lesion. I hope to shew immediately this opinion is based upon very limited pathological observation, and that these same granules, in place of being met with in states of inflammation only, are met with so frequently, it becomes a difficult task to enumerate the morbid conditions in which they do not occur.

That this lesion is not of an inflammatory character may be proved first of all by the general symptoms. Now shivering frequently occurs in the course of this disease, and some have grasped at this as indicative of inflammation. But rigors alone, in the absence of other febrile symptoms, such as pain in the belly, hot skin, bounding pulse, great thirst, &c., cannot be held as proving unconditionally a state of inflammation. In these circumstances they prove merely a derangement of the nervous system. By way of illustration let me instance the case of rigors occurring at the end of the first stage of natural labour. Who has seen anything of obstetric practice, and has not witnessed such a phenomenon; nay, perhaps frequently, and to a degree sufficiently intense? Yet who, in the absence of other febrile symptoms, ever thought shivering in these circumstances symptomatic of a type of metritis, or an indication for the use of the lancet. So in this case of disease of the placenta the patient no doubt shivers, but this is, as I have already stated, dependent on the intense sympathy which exists between the uterine and general system of the woman, and is caused by the irritation of the extravasated matter on the interior of the uterus. She presents, in addition, coldness of the surface of the body in place of heat; a depressed and weak, in place of a bounding pulse; thirst no doubt sometimes; and no pain on pressure over the seat of the placenta. In the face of such symptoms, who would argue the case was one of inflammation, acute or chronic, or be prepared to adopt the usual antiphlogistic treatment? The circumstance which militates most against the inflammatory theory of this lesion, is to be found in the fact, that a placenta so affected is never found preternaturally adherent to the interior of the uterus. It is contrary to all pathologists of the present day know regarding the results of the inflammatory process, to conceive it possible that such layers of coagulable lymph as are sometimes seen deposited between the uterus and placenta, could exist there without giving rise to the same results, as is generally seen when lymph is effused between two solid substances. In the ten cases which happened in my experience, not only was the placenta non-adherent, but it seemed to be unusually less adherent than commonly, for in several of the cases the whole of the contents of the uterus were expelled simultaneously. Another anomaly exists, which is difficult to be accounted for, if the inflammatory theory be accepted; it is this,—why if the most common and almost essential microscopic elements of an inflammation be recognisable, should these cells never attain any higher organization? why, in fact, should pus or abscesses be never met with in such conditions? It cannot be answered,—time is not allowed—for in many of the cases, months positively elapsed between the occurrence of the extravasation, and the birth of the placenta. In the analagous case of the bloodvessels of the pia mater, it has been explained by some pathologists, by saying there is no mucous membrane in their vicinity. But this reasoning cannot be applied to the uterus, for there they are in immediate proximity to mucous membrane in the shape of deciduæ. Another set of arguments in favour of this explanation has been attempted to be drawn from the microscopic examination. Peculiar bodies, argue these pathologists, have been detected by the microscope as composing the essential characters of this disease; these granules are identical in appearance and chemical properties with those seen in ordinary states of inflammation,—hence this lesion must be also inflammatory. This reasoning, however, is inconclusive, in as much as it is based upon a too limited generalization. While we admit, because we cannot deny, that bodies, so far as appearances go, identical with those met in this diseased condition, are to be found in the matter of abscesses, or in the exudation poured into the air-cells of a hepatized lung, we must not forget to notice, that precisely similar corpuscles are to be met with in many other morbid states of the body, which no one now-a-days would consider in the remotest degree inflammatory. They are to be found in great abundance as composing certain forms of cancer, viz., the encephaloid, in certain diseases of the liver and kidneys presenting not the least trace of inflammation; in certain softenings, such as that of muscular fibre in contact with pus, or of bone, or of the brain, nothing is more common than such microscopic appearances. If such be the case,—if such corpuscles are to be found so generally in diseased states of the human system, they must have some other value; their presence must be indicative of something else than any individual lesion, or particular species of morbid action. Any conclusion, therefore, to be deduced from their existence in favour of inflammation becomes equivocal. Now, to my mind I confess, I consider any conclusions to be derived from the presence of such granules as of trivial importance, except as a kind of confirmatory proof of an otherwise tolerably distinct morbid condition. Isolated *per se,* and not contained within other normal structures, it cannot be held they determine the specificity of any lesion whatever. In the case of pneumonia, it cannot be said, oleo-albuminous granules are characteristic of the disease; for not only are they, but molecules and granules, epithelial scales, and true pus-corpuscles, highly organized cells, found in the matter poured into the air vesicles; the former are merely an early stage in the development of the elements composing the matter exuded. It is of little moment to our present purpose to inquire—besides, it is a disputed question—whether or not the true pus-corpuscle is really formed from these granules; this, however, is certain, the former are more highly organized than the latter. Again, in some soft cancers, they are found in great abundance. What shall we conclude from this fact? that soft cancer is an inflammatory lesion! It is impossible; for, in addition to these bodies, and in as great abundance, are seen cells highly organized, possessing one, two, or three nuclei, and these again often nucleoli. Besides, the general symptoms will not permit it. Shall we not rather conclude that the former are the source, or rather the remains of the latter, for it is in the softest forms that these fat-corpuscles are the most numerous;—they seem to me to be the lowest type of organization, the first in the series of cell growth, the penultimate in their decay. In the case of the placenta, that these granules do result from the softening of an exudation external to the villi is proved by their being loosely adherent to their walls, not incorporated in their true structure; easily acted on by water, which causes them to swim loosely in the field of the microscope; and, being easily dissolved on the addition of sulphuric ether,—a striking difference of action we have seen in those cases in which the oleo-albuminous granules were deposited around the bloodvessels within the villi, and therefore removed from the action of water and sulphuric ether. In this last case, it has been disputed whether the corpuscles are owing to an inflammatory exudation, or to a fatty degeneration of the true tissues of the walls of the bloodvessels. In the analogous case of the disease in the bloodvessels of the *pia mater*, in connection with cerebral softening, Mr Paget[1] maintains the lesion in that case consists of a true morbid condition of the proper tissue of the vessels, while Dr Bennett[2] has endeavoured to prove, it is in consequence of an exudation of *liquor sanguinis*, and is therefore inflammatory. I need hardly add, the same general arguments which have been stated already, apply with equal force against the inflammatory view of this condition. I have not been able, in those two instances which fell under my observation, to satisfy myself that the layer of granules was deposited underneath an external membrane, as Mr Paget has pointed out in the vessels of the brain. The regular and equal distribution of the granules over the exterior of the vessels; the non-coincidence with any general inflammatory symptoms; their occurrence in patients of a certain cachectic constitution, in whom it is reasonable to suppose there existed some modification of the nutritive process; their continuance in a low stage of development, with the fact of fibrine occurring as a constituent of the coats of the vessels,—all such circumstances go in support of the view of fatty degeneration of the coats of the vessels.

---

[1] Lectures on Surgical Pathology, vol. i., p. 142.

[2] Edin. Med. and Surg. Journal, 1843, vol. lix., p. 344.

That this morbid condition of the placenta is not an instance of fatty deposition,[1] I consider demonstrated already by what has been said in the preface to this paper regarding what is to be understood by the phrase " fatty deposition."

1*st*, The terms imply a certain amount of fat to be a normal constituent of the organ. True fat cannot be detected, even by chemical analysis in almost inappreciable quantity, in the tissue of the placenta; the first condition, then, for fatty deposition is awanting.

2*d*, The terms imply a deposit of true adipose tissue. In the placenta true adipose cells have never been seen under the microscope, either in its normal or abnormal state.

The only other view of the pathology of this lesion which remains is that of fatty degeneration, and this is divisible into two branches, according to the supposed material which undergoes degeneration; some regarding it as the tissue proper of the placenta, others, a material foreign to its normal constitution.

The facts opposed to the former view are the following:—

1*st*, The villi, even in the most exaggerated forms of the disease, present their natural configuration; they never appear broken up nor disintegrated; occasionally, they are more compressed, and present a smaller extent of surface than normally, but this is quite explicable on mechanical grounds.

2*d*, The placenta in these cases feels hard and condensed. If such were the real explanation of the phenomenon, it would be natural to expect the tissue of a softer consistence.

3*d*, The fat globules are situated on the surface of the villi, and do not form part and parcel of their structure. Besides these, there are certain characters presented by the placenta so affected,—such as an atrophied and shrunken state of the organ, with an appearance of anæmia,—which are capable of explanation only on the hypothesis of a foreign matter having been extravasated amid the tissue of the organ, and undergone a process of condensation.

---

[1] Dr Quain, in his paper on " Fatty Heart," in the London Medico-Chirurg. Society's Transactions, restricts what is implied by the phrase fatty deposition in the same manner. (January 1851.)

It is to this view of the pathology of this lesion we are therefore driven, as the only one comprehensive enough to include all the phenomena. I have said already amply sufficient to shew the facts on which this hypothesis is grounded. It would be a mere repetition stating these reasons in the present place. It will suffice to say, that this lesion presents an instance of a degeneration of the fibrine of the blood, which Mr Gulliver[1] has found to take place in certain conditions external to the body, as in the softening of fibrinous clots, and in the formation of "adipocere" from the fibrine composing muscles, and which several pathologists have witnessed in cases of pulmonary and cerebral apoplexy. Viewed in this light, it cannot be regarded as a new morbid process, but merely a diseased condition found in a previously unknown locality.

ART. IV.—*Experiments on the Communicability of Cholera to the Lower Animals.* By W. LAUDER LINDSAY, M.D., late Resident Physician to the Surgeon Square Cholera Hospital, Edinburgh.[2]

It has often appeared to me surprising, in studying the literature of cholera, that, although the etiology of this dire disease has received a vast,—I might almost say unprecedented, amount of attention from the profession in every part of the civilized world, during the last twenty years, and all the known machinery of science and art has been put into vigorous operation to ascertain its essence or stem its progress, only with the result hitherto of baffling all human ingenuity; and notwithstanding the thousands of volumes that have been published on its etiology, symptomatology, pathology, and treatment, comparatively few attempts have been made, so far as I am aware, to propagate the disease from the human subject to the lower animals,—in them deliberately and experimentally to study all its varied phases. It has been too much the custom among authors, I fear, in the investigation of a subject of such acknowledged difficulty, to prefer the construction of specious and ingenious theories upon supposed or uncorroborated facts, which, perhaps, have been too hastily examined, to throwing aside prejudice and theory of every kind, as fettersome and dangerous, and patiently, silently, and dispassionately observing the workings of nature. It can, however, only be by the careful accumulation of the unprejudiced, simultaneous, and isolated observations of individuals that safe data can be obtained whereon to found just and sound conclusions,—which may be practically acted on and followed out,—as to the nature of a disease about which so little of a specific nature is confessedly known.

---

[1] London Medico-Chirurg. Society's Transactions, vol. xxii.—Mr Gulliver considers a low state of the vital powers as favourable to the formation and softening of coagula.

[2] The publication of these experiments in their present shape is due to the kindness of Dr Warburton Begbie, visiting physician of the said hospital, to whom I take this opportunity of acknowledging my deep obligations.

Seeing no reason why the same poison which produces or propagates the disease in man should not equally, under similar circumstances, affect creatures so much akin to him in their structure and habits as the dog and cat, it occurred to me to take advantage of my opportunities as Resident Physician to the Surgeon Square Cholera Hospital, Edinburgh, by endeavouring to communicate the disease to these animals, through the medium of the different excretions of the patients therein treated. This I proposed to myself to do chiefly in three ways, viz:—

I. By feeding them on the evacuations,—dejections, vomit, and urine,—of patients labouring under various stages of the disease.

II. By feeding them on the blood, drawn during life, or taken from the heart and great vessels after death, of man,—or on various of the solids and fluids, *e.g.*, the blood, muscles, and viscera,—of other animals,—affected with the disease.

III. By causing them to inhale the effluvia from these evacuations and blood, and from the clothes worn by, and saturated with the perspiration and breath of, patients.

By the two former modes of procedure, I was led to expect, from what I have seen of cholera, and from the microscopical and chemical examination of the fluids in question,—and I obtained—negative, *i.e. non-specific* results. By the latter I hoped for, and succeeded in producing, *specific* results, viz., what appeared to me to be cholera, which was simultaneously developed in four animals, and was fatal in two of them.

Every one must remember the great importance that was attached by many authors, during the epidemic of 1848–9, to the presence in the cholera evacuations of certain insignificant vegetable cells, which were originally figured and described by Brittan and Swayne under the name of "annular bodies," "cholera corpuscles," "cells," "fungi," &c., and by others to the much more astounding fact that these discharges were found teeming with millions of microscopic animalcules. On the existence of the former, as the supposed poison agents of the disease, was based the very ingenious fungus-theory of cholera; on the presence of the latter, the no less ingenious but more fragile animalcular theory, each having its hosts of supporters. I have seen all the forms of the so-called "annular bodies" described by Brittan and Swayne, in the evacuations of patients under my own charge, and I have also seen the same fluids abounding in minute vibrios in a high state of activity; but having observed the former only in small quantity, and being able in many instances to trace them back to their original source, the food; believing moreover, that the latter exist equally in all decomposing animal fluids, I could not look upon either as having any causal relation to the disease, and therefore did not anticipate any specific effects from the administration, to animals, of fluids, which I knew by microscopical examination, to contain both in abundance. I found that these evacuations, which were given to the animals in all degrees of freshness and putridity, and in all states of concentration and dilution, produced the simple effects of any other irritant or putrid matter, viz., a non-specific diarrhœa, attended, in the case of the more acrid substances, such as concentrated urine, by the secretion from the intestines of an increased amount of mucus to lubricate and defend the canal. The offending matters were speedily thrown off from the system by the stomach or the intestines. This is what, *à priori*, I was led to expect; for, even supposing the presence in the cholera evacuations of a considerable amount of a volatile poison, it is easy to conceive that while this might immediately generate a specific disease, when entering the system by such an extensive surface as the pulmonary and cutaneous systems, it might be comparatively inert when applied to a surface comparatively so limited as the gastro-intestinal mucous membrane.

By other writers, again, the blood is said to be the recipient and medium of retention and reproduction of the morbific matter of cholera. With regard to this point, I can certify that the blood taken from cholera patients after death and given to animals, was apparently attended by more severe diarrhœa and exhaustion than followed the administration of any of the evacuations. I have reason, however, to look on this effect more as coincident and accidental, than as essentially depending, for its causation, on the blood in question.

During my attendance on cholera patients, I had been constantly struck with the peculiar disagreeable odour given off by their bodies, in the copious clammy perspiration, which is such a common feature of the disease,—the exhalations from one patient being frequently sufficient to taint a large ward. Moreover, I have frequently noticed nurses and attendants suddenly seized with overpowering nausea, accompanied or not by diarrhœa and vomiting, *i.e.* the premonitory symptoms of the disease,—after having accidentally inhaled the effluvia contained in the breath or perspiration of patients for a greater or less space of time; and I have known several fatal cases of cholera occurring in persons who had merely washed the clothes of patients dying of the disease, even at considerable intervals subsequent to their death. One of the washerwomen of the Edinburgh Cholera Hospital, who died of cholera on the 11th October last, after three days' illness, attributed her seizure, and probably with justice, to the exhalations from the clothes of a cholera patient, which she had occasion to wash;—and one of the nurses in the same hospital, was more than once seized with very suspicious premonitory symptoms on leaving her duties in the wards for a day to wash patients' clothes in the washing-house. Experience has led cholera nurses to believe that by far the most dangerous part of cholera patients is their breath and sweat and their clothes, which are more or less saturated therewith. I have read of numerous instances, where the clothes of cholera patients, having been dried, packed up and sent to different parts of the country, were unpacked after the lapse of various considerable periods, with the result of instantly generating or rather propagating the disease in individuals previously healthy, and in places where no cholera had until then occurred. From the observation of facts such as these, I was led to believe it extremely probable that by the pulmonary and cutaneous systems the cholera poison is received into and subsequently excreted from the human body; and a natural deduction from this belief was, that by exposing animals to these supposed poisonous exhalations, *i.e.*, by acting on the skin and lungs, and thereby at once attacking the blood,—and at the same time putting them as much as possible in the same circumstances as the human subject previous to a cholera seizure,—combining ochlesis, damp, filth, poor and innutritious food, deficient ventilation, &c., I was in a manner justified in anticipating some amount of success. By copious dilution with pure air, this noxious effluvium, like all other poisons when diluted, may be comparatively harmless, even when inhaled ; but, if concentrated in a small space, and an animal of any kind be for a length of time compelled to inspire it, surely it is reasonable to expect that a new focus of disease will speedily be produced. If fomites, I thought, were capable of inducing or exciting the disease in a *man* previously predisposed, why should I not be able, by imitating the circumstances, which are well known to predispose to cholera in him, by bringing together the localizing causes of the disease, to excite it in the dog? It is not sufficient to say, that few or no instances exist of the transmission of disease from man to the lower animals, though examples of the propagation of various diseases peculiar to these animals from them to man are far from being uncommon. One great cause of this may be that the subject has never been properly investigated. I cannot help expressing an opinion, that cholera, as an epizootic or as a sporadic disease among certain genera and species of the lower animals—as the result of the same natural influences, whatever these may be, which produce the disease in man,—is more common than is usually held to be the case ; but in them the disease may run a much more rapid and insidious course, and not being thus detectable by symptoms during life, can only be found out by an examination of the *post-mortem* appearances. Cases of rapid death, with symptoms of intestinal disease, are so common among many of the lower animals, that *post-mortem* examinations are seldom had recourse to,—a circumstance which is much to be regretted. Every one who has seen cholera must know that even in man rapid cases,—fatal without any of the ordinary symptoms of purging, vomiting, cramps, or lividity,—are by no means rare, and that, were it not for the simultaneous occurrence of other cases of the usual type, and during the presence of an epidemic, such instances would most probably be attributed to some other cause than cholera. It will be found that in the case of the animals which I compelled to inhale the effluvia from the clothes of a cholera patient, I had combined the following circumstances :—

The previous habitual use of poor, innutritious, and putrid food, in the form of the various cholera evacuations.

Confinement, to a greater or less extent, in a damp apartment, in an atmosphere charged with the non-specific but still noxious effluvia from their own evacuations,—the probably specific exhalations of the cholera discharges,—besides latterly, the specific volatile poison from the clothes of a human cholera patient.

Ochlesis, or overcrowding.

Heat to a certain extent,—gas being burned in the room, in which the animals were confined, during the night, for the purpose of lighting a dark avenue, which led from the street to the hospital entrance-door.

Filth to a slight degree,—the floor of the apartment being sometimes covered for from one to three days with their own excretions.

Deficient ventilation,—and atmospheric pollution from the various causes above mentioned,—but only to a limited extent, the room being periodically cleaned and ventilated.

I selected the dog as the subject of experiment, from its most nearly approaching man in the structure of his digestive apparatus, and in the nature of his habits and food. Many of the lower animals, which might readily be obtained for experimental purposes, are herbivorous, and could not be made voluntarily to swallow blood and putrid animal fluids: many moreover, have a stomach so organized, that one of the prominent features of cholera, viz., vomiting, could not be exhibited by them.

In consequence of my being otherwise occupied, and my general instructions that the clothes of patients admitted into hospital should immediately be fumigated with various disinfectants, steeped in hot water, and laid aside in the open air, previously to being washed, having been rigidly carried into effect by the hospital nurses, I had not an opportunity of testing the effects of fomites until a comparatively late period of my experiments (21st November);—then only in one case, on account of the stoppage of the disease in Edinburgh, and that case an unfavourable one. The result, however, was, that of four animals seized on the same day, two died with symptoms and pathological appearances exactly resembling those I have myself seen in the fatal cases of cholera which have occurred in the Edinburgh Cholera Hospital during the winter. But, in order that I might not deceive myself as to the production of a result which I was very anxious to obtain, I made the *post-mortem* examination of one of the dogs, in presence of Dr Warburton Begbie, the visiting physician of the Cholera Hospital, and the pathological lesions of both animals were shewn to several other gentlemen of great experience in the pathology of cholera, as observed both in Edinburgh and London,—all of whom, I think, agreed with me as to the similarity or identity of the lesions witnessed in these animals and in the human subject. To guard against another source of fallacy, I shewed the same appearances to a gentleman of acknowledged experience in the anatomy and pathology of the dog and other domesticated animals, and asked him whether they resembled the lesions observed in any disease of the dog with which he was familiar; his reply was in the negative, and I have failed in finding on record any disease of the dog or other animals having the same or similar *post-mortem* appearances, unless in a few cases, which have been avowedly published as cholera.

When I began these investigations I was not aware whether any and what experiments on this subject had previously been made in Britain or on the Continent; but I had not proceeded far when my attention was directed to a paper on "The Communicability of Cholera to Animals,"[1] by Mr Marshall of London, who mentions briefly the aggregate of the attempts at the artificial propagation of the disease hitherto made throughout the world. To that paper I cordially acknowledge my obligations for putting me at once in possession of all that has been, up to the present day, accomplished in this branch of experimental etiology. Still the mode and aims of my own attempts were in no way affected by the information therein contained; for I found that, while the author mentioned scarcely any good instances of the natural influence of cholera in animals, he detailed but very few positive and no conclusive results of direct experiment. In a word, all the experiments hitherto made appear to be either negative in their results, objectionable, or inconclusive. The mode of procedure in almost all cases has been injection into the veins, cellular tissue, or stomach; and the animals chiefly employed in many of the experiments to which he alludes were rabbits, guinea-pigs, goats, and fowls,—animals which, from their dissimilarity in structure and habits to man, we should not expect to be equally subject to the same poisonous influences. Injection into the veins or cellular tissue is, I think, a mode of experimenting exceedingly objectionable on the ground of its danger, and the liability of its results to fallacy,—the very violence used, especially in the case of a timid animal like the rabbit, of itself might do irreparable mischief. Its effects have uniformly been unfavourable and unsatisfactory. Injection into the stomach, again, is a most unfair and unnatural means of forcing the introduction of food. It appears to me infinitely preferable, in such experiments, to have it swallowed voluntarily. My anticipations of the effects of the cholera poison on the pulmonary and cutaneous systems of animals were thus nowise affected by the negative or unsatisfactory results referred to in Mr Marshall's paper.

---

[1] Brit. and For. Medico-Chirurgical Review, April 1853.

In the detail of my experiments I have been minute—perhaps unwarrantably so—in mentioning the collateral circumstances; but this has arisen from a belief that, in the investigation of such a subject, where the results will be viewed in different lights by the supporters of opposed theories, it is important to state candidly *all* the conditions, however insignificant they may appear,—no less than from a wish that, in future experiments of a similar nature, the errors and omissions of my own rough and hasty essays may be rectified and supplied.

The room in which the following experiments were conducted, was an apartment some ten feet high, thirteen and a half feet long, and eight and a half feet broad, having two small windows looking to the north, and containing a gas-meter and other appurtenances. For the first two or three weeks after the hospital was opened (which was in the end of September last), and prior to the commencement of the experiments, the porter slept in this room, but he was obliged to give it up in consequence of its great dampness and coldness, the floor being slightly sunk below the level of the ground, and the pipes of a cistern above having once or twice burst, and the water percolated through the roof; it had always a damp, musty smell. In this apartment several dogs were shut up for various periods, and allowed to feed, when so inclined, on different evacuations of patients. These were occasionally mixed with milk, porridge, beef, &c., to insure their being swallowed. As was naturally to be supposed, most of the animals shewed a decided aversion to their food, unless impelled by hunger, and some of them refused food for days; but I generally found *every* kind of matter ultimately swallowed voluntarily. One dog licked almost anything; the others were more cautious and capricious in their appetites. It must be obvious that, operating as I did, in an apartment of a cholera hospital, I could only, to a comparatively small extent and for a comparatively short period, produce and maintain such a combination of circumstances as is usually found to predispose to cholera in man. I did not feel myself justified in keeping the animals shut up in the same apartment, in an atmosphere greatly polluted by the exhalations from their own bodies and excreta, and from the putrid matters on which they were feeding, for a longer period than two or three days consecutively, on the ground alone of danger to the inmates of the hospital. I was therefore under the necessity of having the animals let loose and the room well cleaned and ventilated at short intervals. It happened accidentally that a barrel of chloride of lime and a carboy of solution of sulphurous acid were left in the room while the experiments were being made, and these, though they had no appreciable odour, must have in some degree acted as disinfectants.

The animals experimented on were:—

Dog No. 1.—A male; mongrel terrier; comparatively young; weighing about 10 lb., and having the following measurements:—

Length of head[1]..................... 5 inches.
... body[2]......... ....18 ...
Greatest girth of thorax..........15½ ...
... of abdomen........14½ ...
... of head............10¾ ...

When obtained (on October 24), was very lean and in poor condition; shy, skulking, timorous; apparently quite healthy; after being a few days in hospital became active, lively, and frolicsome.

Dog No. 2.—A female; a mongrel spaniel of the King Charles type; also young; weighing about 6 lb., and having the following measurements:—

Length of head .... 5½ inches.
... body.......................15 ...
Greatest girth of thorax...........11½ ...
... head .... 8½ ...

A very restless, timorous little animal; appeared healthy, but was very thin and in poor condition on admission (Nov. 7); never seemed to have become naturalized to the house, always meditating and attempting escape;—refused food for considerable periods.

Dog No. 3.—A female; mongrel bull-terrier; comparatively old; weighing about 17 or 18 lb., and having the following measurements:—

Length of head..... .... 6½ inches.
... body....................23 ...
Greatest girth of thorax..........18 ...
... abdomen..............12½ ...

Well covered with flesh, and in good condition; quiet, shy, crouching apart from other dogs; avoided food, shewed no vivacity, and made no great attempts to escape. (Nov. 19.)

---

[1] Measuring from the point of the muzzle to the occiput.

[2] ——— the occiput to the base of the tail.

Dog No. 4.—A male; mongrel Skye-terrier; middle-aged; weighing about 14 lb., and having the following measurements:—

Length of head	5	inches.
... body	20	...
Greatest girth of thorax	18	...
... abdomen	14¾	...
... head	12	...

In good condition; healthy, lively for a few days after admission (Nov. 19th); shy and timorous, taking little food.

No. 5.—A domestic cat of the ordinary kind; female; weighing nearly 4½ lb., and having the following measurements:—

Length of head	4	inches.
... body	14½	...
Girth of thorax	11½	...
... abdomen	12	...
... head	8½	...

Middle-aged; healthy, but in poor condition; lively and active, constantly attempting escape; immediately after admission (Oct. 24), fighting with dog No. 1. for its share of the viands.

Dogs 1, 2, and 3, and the cat, were stray animals, which frequented the public streets, and probably picked up the scanty means of a miserable existence among the ashes and other refuse of private houses, thrown out for the purpose of removal by the police carts. Dog No. 4 was parted with on account of its master not having the means to feed him; so that all these animals might be considered in a condition exactly analogous to that of the underfed, badly clothed, and worse-housed poor, exposed to similar vicissitudes and subject to similar privations.

Though first in order of time, want of space in the present number of the Journal compels me to postpone the experiments with the evacuations,[1] and merely to detail at present the results of the one with the fomites, *i.e.* the facts of the seizure of the four animals, and the *post-mortem* appearances seen in the two fatal cases formerly alluded to. The whole collateral conditions cannot, of course, be appreciated till the circumstances of the prior experiments have been mentioned, all the animals concerned having been fed on various of the cholera evacuations for greater or less periods. I believe, however, that the nature of their food had no further share in producing the disease in this experiment, than as a powerful predisposing cause; the continued use of fluid food, more or less putrid and sometimes irritant, may have induced a constitutional state highly favourable to the efficient action of the exciting cause, *i.e.* the exhalations from the fomites. This question, however, obviously admits of discussion. Suffice it, meanwhile, to say that, from the 26th October, dog No. 1 and the cat,—from the 7th November, dog No. 2,—and from the 19th November, dog No. 3, were more or less confined in the apartment already referred to, and fed on cholera evacuations,—up to the 21st November, when the experiment with the fomites was begun. As I have before stated, the animals were not constantly shut up; but the room was cleaned at intervals of a few days, and they were let loose to run about the hospital or in some of the empty wards thereof. Dog No. 1 chiefly frequented the kitchen or washing-house; but he was occasionally in the wards, while they contained cholera patients, though never for a longer period than a few minutes consecutively. In them he never had any opportunity of swallowing the evacuations, nor of lying among the clothes of patients; nor did he ever lie or sleep in any part of the wards. The cat, when set at liberty, usually betook herself to the kitchen fireside; but she was also frequently in the wards while tenanted by patients, and often lay in empty beds therein. These wards, which were naturally large and roomy, were well ventilated and fumigated:—they seldom contained at one time more than a single patient, whose clothes were always removed immediately on admission. Dogs 2 and 3 were never in any of the wards while tenanted; they were occasionally, however, allowed to run about in an empty ward.

---

[1] These will appear in the next number of the Journal, and will be accompanied by drawings of the microscopical appearances of the evacuations on which the animals were fed, as well as of the discharges of, and the pathological appearances in, these animals themselves.

Between the 24th October and 20th November there were admitted into hospital seven cholera cases, five being in deep collapse, and two passing into the consecutive fever; besides two cases of suspicious premonitory symptoms. Of these, four were fatal—two in collapse, and two in the consecutive fever (or perhaps this should more properly be called the typhoid stage of the cholera fever); the rest made good recoveries. On the blood and evacuations of these cases the animals had been fed from the respective periods of their admission.

On the 21st November, a woman, aged thirty-four, named Macaulay, from Chalmers' Close, High Street, was admitted in a state of collapse, passing into the consecutive fever. She had been seized three days previously in the following way: Having been out washing all day, she returned to her home in the evening much fatigued, swallowed hastily a full meal of potatoes, an article of food she was not much in the habit of using, and was suddenly attacked early the following morning (6 A.M.) with diarrhœa: this was followed by the ordinary symptoms of the disease, viz., severe serous purging, with vomiting and cramps. She had been very intemperate in her habits,—scantily fed and poorly clad,—in consequence of having pawned her clothing, she had been greatly exposed to cold and wet,—appeared to have sustained severe violence, as evidenced by several bruise marks on various parts of her person,—and was four months advanced in utero-gestation. She had had fever seven years previously. On admission (in collapse) she had complete suppression of urine, of two days duration, and was greatly debilitated. The consecutive fever was marked by great injection of the conjunctivæ,—continued, almost complete, suppression of urine,—involuntary evacuations in bed,—a great tendency to coldness of the whole body, and a low typhoid depression, attended by considerable stupor and muttering delirium. She died in the acmé of the fever-delirium on the 24th October. The principal features of the *post-mortem* examination were: absence of lividity of any part of the body, which was, however, very pale,—no marked dryness of the muscles nor viscera,—punctuate ecchymosis, to a slight degree, on the posterior surface of the heart,—absence of congestion or ecchymosis of the intestines, which were essentially healthy,—no "psor-enterie," nor undue prominence of Peyer's patches,—liver slightly and kidneys considerably fatty,—lower part of abdomen, up to near the level of the umbilicus, occupied by a gravid uterus. These negative characters of the pathology of cholera I have noticed in all the cases in this hospital which have been fatal during the consecutive fever.

Immediately on admission, her body-clothes, which consisted of a chemise, petticoat, gown, stockings, and neckerchief, were thrown in a heap into a corner of the room occupied by the dogs. They were comparatively new and clean, and had been given to her, I understand, by her sister immediately before she left her home to come to the hospital. The greater part of her own wearing apparel, as well as household furniture, she had pawned to supply herself with whisky. These clothes remained in the room for three days (from the 21st to 24th October), when the patient died; they were then removed and the bedding in which she had just died, soiled by her discharges, was substituted. This was left in the room for a night and then removed.

The effects of feeding the animals on the cholera evacuations up to the 24th was merely the production in most of them of a non-specific diarrhœa; their general health did not appear in any way deteriorated. But though the polluted atmosphere of the apartment, and the nature of their food, did not seem appreciably to affect the health of the dogs, it unfortunately happened that one of the hospital attendants,—a stout, healthy, middle-aged man, was, on several occasions, suddenly seized with nausea on accidentally remaining in the room for a few minutes, while feeding the animals, or otherwise,—nausea, followed by a general feeling of exhaustion sufficiently severe to render him unfit for work for the remainder of the day. On the 23d November, having been two or three times beside the dogs in the course of the morning and forenoon, he was suddenly seized with diarrhœa, while at dinner (1 P.M.), and within a few minutes he had three very fluid stools, which, however, were dark brown, fetid, and feculent. It was followed by considerable languor and nausea, and he felt much exhausted during the evening. This state continued next day; but being sent to bed, and various restoratives had recourse to, he felt comparatively well in the evening. He attributed his various seizures to the close and noxious atmosphere of the dogs' room; with what justice admits of considerable doubt. Still I consider myself bound to mention the fact, as with some it may have its value. At the same time, I must not omit also to state that, about a week previously, he had been at a Highland ball, after which he probably adjourned to a tavern. The effects of this jubilee undoubtedly had their weight as powerful predisposing causes of his subsequent slight attacks; at all events it was noticed that he had "never been quite well since."

On the 24th, at 6 P.M., one of the nurses of the hospital, who had been in attendance on Macaulay, was seized with sudden diarrhœa, attended with vomiting and nausea. The evacuations were copious, fluid, but dark-coloured, fetid and feculent. There was no prostration, nor any tendency to collapse; on the contrary, the pulse was good and the face flushed. Several days previously she had had an attack of diarrhœa of two days' duration. She attributed her seizure to the breath and exhalations from the body of Macaulay, with whom she had sat up all the preceding night. Such seizures, attributed to similar causes, I have already alluded to as not uncommon among our nurses. She recovered completely in two days.

On the 24th November the floor of the dogs' room was covered by a considerable quantity of urine, and by masses of viscid, feculent matter of a dark brownish-green or tarry colour. There was no discharge of mucus from any of the animals, and none of the fæces were very light in colour, nor fluid in consistence. Of course, the effects of the different matters given as food varied in different animals and at different times. Sometimes the diarrhœa was more severe, the evacuations more fluid, and the animals more or less prostrate and ill; but the average result was the evacuation of urine and fæces having the following characters:—

A specimen of the urine collected from the floor was acid; of a deep amber colour; without sediment; slightly phosphatic; non-albuminous; yielded a claret colour with heat and nitric acid, but no tinge of green or purple; contained abundance of urea, the nitrate crystallizing in beautiful, delicate, colourless plates; became very ammoniacal on standing, and effervesced strongly with nitric acid, after being slightly concentrated.

A portion of the dark viscid fæces, under the microscope, was found to consist of fragments of the epidermis and cellular tissue, and of the spines, hairs, spirals, and isolated cells of various vegetables,—chiefly the elements of broth, &c., which had been consumed by patients, and ejected in their vomits or contained in their stools; and having been thus given to the dogs, had passed through their system almost unchanged. I could trace the large oval cells of the potato,—the smaller cells of the carrot and turnip,—portions of the parenchyma and epidermis of the onion and cabbage,—the epidermis of barley grains,—and starch from bread, barley, &c. All these substances had been copiously supplied a few days before in the vomits of some collapse cases, when the cholera seizure had supervened on a surfeit of potatoes and herring, potatoes and beef, broth, &c. There were also a few entozoon-ova, of a dark brown colour, hairs, "annular bodies," and compound granular cells; the remainder of the field was filled up with molecular debris of a bright greenish-yellow tinge, a few phosphates and crystals of chloride of sodium. Water agitated with this tarry feculent matter, and filtered, had a rich port-wine colour, was slightly alkaline and albuminous, and had a peculiar mawkish disagreeable smell; the addition of nitric acid slightly decolorized it, but the subsequent application of heat converted the colour into a greenish yellow.

From the 21st to the 26th November the animals were fed chiefly on the reaction and fever stools, and the collapse vomit of Macaulay, as well as on the evacuations of previous patients, without any farther results than have been already described, viz., a continuance of semifluid but dark stools, with a copious discharge of urine. Macaulay's alvine discharges were not consumed for about a week; the vomit was swallowed immediately, chiefly, it appears, by dog No. 1 and the cat.

From the 24th November to the 7th December there were no cholera cases in hospital; nor, so far as I am aware, were there any, during the greater part of that period, in Edinburgh or Leith.

On the morning of the 22d, I found dogs 2 and 3 lying quietly and apparently comfortably among Macaulay's clothes; here they had probably slept all night. In the same position, I frequently saw them subsequently. Dog No. 1 and the cat were lying in different parts of the room, about a yard from the clothes. I never noticed either of them nestling among the clothes as did dogs 2 and 3. Dog 3 seemed very quiet; only rose when called to, and then appeared quite well. Dog 2 rose at once and tried to escape; it was trembling and timorous. Dog 1 and the cat were both quite lively. Little food had been swallowed by any of the animals.

The atmosphere of the apartment was so loaded with noxious effluvia, on the 25th November, that the animals were turned into a well-aired empty ward for the day, and their room was cleaned and ventilated. No food was meanwhile given, and they were replaced in the same room at night. They all seemed quite as healthy as usual. On the 26th, they were confined all day, and in the afternoon were given about ℥x of dark semiclotted blood taken from the heart and great vessels of Macaulay, at the *post-mortem* examination on the 25th; this was speedily consumed, the greater part of it probably by dog No. 1. They were all healthy and well, so far as I could judge by external appearances, until about 11·30 P.M., when dog No. 2 appears to have been seized with vomiting, purging, and cramps, uttering the most piteous cries and moans, frothing at the mouth, trembling from head to foot, shy, exhausted, and in extreme distress. Her cries continued at intervals for about an hour; but, attributing them to mere restlessness and wish to escape confinement, and occurring, as they did, at so late an hour (midnight), I did not visit the animal, and have consequently to regret exceedingly my not having witnessed the mode of her seizure and death.

Next morning (27th) I found her lying on the top of a barrel, over which her head hung stiffly; her limbs contracted to different degrees, and all quite rigid. On the floor was a small quantity of frothy mucous matter of a pale greenish-yellow tinge, which she had vomited, and a larger quantity of a greenish liquid, having an intermixture of greenish semifluid fæculent matter; its odour was exceedingly disagreeable, though not fœtid. Dog No. 3 was lying apart from the rest, crouched up in a corner of the room, quite apathetic and listless, making no attempt to escape, and scarcely looking up when spoken to; her eyes were heavy and suffused, and her whole appearance denoted great prostration. It was evident that she was dying, but there appeared to be little or no diarrhœal discharge round her; the evacuations were small in quantity, semifluid, fæculent-looking, and green; her tail and hips were not soiled in any way thereby. Dog No. 1 lay trembling in another part of the room, one side of his body, with the tail and hips, being much soiled with his evacuations, which were very fluid, but distinctly green, non-mucous, semifæculent, and mixed with a considerable quantity of urine; his eyes were suffused; he made no attempt to escape, though he rose tremulously when called to: he was evidently greatly debilitated and ill. The cat lay on the top of a cupboard, also quite languid and apathetic, making no attempt to escape; her fur moistened by her dejections, which were like those of the dogs, semifluid, green, and of a fæculent character. The floor was covered with the vomit, dejections, and urine of the animals; the alvine discharges were all very fluid, but at the same time, distinctly biliary in colour, *i.e.*, of a green tint;—there was a large quantity of urine intermixed. The stench emitted by these and by the bodies of the animals was insupportable; it was not the ordinary odour of any putrid animal matter; I know of no smell with which I could compare it. The animals were therefore again brought out, and the room cleaned and ventilated; it was also fumigated by chlorine evolved from chloride of lime by sulphuric acid. The cat made its way wearily and stealthily to the washing-house; its appearance was quite altered; she had apparently had severe vomiting and purging, attended by great prostration.[1] She lay crouching here the remainder of the day, apathetic and exhausted, but without any vomiting or purging so far as I could see. Dog No. 1 also made its way to the washing-house, to the front of the boiler furnace; there it had severe retching and vomiting, with occasional purging during the afternoon; his mouth was covered with frothy mucus of a light greenish-yellow tint; he seemed much debilitated and quite altered in appearance. With the retching was combined the appearance of an effort on the part of the animal to relieve himself of a feeling of choking, but I did not notice any marked laborious respiration in this or any of the other animals affected. Dog No. 3 retired to a dark coal cellar, where she lay immoveable as if dead, until removed again in the evening to the experiment room. The collapse here appeared extreme; but during the day I observed no vomiting nor purging, and she made no cries nor movements of any kind to lead to the belief that she was cramped. Shortly after being shut up for the night, there was a good deal of whining from dog No. 1, but this probably arose from his being tied up, which in him usually excited demonstrations of this nature.

---

[1] My attention was so engrossed by the dissection of dog No. 2. that I neglected examining the symptoms exhibited by the other animals during life so carefully as I would otherwise have done; on this account I did not note the state of the circulation or heat, &c., in any of them.

On the morning of the 28th, dog No. 3 was found lying quite stiff and dead in a corner of the room, in a pool of liquid greenish semi-fæculent matter, which had greatly soiled her right flank, tail, and hips; her limbs were contracted and rigid. Apparently she had partaken of little or none of the food supplied her; there was no appearance of any vomit, nor was the muzzle covered with frothy mucus. Dog No. 1 was again somewhat lively and greatly better; he had eaten a large quantity of the flesh, fat, and blood of dog No. 2, which died on the previous day. The cat was also considerably improved in appearance. There was no diarrhœal discharge from either animal. The cat passed solid dark fæces of tolerably firm consistence, in the course of the afternoon.

Two or three days subsequently, having no further materials for experiment, I let loose the surviving cat and dog, allowing them to run about the hospital and the grounds in front, and giving them abundance of nutritious food in a solid form. Both animals rapidly recovered, and at present (December 10) they appear to enjoy perfect health; the dog, in particular, which has swallowed an immense amount of cholera-blood and evacuations, is very lively and frolicsome—nay, he has even gained flesh, being considerably fatter and heavier than when first admitted.

Dogs Nos. 2 and 3 must have died between midnight and 6 A.M.,—probably about 4 A.M., which appears to be a very critical period in cholera, especially as regards the seizure. The *post-mortem* examination of dog No. 2 was performed at 10 A.M. on the 28th, about six or eight hours after death, and that of dog No. 3 at 1 P.M. on the 29th, about nine or ten hours afterwards; the viscera in the latter case were slightly warm when the body was opened in presence of Dr Begbie.

The following is a note, taken at the time, of the *post-mortem* appearances in dog No. 2.

Body exhales a strong, peculiar, indescribable odour, different, however, from that of the effluvia of the body of the human subject labouring under cholera. Considerable emaciation; limbs appear very fine, their bones and muscles prominent. Legs drawn up towards trunk. *Rigor mortis* well marked. Tail and hair around anus much soiled with the discharges, which, as collected on the floor of the room, are very fluid, of a yellowish-green colour, semi-fæculent, and have the same peculiar odour as the body. A *Tœnia* [*solium ?*] was found among the evacuations. On examining these more narrowly, the fluid portion is found to contain minute flocculi of mucus; is slightly albuminous and alkaline; yields a deeper greenish-yellow colour on the application of heat and nitric acid; collected in too small quantity to take the specific gravity; sediment contains a few entozoon-ova of a chocolate-brown colour, a few prismatic phosphates, one or two "annular bodies," and a mass of finely granular debris tinged greenish-yellow. On allowing the dejections to stand for a day, the quantity of phosphates becomes greatly increased. About the muzzle there is a quantity of frothy-mucousmatter of a light greenish-yellow tinge, corresponding in appearance with the vomit collected from the floor. It is acid; gives, with heat and nitric acid, a fugitive pinkish-red colour which gradually becomes greenish-yellow. Sediment, consists almost entirely of pavement epithelium; many of the scales are shrunk and atrophied, dark and granular; in the latter case they have a greenish-yellow tinge; if perfect and transparent they are colourless. Besides these there are a number of granular corpuscles nearly equal in size to and resembling pus; they have a light greenish-yellow tinge, and many of them, when acted on by acetic acid, exhibit round them a delicate cell-wall; they are rendered less granular, but there is no appearance of a distinct nucleus or nuclei simple or divided. Mucus scraped from the mouth contains healthy pavement epithelium. Eyelids half closed; corneæ glazed. Subcutaneous cellular tissue dry, crackling; fat, when removed, quickly becomes hard and dry like suet; muscles dry, but of natural colour.

On opening *thorax*, contents dry, collapsed.

Lungs considerably overlap pericardium; slightly emphysematous at their anterior margins; flaccid; of light red colour; crepitant throughout; float in water; somewhat congested posteriorly; on making a section dark blood exudes, along with thickened mucus. *Bronchi* quite healthy; weight of lungs 2¾ oz.

Mucus taken from the trachea contains normal cylindrical epithelium.

Juice scraped from the lungs contains a quantity of normal epithelium and fatty granules, and a number of hyaline globules.

Pericardium moist, but contains no appreciable amount of serum; no ecchymosis.

Heart weighs 1¼ oz., natural in size; cavities, valves, and orifices healthy; organ contains little blood. Left ventricle firmly contracted; no clots in either side of heart; muscular substance firm, of a dull red colour; normal in structure; no ecchymosis. Aorta healthy; no congestion nor ecchymosis.

Blood from great vessels at base of heart, dark, fluid; scarcely any appearance of coagulation. On allowing it to stand in a urine glass, surface becomes bright arterial red, lower stratum continues dark and grumous; the whole mass coagulates feebly into a tremulous jelly; corpuscles normal.

Pharynx, *œsophagus*, and *trachea* healthy; no increased vascularity nor ecchymosis.

On opening *abdomen* there is a very fœtid smell; viscera dry and greatly collapsed. No marked congestion of the peritoneal coat of the intestines, which feel as if covered with mucilage, thickened, and as if containing very viscid glutinous matter. The *omentum* is shrunk into a few bands of the most delicate areolar tissue, containing in its meshes very little fat. The *mesentery* is quite dry and transparent, resembling silk or tissue paper; its veins full of dark blood. *Stomach* contracted; feels, like the intestines, as if much thickened, and having very viscid contents. Rugæ very thick and prominent, of a dark reddish-purple; whole mucous membrane much congested, copiously lined, like the œsophagus, with very viscid stringy mucus, which, however, does not appear as distinct flocculi. Congestion greatest towards pylorus.

In the stomach and intestines are found about three or four ounces of a very turbid opaque fluid, of a dirty greenish-yellow colour, having a very disagreeable mawkish odour; sediment, which occupies about two-thirds of its bulk, is finely granular and of same colour as fluid portion; resembles very fine pease-meal and water; when filtered, is alkaline and albuminous; yields a deeper greenish-yellow tint, with heat and nitric acid. In the mucus scraped from the stomach is found a quantity of cylindrical and pavement epithelium very dark and granular.

Œsophagus healthy in appearance; considerable increase of its mucous lining; no congestion nor ecchymosis of its plicæ or their interspaces; no softening of the mucous membrane. On scraping some of the mucus from the surface of the gullet, and submitting it to microscopic examination, the epithelium is granular; contains several large "gonidic bodies."[1]

The *small intestines* are copiously lined with a thick, viscid, flocculent mucus, of a very pale greenish-yellow colour,—when this is washed out by means of water, it has precisely the appearance of the "rice-water" stools of cholera, gradually separating into a supernatant liquid, which is whitish, opaque, turbid, whey-like in colour, but somewhat thicker in consistence; and a sediment wholly composed of whitish, opaque, mucous flocculi. The mucous membrane is much thickened, and when the gut is placed under water, it assumes a beautiful velvety or villose appearance. Here and there, where the mucous coating is thinner, punctuate ecchymosis, with general congestion of the intestine to a slight degree, is observed. But if this thick viscid mucus is removed by washing, the ecchymosis is found to be general, the whole length of the intestine being copiously studded with minute dark points. It appears as if almost all the follicles of the various intestinal glands had become the seat of hemorrhagic extravasation. Peyer's patches are not exempt; they are very distinct, though not perhaps unduly prominent; their margins are thickened, but are not marked by any increase of the general congestion or ecchymosis. There is no marked enlargement of the solitary glands at the lower part of the ilium, but the mucous membrane is greatly thickened, rough, and villous, and the openings of the Lieberkuhnian follicles are very distinct. The increased vascularity is greater in some parts of the intestine than others; it is more a blush of a bright arterial hue than arborescent. The intestinal villi are quite nude.

The same flocculent mucus lines the interior of the *large intestines*, which, however, contains in addition a small quantity of a fluid, yellowish-green, slightly fœtid matter, having, to the eye and under the microscope, the usual characters of fæces; it resembles the evacuations collected on the floor of the room in which the animal died. The ecchymosis is as general and has the same appearance as in the small intestines; the increased vascularity is much better marked. Lower part of *colon* and the whole of the *rectum* are very rugose, in this respect resembling the appearance of the stomach; intensely congested, of a dark purple colour, and thickly covered with a bloody, prune-juice-like viscid mucus. The mucous membrane is much softened and very velvety, but it is much less thickened than in the small intestines. A *lumbricus* was found among the fluid washed from the intestines. On macerating the large intestines, so as to get rid of their mucous coating, the solitary glands are found to be much enlarged and very prominent, especially in the rectum; the surface of the mucous membrane appears as if studded over with pearly sago-grains.

---

[1] I call these provisionally "gonidic," from their resemblance to the gonidia of the lichens. As generally seen, they appear quite globular, usually larger than pus corpuscles; have a distinct wall, colourless and transparent; frequently a distinct central nucleus, also colourless, round which are aggregated a number of rounded granules of a bright greenish-yellow or orange colour, resembling the chlorophylle grains in the cells of plants. These bodies have occurred in greater or less abundance in the evacuations of all the cholera patients under my charge; their precise source I have not had time to ascertain. They pass through the digestive apparatus both of man and the dog, apparently without change. Drawings of their microscopic appearances will appear with the continuation of this paper in the following number of the Journal.

The whole intestines, including the œsophagus and stomach, weigh 9¾ oz.

The ricy matter washed from the intestines has a peculiar mawkish smell,—similar to that exhaled by the body of the animal; specific gravity is very low, ranging about 1010,—alkaline—highly albuminous,—gives a greenish-yellow colour, with heat and nitric acid. It is more mucous and viscid, and much more albuminous, than the "rice-water" stools in man generally are. The flocculi consist of an almost transparent, finely striated, colourless basis, covered by innumerable corpuscles, which are granular, and vary in size from about half to equal that of pus corpuscles, the greater number being somewhat smaller than pus. On the addition of acetic acid, or after boiling with that re-agent, the granularity and haziness of some of these bodies seem in no way affected, while surrounding or attached to them, a delicate cell-wall may be observed; in others, the granules become fewer, darker and larger, and no cell-wall can be detected: while in others, again, a distinct nucleus or nuclei—single or divided, may be rendered visible, and the granular matter partially or completely dissolved. I would avoid giving any decided opinion as to the nature of these bodies; the former may be epithelial or other nuclei, the latter, mucus corpuscles: but I confess my inability at once, under any circumstances, and without difficulty, by microscopic examination alone, to distinguish mucus, pus, lymph, and white blood corpuscles and epithelial nuclei from each other. These corpuscles, though similar in appearance to those found in the flocculi of the "rice-water" stools in man, are much larger, more distinct, and more numerous; this is probably owing to the much earlier period, subsequent to death, at which they were examined. The mucous bands and the corpuscles are rendered much more distinct by acetic acid or tincture of iodine.[1] No intermixture of the debris of food can be noticed.

The yellowish-green fluid taken from the large intestines, and filtered, is highly albuminous, alkaline, and gives a faint and dull brownish-red tinge with heat and nitric acid.

The water, in which the intestines were subsequently macerated for two days (28th and 29th November), likewise presented the appearance of ricy stools, but had a bloody tinge—it was alkaline, and gave a faint greenish-yellow colour with heat and nitric acid,—densely albuminous, the whole coagulable matter falling as a sediment, and carrying with it the greenish-yellow colouring matter produced by heat and nitric acid, leaving the supernatant fluid quite colourless.

Liver weighs 4 oz., of a dark purple colour, much congested with dark fluid blood; otherwise healthy.

Gall-bladder moderately distended, with deep yellowish-green bile, which is quite healthy in appearance. When the bile is diluted considerably, heated, and a few drops of nitric acid added to it, a dense turbidity is immediately produced, along with a deep bluish-green colour, which quickly passes through purple and pink into brownish-yellow. The biliary sediment, which is scarcely appreciable, consists almost entirely of minute dark globules and granules, resembling oily matter. In the mucus scraped from the gall-bladder, there is found a large quantity of the same dark granular matter, and a few "gonidic" bodies.

Kidneys weigh 1 oz.—firm and congested with dark blood; epithelium fatty (normal state ?); capsule tears off readily; surface smooth; Malpighian bodies and tubuli healthy.

Spleen weighs ¾ oz.—flaccid; dark-coloured and congested; otherwise healthy.

Pancreas healthy.

Urinary bladder weighs ¼ oz.—moderately distended, containing about ʒii–iii of urine, healthy in appearance. On opening the organ and drawing off its contents, its fundus is found to be much congested, and covered with a bloody mucus; this was not removed even after three days' steeping in water. On immersion in water, the bladder becomes thick, corrugated, and contracted, and its interior rough and rugose. Its epithelium is hazy, but healthy.

---

[1] In examining the flocculi of the "rice-water" stools or similar substances, which are exceedingly delicate and almost transparent, I have been in the habit of using iodine, which by communicating to mucus, epithelial cells, &c., a deep brownish-yellow colour, renders them much more easily visible. Bile or any similar dark pigment would answer equally well.

Urine contained in bladder, acid; comparatively clear; non-ammoniacal to the smell; non-albuminous; on heating it and adding nitric acid, an intense dirty green colour is produced, which passes slowly through deep purple into brown. On standing a night after this reaction, continued of a rich chocolate-brown colour, having a scanty granular sediment. The same urine, tested two days afterwards, was non-albuminous, and with heat and nitric acid gave a deep grass-green colour, passing through blue and purple into deep blood red; the latter tint was not destroyed on the addition of a large quantity of acid. On boiling down the urine, to test for urea, it rapidly becomes as dark green as the bile, and exhales a very peculiar and offensive odour, resembling that of putrid fish. On adding nitric acid, great effervescence is produced, and the mass becomes of a dark greenish-brown colour; while in thin layers, on the sides of the evaporating basin, it exhibits various shades of purple, pink, and red. It contains abundance of urea, the nitrate of which, under the microscope, occurs in foliaceous masses of a dark greenish-brown colour, which can be partially removed from it by repeated washing; it also contains a quantity of large prismatic phosphates, and, in smaller amount, dark granular urates.

Uterus and appendages, *per incuriam*, not examined.

Large abdominal veins full of dark, fluid blood; little or no blood in aorta.

Brain and its membranes—skull and scalp healthy; no marked congestion nor venous engorgement in any; ventricles moist, but contain no appreciable amount of serum; choroid plexus congested; cerebral substance firm, normal, no increase of its "puncta vasculosa;" the internal ganglia and the cerebellum normal. [Brain examined 60 hours after rest of body.]

The weight of the body after evisceration is 4 lb.

The following were the *post-mortem* appearances noted, on the 28th November, in dog No. 3.

Exhales a most nauseous odour; *rigor mortis* well marked; eye-lids half closed; corneæ hazy; muzzle firmly closed; stout muscular animal; muscles well developed, dull in colour, very dry; temporal muscles, when removed, appear as rigid as dried ham, and can readily be cut into thin slices; subcutaneous cellular tissue, and fat and cellular tissue of thorax and abdomen also very dry.

On opening *thorax*, viscera are dry and collapsed; cartilages of the ribs ossified.

Lungs weigh 6 oz.; slightly overlap pericardium; of a light reddish colour, covered with bluish-black punctuate mottling; crepitant throughout; congested to a slight degree posteriorly; on being cut into dark blood escapes from numerous points; otherwise healthy.

Bronchi normal; no congestion nor increase of mucus.

Trachea pale; quite healthy.

Pericardium merely moistened with serum; no ecchymosis.

Heart weighs 3¼ oz.; normal in all its walls, cavities, orifices, and valves. Both ventricles contain dark, semi-clotted, but comparatively fluid blood, which also fills the auricles and great vessels. No coagula of any kind. Left ventricle firmly contracted; right very flaccid; muscular substance firm, dull in tint; heart appears dilated, especially at its base, probably on account of the quantity of blood contained in the ventricles, no ecchymosis posteriorly; muscular substance normal; aorta quite healthy.

Œsophagus healthy; mucous membrane pale; plicæ distinct, covered with very thick viscid but normal mucus.

On opening *abdomen*, viscera very dry and collapsed; *mesentery* resembles a diaphanous membrane like tissue-paper beautifully ornamented by arborescent lacteals and veins, the latter gorged with dark blood; the *omentum* has the appearance of a lax network of the most delicate silky fibres, covering the intestines, and containing in its areolæ little or no fat. The fat accumulated around the kidneys and other viscera, when removed from the body, becomes whitish and hard like suet. Peritoneal surface of the intestines feels gluey, but there is no distinct exudation of viscid mucus as there often is in man. Intestines feel doughy and thickened, as if containing pultaceous matter; the surface of the small intestines is marked by a light red blush of general increased vascularity; thatof the large has somewhat of a slaty colour.

Stomach distended, much thickened throughout, but especially at pyloric extremity; interiorly, its rugæ are very prominent and thick, whole mucous membrane softened and pulpy; highly congested, having a uniform dark, bloody, purple tinge, which is deepest towards the pylorus; a small portion of the mucous membrane of the *cul de sac* at the cardiac extremity, along the greater curvature, comparatively smooth and thin. In the interspaces of the rugæ there is found a dark brownish-green frothy fluid, intermixed with a quantity of minute curdy flocculi. From the stomach was squeezed, before it was opened, about ℥ij of a greenish chocolate-coloured fluid, having very little mucus intermixed. When filtered it is found to be slightly alkaline; has little or no odour; highly albuminous; yields with heat and nitric acid a brownish-red colour, which gradually becomes greenish-yellow. Mucus scraped from stomach contains a few "gonidic" bodies. The whole mucous membrane is soft and velvety, and copiously covered by very viscid, ropy mucus.

From the *large intestines* there was squeezed about ℥i, and from the small about ℥ii of a thick, viscid, tenacious mucous matter of a deep brownish-green tinge, containing a scanty admixture of ordinary fæculent debris. This matter exactly resembles, in appearance and consistence, soft soap mixed with a little water; it is found adhering firmly, as a thin layer, to the whole extent of the canal, and is with difficulty washed off. It has an exceedingly nauseous, though not a fæculent odour, resembling that of the body of the animal; agitated with water and filtered, it yields a very turbid fluid of a brownish yellowish-green colour, which is slightly alkaline, highly albuminous, and gives with heat and acid an orange-red or brownish-red colour, which ultimately becomes greenish-yellow. Under the microscope the viscid matter is found to consist essentially of delicate striated mucus, entangling a number of granular corpuscles, similar to those mentioned as occurring in the mucous flocculi found in the intestines of dog No. 2, along with a few fragments of vegetable tissues, "annular" and "gonidic" bodies, phosphates in irregular crystals, and a quantity of molecular debris of a bright greenish-yellow colour.

When this viscid muco-fæculent matter is gently removed, the subjacent mucous membrane is found greatly thickened, softened, and velvety; it is copiously lined with a coating of thick whitish or pale brownish-yellow mucus, which, on being scraped off and mixed with water, forms the same rice-watery matter as in dog No. I. It appears as if the mucous membrane had undergone a gelatinous degeneration, there being no line of demarcation between it and its thick covering of gelatinous matter, the one seeming to pass insensibly into the other. This mucus adheres so tenaciously that it is with difficulty removed, even after a lengthened maceration; it does not extend over a number of Peyer's patches, especially in the upper part of the gut, and these consequently appear as if deeply sunk in the mucous membrane, their margins being very thick, puffy, and much congested. There is marked congestion of a bright red hue throughout the canal, but no ecchymosis; it occurs in patches, and is very irregularly distributed.

The *duodenum*, at its commencement, feels much thickened, this thickening being continuous with that of the pyloric extremity of the stomach; the hypertrophy seems seated chiefly in the mucous membrane; it is also somewhat distended; its interior is marked by several patches of deep congestion. This congestion appears only to a slight degree externally, in the form of slightly increased arborescent vascularity of a vivid red hue.

In the lower part of the *ilium*, and throughout the great intestine, the vascularity is much increased, the mucous membrane and its mucous lining having a uniform bloody tinge; the plicæ of the interior of the large intestine appear as dark bloody ridges. The lower part of the *colon* and the whole of the *rectum*, besides being covered with an abundant bloody mucus, has a slaty colour, and the whole appearances of this portion of the intestine exactly resemble those seen in some cases of dysentery. The glands are quite obscured by the thick mucous coating. On being macerated for a few days, the blood and mucus were both gradually washed out, and the surface of the gut was then found studded over with pearl-like grains of sago, *i.e.*, the enlarged solitary glands, as in dog No. 2.

After maceration, Peyer's glands also were rendered much more distinct; their apparently eroded or excavated base and thick congested margins rendering them very prominent.

The whole intestines, including the stomach and part of the œsophagus, weigh 1 lb. 5½ oz.

The water in which the intestines were macerated exactly resembled "rice-water" stools, except that it had a bloody tinge; it was very turbid,—alkaline, albuminous,—of specific gravity, about 1005 to 1010, and gave a greenish-yellow colour with nitric acid and heat. The flocculi of the sediment had the same microscopical appearance as those of the "rice-water" stools in man, viz., a delicate striated hyaline mucus, with a greater or less number of granular corpuscles, molecules, and granules, which were colourless, or had a very faint greenish-yellow tint; there were in addition a number of "gonidic" bodies and epithelium scales, and a quantity of dark granular pigment.

The intestinal villi are all nude.

Mucus scraped from any part of the gut is quite pure, unmixed with debris of food or colouring matter of any kind. It appears as a striated hyaline substance, so delicate and transparent that it can scarcely be seen in the field of the microscope. There are few or no granular corpuscles intermixed with it.

Liver weighs 11 oz., large, dark coloured, congested; apparently normal. Hepatic cells fatty to a slight degree. *Gall-*

bladder moderately distended with bile of a dark green colour, which is normal also in consistence. When diluted, it gives the usual reaction with heat and nitric acid, the purple, pink, and red tints being very rich and deep. Biliary sediment, which is not appreciable, contains a large quantity of granular matter and oily globules. The mucus scraped from the interior of the gall-bladder contains a quantity of cylindrical epithelium, isolated or in patches, united by a membrane along the apices of the individual cells; there is also some pavement epithelium and free nuclei, all having a bright greenish-yellow tinge.

Kidneys weigh 3¼ oz., are large, firm, darkly congested; apparently healthy; surface quite smooth; capsule strips easily; epithelium very fatty.

Spleen weighs 1¼ oz., flaccid, dark coloured; structure normal.

Urinary bladder greatly thickened and contracted, plicated or rugose, both externally and interiorly. Its exterior is marked laterally by very tortuous veins filled with dark blood. Contains only about half a drachm of very turbid, but non-ammoniacal urine. Interior reticulated, pale; no congestion nor ecchymosis. Interior of urethra much congested, especially at its commencement. Bladder weighs ¾ oz.

Urine is mixed with a granulo-flocculent whitish matter in small quantity; is of a deep amber colour, acid, non-albuminous. With heat and nitric acid it gives the same reaction as the bile, *i.e.*, if one or two drops of acid be added to the heated urine, a zone of a bluish-green colour is produced at the bottom of the fluid; if three or four drops are added, an orange-red zone is developed at the bottom, with blue and green zones immediately superjacent; by agitating, while warm, the whole fluid acquires a deep grass-green colour, which rapidly passes through bluish-green, blue, purple, and orange-red, into a light chocolate tint, which is permanent. It contains a considerable amount of urea. The sediment contains a quantity of minute oil-globules, aggregate or isolated,—exactly similar to those found in the bile.

Uterus is comparatively free from congestion or ecchymosis externally; but vagina is congested internally, and towards its outlet has a somewhat slaty colour. There is also general congestion of the interior of the Fallopian tubes, each of which also contains three patches of punctuate ecchymosis, with surrounding slaty discoloration. Weight of uterus and appendages one ounce. Epithelium from the interior of the uterus, Fallopian tubes, and vagina, healthy.

Brain and its membranes, with the scalp and skull, appear quite healthy. The subcutaneous cellular tissue, and all the muscles of the head, are particularly dry. No marked venous oozing from the scalp nor bone; no marked engorgement of the sinuses of the dura mater, nor of the veins of the cerebral convolutions. On slicing the cerebral matter, the "puncta vasculosa" are normal. Ventricles are moist; small quantity of serum (about ʒss) in the anterior cornu of each; choroid plexus not congested. Internal ganglia and cerebellum healthy. Cerebral substance comparatively firm,—gray matter quite distinct.

Weight of body after evisceration, 14 lb.

The large veins of the abdomen are turgid,—their contents fluid, but not extremely so, and dark.

The *blood* corpuscles are normal. About ℥iii of blood from the heart and great vessels, on standing for a day, exhibited no distinct coagulation, but formed a delicate tremulous jelly; it was of a bright arterial hue on the surface,—dark below.[1]

(To be continued.)

CRICHTON ROYAL INSTITUTION,
DUMFRIES, *March* 1854.

ART. V.—*Suggestions in regard to the performance of Post-mortem Examinations.* By W. T. GAIRDNER, M.D., one of the Ordinary Physicians of the Royal Infirmary of Edinburgh, and lately Superintendent of Morbid Anatomy in that Institution.

The Editor of this Journal has done me the honour to say, that in his opinion a short and clear statement of the methods pursued for some years past in the Royal Infirmary, of making investigations in Morbid Anatomy, and of recording and preserving for reference the facts observed, would be useful to the profession, and suitable for publication in this place. Under these circumstances, I shall be happy if I can contribute in any degree to the advancement of a study which, though everywhere highly appreciated, is, from obvious causes, too little pursued in the field of general practice.

The numerous obstacles to the performance of *post-mortem* examinations, which prevent these from becoming, as they ought undoubtedly to be, quite general, arise, partly from prejudice on the part of the public, and partly from carelessness or imperfect information with regard to the results of such investigations, both on the part of individual medical men and of the public. It may be admitted, also, that among the dwellings of the poor there are often real reasons of great cogency for avoiding the inconvenience caused by even the most careful performance of this duty, and that these may in some cases amount to an insuperable obstacle. Were the attention of the country at large, however, alive to the fact that the scrutiny of the bodies of the dead is their only real security against professional and extra-professional ignorance and quackery, they would most surely take another view of this subject than a sentimental one; and instead of making difficulties, and rewarding with dislike or faint approval the efforts of conscientious practitioners to overcome their scruples of feeling, they would join cordially in establishing, as a principle, that the performance of *post-mortem* examinations is an essential part of the duty of the medical man; so essential, that if the profession were willing to neglect it, it would be more than ever the interest of the public to compel its performance.

---

[1] I shall be glad to be favoured by any of the readers of this Journal, or others interested in the subject, with information regarding, or cases illustrative of, the natural influence of cholera on the lower animals, or the results of any experiments, which have been made on the artificial communicability of the disease, during the present or any previous epidemic, in this or other countries.

If to any one this statement appear too strongly worded, I would simply say that I believe three months' experience of the routine duties of the morbid anatomist in an hospital, and the conscientious performance of the first dozen of *post-mortem* examinations which arise out of any private practice, will speedily convince him that he is mistaken; and that no man can be said to be even moderately well acquainted with the resources, or duly impressed with the difficulties and imperfections, of the medical art, who does not habitually face this ordeal, in cases apparently of the simplest and clearest description, as well as in those of difficulty and doubt. Indeed, however useful may be the inspection of the parts traversed by an operation, or disorganized by a known disease, it would not be difficult to shew that practical results of at least equal importance spring from the consideration of the morbid changes, which escape observation or are misinterpreted during life. It is these which have taught us all we know of the arrest and cure of organic diseases. It is these which, amid the stern fatalities and disappointing accidents of practice, exhibit the healing powers of nature, and inspire confidence in her resources. It is these alone, which can lighten the gloom thrown by modern diagnostic refinements over the future, when the unfortunate proprietor of a bellows murmur, or of a urinary curiosity, appeals to us against the verdict of his own despair, too often backed by that of an incautious or half-informed physician. And it is from the careful and close observation of the innumerable minor lesions which leave their traces in the dead body, that the hospital physician in the present day acquires that invaluable experience of the *beginnings* of disease, which can alone render speculation legitimate, reasoning sure, or observation fruitful.

If anything were wanting to prove the necessity of a careful and *continuous* attention to morbid anatomy in private practice, it would be the lamentable errors of those who, diverting it from its legitimate uses, have viewed it only as the prop of preconceived opinions, or the key to previously ascertained difficulties. How many suggestions in practice, how many baseless theories, have owed their influence to the presumed observation of congestion of the brain, a condition which, as one of actual disease, may possibly occur two or three times in a thousand cases! How many patients have been bled in Parisian hospitals because Broussais led himself and others to believe that the ordinary states of the intestines and lungs after death were the manifestations of fatal disease! And yet half-a-dozen dissections carefully, honestly, and thoroughly performed, and compared with facts observed during life, would be sufficient to save any man of common accuracy from these and other errors which are every day committed in courts of justice, medical periodicals, systematic treatises, and even in hospital practice. On the other hand, a much greater number of observations confined to points about which curiosity has been excited, will fail to dispel errors, nay, may even create errors about these very points.[1]

It would be inconsistent with the object of this paper to point in detail to numerous facts which occur to me as illustrating these remarks; nor would the mention of a few cases of erroneous diagnosis, or of defective pathology, suffice for my purpose, which is to shew that the habitual and complete investigation of fatal cases of disease, and not the mere satisfaction of an accidentally excited curiosity, ought to be the object of the physician in *post-mortem* examinations.

---

[1] French pathology is full of these errors; the works of Laennec, Louis, and a few others, being, however, great exceptions. The admirable dissertation of Dr Abercrombie on apoplexy, and that of Dr John Reid on the state of the brain in fever, are directed against similar errors which received extensive currency in this country. In Germany, on the other hand, few serious blunders of this kind receive support, as every one is systematically educated in morbid anatomy.

By nothing less than this can he be placed in a position of superiority to the errors and fallacies of his age, which is emphatically the age of morbid anatomy. The education of his own mind, and the duty he owes to his patients, should combine to require of him a habitual attention to this subject; and the public should be in all cases impressed with the serious responsibility incurred by those who place their own feelings and prejudices between the medical man and his performance of a disagreeable though necessary duty. There can be little doubt that in some parts of this country the safety of human life has been extensively compromised by inattention to the investigation of causes of death, and in all places where the right of making this investigation is habitually denied, quackery and ignorance have become rampant, and the position of the medical practitioner has been degraded almost to the level of that of the extra-professional quack. The wholesale poisonings which have occurred in some villages in Essex, and the melancholy revelations of public ignorance which have transpired through the trials of Coffinites, and other tolerated dealers in destruction, in the northern counties of England, supply proofs of a state of matters of which we have happily little experience in this neighbourhood.

Before leaving this subject I cannot refrain from expressing my conviction that the governors of our public charities have in some cases adopted, from motives of mistaken benevolence, regulations calculated to obstruct the performance of this medical duty, and to foster, instead of discouraging, those prejudices and objections on the part of the public which it is so desirable and even necessary to remove. The treatment of disease in our great hospitals involves a quite peculiar relation between the medical profession and the public. The former is intrusted with a serious responsibility, and performs large services on behalf of science and humanity, usually without any direct reward, except what results from the accumulation of experience and knowledge under favourable circumstances. To the zealous and careful execution of these services it is indispensable that every encouragement should be afforded. Hence it has been found that the admission of students to our hospitals is eminently favourable to the interests at once of the sick poor and of medical science, by insuring that the duties of the medical officers are performed under watchful eyes, while, at the same time, the treatment of disease is made to subserve the double object of charitable relief and medical instruction. But the benefits of this publicity are incalculably diminished, and the education of many minds is thwarted in every instance in which a case carefully studied during life fails to be examined after death. False impressions remain uncorrected, difficult problems unsolved; the energies both of teacher and student receive a shock, and observation is discouraged for the future in no ordinary degree. It becomes, therefore, a serious question whether the easy and credulous admission of sentimental objections to *post-mortem* examinations ought to form part of the system of our hospitals, and whether the medical officers of such institutions ought to be subjected to the indignity of encountering such objections, urged, as they frequently are, by persons quite inaccessible to moral considerations, and in some cases actuated by very mercenary motives.[1] I am fully persuaded that the most tender and delicate consideration for the real wants and distresses of humanity is compatible with a somewhat stern determination to overrule these excesses of feeling and prejudice, which increase the more they are indulged, and are not less detrimental to the character of those who give way to them, than they are injurious to the best interests of society.

The erection of houses of reception for the bodies of the dead before interment, would be, in our great cities, a sanitary measure of the utmost importance, and might, at the same time, be made greatly to subserve the ends of medical science, by enabling *post-mortem* examinations to be conducted without the annoyances which they inevitably occasion, especially in the houses of the poor, as at present performed. The suggestions of the sanitary commissioners upon this subject appear to be on every ground worthy of warm support from the medical profession.

Instruments required in making post-mortem *examinations.*—In private houses investigations in morbid anatomy are necessarily carried on with more limited objects, and fewer implements than in hospitals. Indeed, the fewer and simpler the instruments used, the more work will generally be performed in the end. It is, therefore, of importance to consider well what is indispensable, and what is merely an occasional or a superfluous article of convenience, to be kept in the armamentarium for use when required, but not to form part of the ordinary or minor apparatus. It is of extreme importance that the practitioner should possess this in a portable form, packed into a case where everything has its proper place, otherwise he will be subjected to much trouble and annoyance by the attempt to remember everything he wants at a given moment, and will not unfrequently discover an omission when it is too late.

---

[1] I have repeatedly had to take an unwilling consent, or a positive refusal, from the lips of intoxicated "relatives." I have many times been roundly abused, and have even been stigmatized as a "butcher" or a "ruffian" in presence of my patients, and in not a few cases have been pursued by offers of consent for a "consideration." Lately, this happened to me in four or five cases successively.

The ordinary cases of instruments sold by cutlers for *post-mortem* examinations are extremely ill adapted for the purpose. The knives are much too small for rough work, with the exception of the cartilage knife, which, from its form, is useless for dissection. The saw, short in the blade and cramped in the handle, is a most disagreeable and difficult instrument to work with; the scissors likewise are too small; and for these disadvantages there is no compensating advantage, as the box, with all its compression and packing, is a clumsy thing after all to carry in the pocket. It is better to give up at once the idea of a pocket-case, and with it all those contrivances for torturing instruments into small bulk by means of adaptations, which sacrifice their efficiency, as when the saw and the cartilage knife are fitted into the same handle by a moveable joint. Every cutting instrument which is necessary in most dissections, may be fitted into a box or a leather case not more than 12 inches long by 3¾ broad, and 1⅝ thick, and this can easily be carried in the hand. Cases of instruments of this kind are now made, at my recommendation, by Mr Archibald Young, cutler, 79 Princes Street.

The following instruments may be considered as belonging to the indispensable class, except where otherwise mentioned.

1. *Knives.*—These should in all cases be *fixed* in their handles, and, as a general rule, should be considerably larger and stronger both in handle and blade than those commonly employed in dissection. The one I use for opening the cavities, removing the organs, and in some cases, even for finer work, is about 2½ to 3 inches long in the blade, which is ⅜ths to ½ an inch in breadth, and rather less than $\frac{1}{12}$th of an inch thick at the back. The handle should be about 4 inches long, and so thick and strong as to give a firm hold to the entire hand of the operator. The blade should be in form like that of a well-made scalpel, neither too round nor too straight in the edge, which should be ground rather with a view to power than to keenness or delicacy in cutting. With two knives of this kind, and a smaller scalpel or bistoury for finer work, the practitioner will find himself sufficiently provided under all ordinary circumstances, and a cartilage knife is thus rendered quite unnecessary. In hospitals, or where many dissections follow one another in suc-

cession, more knives must of course be provided, as the rough work of a single case is often sufficient to destroy the edge of two knives, and I have found six or even eight not too many for a heavy day's work. In the country it will always be well to have two or three *sets* of knives, only one of which, of course, need be in the case at one time.

2. *Saw.*—A common amputating saw is the best; but it is not necessary (except perhaps for opening the spine) that the blade should be so long, or the handle so large. Sufficient power will be obtained by an instrument 11 inches in length, and with a blade about 8 inches long, the handle being as nearly as possible in a line with the blade, so as not to add additional breadth to the instrument. In ordinary dissections all bones or bony cartilages can be divided with the saw, but in hospitals, or in all cases where the spine is to be opened, or when the base of the skull, ear, orbit, &c., are the subject of operation, it is convenient to employ,

3. *Bone-pliers* of the kind used by surgeons. They need not, however, be included in the minor apparatus, as they add much bulk to the instrument case, without corresponding advantage.

4. *Scissors.*—The most convenient and useful are about 6 inches long, being 2¼ inches from the joint to the point of the blades, which are very strong, and only very slightly curved at the back. One of the blades should be sharp at the point, the other blunted. A smaller pair of scissors may be used for fine dissection, but are not commonly required. By placing a small cork upon one of the points the common scissors may be made to serve the purpose of

5. *Enterotome Scissors.*—In these both the blades are blunt-pointed, and one of them is prolonged into the form of a flat blunt hook, a construction which allows of the intestines being opened with great facility and rapidity, but which is not necessary to the *apparatus minor.*

6. *Forceps*, as in ordinary dissecting cases.

7. *Probe-pointed curved bistoury*, useful in incising the cerebral meninges, the bronchi, &c., but not necessary in the *apparatus minor.*

8. *Slicing knives*, thin and broad in the blades, which should be from 6 to 10 inches in length, and from ¾ to 1¼ inch broad. A small slicing knife is very useful, but scarcely indispensable for the *apparatus minor.*

9. *Cross-headed chisel*, 4 to 4½ inches long, 3 to 3½ broad.

10. *Mallet* and *blocks*, to support the head, should always be in the anatomical theatre of an hospital.

11. *Needle* should be upwards of 3 inches long, and strong

in proportion, well sharpened, slightly bent towards the point, and with a large broad eye, sufficient to admit a strong packthread.

12. *Charriere's Rachitome* is a favourite instrument with some for opening the spine. I have never used it, and it need form no part of the minor apparatus.

13. *Grooved Director.*

14. *Blowpipe.*

15. *Probes.*—It is well to have at least two, and one of them should be rather fine. A few bristles may also be useful, and do not take up much room.

16. *Catheter.*—Very necessary in some cases, but usually not required.

17. *Pipette and Measure-Glasses.*—In certain cases almost indispensable, but inconvenient to carry, and commonly not required. The pipette should have a large bulb in the centre of the stem.

18. *Graduated Inch-Rule* and *Measuring Tape.*—The former should be divided decimally, and also in parts of $\frac{1}{50}$th of an inch. The latter should be at least 7 feet in length, and well varnished or waxed. As the measuring tape is soon dirtied and worn, it should be of the commonest and simplest kind.

19. *Callipers.*—A small pair, capable of taking inside and outside measurements of one or two inches, and so well made as to be good for the finest divisions of the inch-rule, may be ranked among the indispensable apparatus. I am in the habit also of using, both on the dead and the living, a large pair of callipers for external measurements. One of the limbs is fixed, the other slides upon a graduated foot-rule, which tells accurately the distance of the points from each other. A third limb is placed between the other two, and consists of a straight rod, placed perpendicularly to the foot-rule, and having both lateral and perpendicular motion. When the point of this rod is placed on the same line with the other two, it indicates zero; and when retracted, a graduated scale in the rod itself indicates the amount of perpendicular motion, while the foot-rule, on which all the limbs are fixed, gives their respective lateral distances from each other. By this instrument every external measurement, whether of distances or of angles, can be at once determined. All the points are guarded by bulbous extremities.

20. *Graduated Balls and Rods for Measuring Openings.*—These cannot reasonably be introduced into the *apparatus minor*, but are very necessary in cases where accuracy is of importance, particularly in determining the size of the orifices

of the heart. Graduated balls were recommended for this purpose some years ago, by Dr Peacock, then superintendent of morbid anatomy in Edinburgh, and now physician to St Thomas's Hospital. I have also found them very useful and far more accurate than Bouillaud's method of laying open the orifices, and measuring their circumference. For the smaller sizes of openings, bristles, probes, catheters, and rectum-bougies, or other apparatus at hand or easily procured, will usually suffice; more considerable sizes are to be gauged by a series of balls, carefully selected, so as to give a sufficient number of progressive sizes. Any well-furnished china-shop or toy-shop will usually enable the practitioner, at very small expense, to provide himself with these; and by means of the callipers mentioned above, he can ascertain their diameter, either before or after using them. I have no hesitation in recommending that every one engaged in *post-mortem* investigations should be possessed of such a set of graduated balls. It is not necessary, as some assert, that every object should be measured, but there is no doubt, that the frequent use of precise measurements, tends to render both the mind and the eye more accurate, and cases may arise at any time, where great accuracy may be of the highest importance.

21. *Fluid Measures*, from a minim to a gallon, should be in every hospital theatre.

22. *Scales and Weights* ought also to form a part of the *apparatus major*. The most convenient scales for weighing the organs, are those in which the arms of the lever support directly, and without the intervention of suspensory chains, on the one side, a platform for the weights, on the other, a cup for the objects to be weighed. Such balances are to be had everywhere.

23. *Areometer* for testing specific gravity of solids and fluids. In the case of the fluids, the specific gravity is rarely of much importance to the morbid anatomist; but from the determination of the density of the solid organs, much instruction is to obtained. I have been accustomed for several years past, to make observations on specific gravity, in the following manner. The results are perhaps not altogether accurate, but they are sufficiently so for most purposes. A similar method has been employed by Mr Sankey, at the London Fever Hospital, in his very important observations on the density of the brain. A saturated solution of common salt is prepared, having a specific gravity of about 1200, and by mixing this with various proportions of water, test solutions may be obtained of various densities, as tested by the fluid areometer, and including all the degrees of density presented

by the internal organs of the human body. Small portions of the organ to be tested are then placed in these solutions, and the experiment is pursued until a solution is found in which the mass preserves a nearly perfect equilibrium, neither tending to rise nor to fall to the bottom. This solution is then presumed to be identical in specific gravity with the organ so tested. It is important to guard against the following fallacies. A portion of a soft organ rapidly parts with its own fluids, and imbibes the surrounding solution; so that except at the first moments after-immersion, the experiment is not valid. Moreover, a certain number of air bubbles, either suspended in the fluid or adhering to the solid tissue after its immersion, will vitiate the result. Lastly, if the fluid be newly mixed, it is necessary to take particular care that the mixture is completely effected. Lovi's density beads, or the common urinometer, supplemented, however, by an additional instrument, ranging from 1050 or 1060, up to 1120 or 1140, will be found sufficient for most purposes.

24. *Simple Lenses and Microscopes.*— A series of good pocket lenses is, if not indispensable, at least of the highest possible usefulness to the morbid anatomist. Plano-convex lenses are readily procured of good quality, up to 20 or even 30 diameters magnifying power; but beyond this, Coddington lenses or Wollaston's doublets are required to give a clear and flat field. The Stanhope lens which is used by naturalists is not a good one for anatomical purposes, nor is it in almost any case so useful as the small Coddington lenses now manufactured by Mr James Bryson, optician, Princes Street. These may be had of various powers up to more than 200 diameters. The lower powers up to 50 diameters are best set in a simple metallic ring, with a short handle, protecting the lens completely on the side of the eye, and on the side of the object armed with two short crutches, which, without intercepting much light, prevent the convexity of the lens from approaching too closely to any hard, flat surface. With a little practice, both opaque and transparent objects may be readily examined with these lenses, which will also be found useful in clinical studies, as they may be applied to urine in test-tubes, some diseases of the skin, crystalline powders, &c. Lenses of higher power than this are scarcely useful, except for objects to be viewed by transmitted light; but by the adaptation of a small plate of thin glass set in silver, and focalized by means of a small screw, Mr Bryson has succeeded in making a perfectly effective simple microscope, easily carried in the pocket, and with which any one at all conversant with such studies may make

investigations into the blood and most of the tissues, when a compound microscope is not at hand.[1] I believe Mr Pillischer, of Oxford Street, London, also makes high power simple microscopes, adapted for the examination of urinary sediments, &c., but I have not seen them.

The compound microscope is necessary for all elaborate investigations into animal structures; and, indeed, except by one familiar with its use, the higher powers of the simple microscope can scarcely be advantageously employed. The use of high powers of all kinds, however, is only safe for those who have devoted a certain amount of time and study to mastering the preliminary difficulties of histological anatomy. Moreover, no one can tread far or surely, except in the beaten track of inquiry, by means of high powers *alone;* simple lenses and careful dissection by the naked eye are necessary to all safe progress; for ample experience has proved that by the neglect of these, endless confusion has been introduced into pathological science. When carefully and correctly pursued, microscopical studies tend to educate more highly the unaided eye, and, therefore, to lead to an increased amount of confidence in its decisions; but this most desirable and highest result of such investigations can never be attained by those who neglect the intermediate links between the high powers and ordinary vision, nor can the opinion of such observers (except in regard to the mere description of individual objects) be commonly accepted as worthy of confidence.

I shall not attempt to indicate the things necessary to be known in the selection or in the use of a compound microscope. Numerous works and papers have been written on this subject, which are accessible to all; as those of Quekett, Hannover, Schacht, and Dr Bennett of this University, to whose untiring zeal our Scottish students owe much of their familiarity with microscopical investigations. French instruments, of very moderate price, and good uniform quality, can now be had in Edinburgh, of Mr Young, Princes street.

The various adjuncts to the microscope, such as dissecting apparatus, needles, double-knives, &c., I shall also pass over.

25. *Sponges.*—Many practitioners habitually carry sponges about with them to *post-mortem* examinations, and have bags of oiled silk for holding them when wetted or soiled. I have found that in most examinations at private houses (unless

---

[1] In some recent publications I observe that these lenses of Mr Bryson's are called Wollaston's doublets. This is incorrect; they are, as above mentioned, Coddington lenses. Wollaston's doublets are, however, manufactured in great perfection by some of the English opticians.

where clean sponges can be procured), it is better to employ tow for the purpose of removing superfluous fluid; as this can always be had in any quantity when required, and can be thrown away when it has served its purpose. The work, however, is more expeditiously done by sponges, which, accordingly, may be employed in hospitals, and where a large quantity of fluid has to be baled out.

26. *A small tinned-iron box*, with compartments, is useful for the removal of small portions of organs for microscopical or other examinations. One of the compartments should contain three or four strong glass tubes, about 1½ inches long, and as broad as an ordinary test-tube, well corked, for the purpose of containing small portions of fluid matter. It is a proper precaution never to use the same cork twice in these tubes, and never to use old or decaying corks, as these readily impart to fluids elements which may prove confusing in a microscopic demonstration. Perhaps a better plan would be to have a series of glass-stoppered tubes made for the purpose.

27. It remains to be mentioned what ought to be the furnishings and arrangements of a theatre for public demonstrations in morbid anatomy. Such a theatre should be in the immediate vicinity of the dead-room, and in a situation where free ventilation and a good light (the latter, if possible, from the ceiling) are procurable. The supply of water should be abundant; and a private work-room should be attached, also lighted from the roof, and well furnished with water, fire, and with shelving or presses for keeping preparations until they are ready to be placed in the museum. Let me add here that I am satisfied that a portion of the funds of every large hospital ought to be devoted to the preservation of such preparations as are of real clinical interest, and are illustrated by its case-books. This is done in all the great hospitals of London, and I have seen abundant occasion to regret the absence of such a collection in Edinburgh, where, notwithstanding the existence of two large and valuable public museums of morbid anatomy, very many of the most interesting preparations are daily lost to science, or preserved only by the zeal of private collectors.

The theatre should comprise a central area of sufficient size to admit of free movement around the dissecting tables, which should be one or more according to the size of the hospital; and a series of raised seats for spectators of the proceedings. The best form of table is, I believe, that which was introduced into Edinburgh by my predecessor in office, Dr Bennett. It may be of wood, painted, or coated with lead. It should be not less than 8 feet in length, and broad enough to admit of two bodies being placed on it at once. It should revolve upon a central axis, and should incline from the circumference, so as to allow of the free gravitation of fluids, through the medium of channels cut in the upper surface, towards an opening in the centre. By means of such a table much trouble is saved, cleanliness is subserved, and the utmost facility is given for demonstrating to the spectators everything connected with the external appearances, or with such of the internal parts as cannot be removed from the body for the purpose.

For the better demonstration and dissection of the internal organs, it is advisable to have, immediately in front of the spectators, a shallow leaden trough, several feet in length and breadth, connected with a waste-pipe, and communicating with another small covered trough (on a lower level), into which fæcal matter or other impurities may be swept in the course of the dissection. Immediately over the larger of these troughs should be fixed two or three water-jets playing downwards. One of these should be between 1 and 2 feet above the level of the trough, and the water-pipe should be armed with a rose. The others should be simple jets, nearer the bottom of the trough, and it is often convenient that to one of the jets should be adapted a small piece of vulcanized India-rubber tubing, for the purpose of conveying the water to any part of the trough. Warm and cold water-pipes, and a complete apparatus for washing, are of course indispensable.

In the demonstration of the morbid organs it is convenient to have a series of wooden trays of various sizes, in which the parts are placed to be handed round.

General Directions as to the Mode of Conducting Post-mortem *Examinations and of Recording the Results.*

I had occasion, a few years ago, to perform in the Theatre of the Royal Infirmary, a *post-mortem* examination in the case of a woman who died under the care of one of the physicians to the institution. On proceeding, it was found that the physician and myself had been anticipated in our purpose of unravelling the obscurities of an interesting case, by a gentleman armed with a warrant from the public authorities. The case did not come to trial; and as no explanation of the facts could be procured from the legal authorities, I am still in the dark as to the nature of the report presented, and quite at a loss to understand on what ground the gentleman alluded to conceived it to be his duty, without communicating with any of those responsible for the treatment, to procure a legal mandate to supersede the ordinary functions of the medical officers of the house. Certain it is that the case was very imperfectly investigated; for, with the exception of the heart (which was carried off as a trophy, and returned to me by special application a fortnight afterwards), and the *anterior* part of the lungs, not a trace could be discovered indicating that any organ had been disturbed. Death had in fact been caused by a complex series of morbid conditions affecting the heart, arteries, lung, liver, and kidney; but some imperfect knowledge of the history of the case (derived from a preliminary gossiping conversation with the nurses of the Infirmary) had induced the professional examiners employed by the authorities to confine their researches within the narrow limits above specified.

If the above were a solitary, or even a very exceptional instance of the mode in which dissections are sometimes performed in cases of medical and legal interest, it would be superfluous to allude to it; but this is, as I have reason to know, far from being the case. The fact is, that the duties of a medico-legal examiner are in this country so lightly appreciated, and so inadequately remunerated, that those who could best perform them shrink from a service in which they can gain little credit, and which may bring them into unpleasant collision with their professional brethren. Hence nothing is more common than to see medical evidence, even as to facts, break down under cross-examination; a circumstance which has afforded abundant ground for cavil, and impertinent depreciation of the value of medical testimony in general. In this respect the administration of justice in Britain stands in most unfavourable contrast to that of France and Germany, and it would surely be better worth while to inquire into and to remedy the causes of this difference, than to indulge in ill-natured criticism of its results.

The performance of a *post-mortem* examination, whether for legal or scientific ends, is not to be accomplished successfully, nor can its results be made the basis of secure conclusions, unless the operator be well assured by previous experience that he knows how to distinguish the slightest visible traces of disease in every important organ of the body. This knowledge can of course only be acquired in the first instance, and retained afterwards, by habitual use of the opportunities presented in hospital and private practice of making a careful examination of all the viscera. Such examinations are now constantly made in most of our large hospitals, but they ought not to be confined to them; indeed, there is far greater necessity for such investigations in private practice, where the opportunities of keeping the eye educated, being rarer, are the more sedulously to be used. Yet it is too common to see examinations conducted entirely with reference to one fixed idea of the disease. The cavities being laid open, a rush is made at the suspected organ, and if a supposed adequate cause of death is found there, everything else is either neglected altogether, or very hurriedly and inaccurately observed. If those who take this short and easy way of getting at results were careful in recording them to indicate the various steps of their proceedings, or even to give a general idea of the *rate* at which their exploits among the viscera were performed, the interests of science would not suffer so much as there is reason to suppose they do from the "false facts" alluded to by Cullen. But it is so easy to make the assertion that "the other viscera were healthy," or that "the abdominal viscera were free from disease," and so difficult, as every experienced anatomist knows, to verify such sweeping generalizations, that it is seldom prudent to infer from them anything more than that the author of the record in which they occur was willing to save himself trouble, and, as a matter of convenience, threw together the results of a certain number of careless and inadequate observations into these formulas. A conscientious, careful, and well-informed observer will generally protect himself from any such suspicion, by making his record bear the character of an accurate narrative of his proceedings, indicating at once where they were complete and elaborate, and where they were from any cause defective or questionable.

An examination may be from many causes insufficient as a basis for general statements. Time, opportunity, skill, may each or all be wanting; and of these facts the reader of a record can only judge through the medium of the record itself. He looks, therefore, to find in that document details sufficiently ample, not merely to indicate the important facts observed, but to leave no doubt as to the method adopted in ascertaining those facts, and the amount of care bestowed upon the entire work of the examination. For this reason it is a good general rule that the record should at least state *separately* the condition of each organ actually examined, and, in many cases, the details of observation from which its condition of health or the contrary was inferred.

Of course, it is quite possible to push this principle to the verge of pedantry, and to make the record of a *post-mortem* examination a tedious and intolerable mass of dry and trifling facts. But if care be taken to study conciseness of description, few readers will quarrel with such details as are really necessary to authenticate and give a graphic character to the narrative. These will, on the contrary, when intelligently recorded, be highly appreciated by many who would turn away from a shorter and more summary account, as bearing the marks of carelessness and haste.

The amount of labour bestowed on an examination must, of course, in the majority of cases, have some relation to its preconceived importance; and it is not only reasonable, but necessary, that the organs known to be seats of disease should occupy the first and the largest share of the anatomist's attention. Even in hospitals, it is never an easy, and seldom a profitable task, to carry out on the large scale the instructions of some writers, by the examination of *every organ* in *every case;* though, in a limited number of cases, and with a special object in view, this method is calculated to subserve the purposes of science, by furnishing accurate data for the comparison of one disease with another, or with the results of a miscellaneous collection of cases. Still less can I recommend the method said to be pursued by some of our indefatigable brethren in Germany, to begin the dissection always with the least interesting organs, in order, I suppose, to bring the process to a kind of climax at its close. Those who know practically the time often required for the thorough and careful examination of a single important and obscure lesion, will not readily adopt this method, knowing that they must not unfrequently sacrifice minor objects of interest to those which, in the natural course of the dissection, and following out the train of ideas suggested by the symptoms, are first made the subjects of investigation.

Although, however, it is not wise nor expedient to lay down any absolute rule as to what should, and what should not be examined, in cases of ordinary fatal disease, there are certain internal viscera, whose condition in almost all cases of death, from whatever cause, is interesting, not merely from the frequency of their local disease, but from their importance as indications of the general condition of the system. The lungs, from the extreme frequency with which they share in the diseases of the economy in general; the heart, as indicating the state of the machinery of circulation; the liver and spleen, as revealing the condition of the portal system; the kidney, from the great frequency and symptomatic importance even of its slightest derangements, are all of them organs which the practitioner is scarcely ever justified in omitting to examine. This may also in some degree be said of the brain, an organ whose functional derangements are, however, much more frequently unconnected with obvious structural changes, than in any of those above mentioned. The examination of the uterus, in these days of diagnostic refinement, has assumed the highest degree of importance, and should rarely be omitted. The alimentary canal, the pancreas, the spinal cord, are more frequently passed over by many; but where facilities exist for performing examinations, and where the number is not oppressively great, there is little doubt that much advantage would accrue to science from the more constant pursuit of information in these directions.

The mode of conducting the examination of each of these parts will come under consideration hereafter.

The records of anatomical examinations should be kept in such a form as to be easily accessible. Most practitioners have their own ideas upon the subject of note-taking, which it is unnecessary to disturb by laying down any arbitrary rules. I shall hereafter indicate incidentally the order I commonly follow; meantime it may suffice to discuss the qualities which ought to belong to every record, without which it can never adequately fulfil its purpose; having which, it is a perfect document, whatever its form.

1. The record should be *brief*, so far as brevity can be attained by avoiding all superfluities or redundancies of expression. This valuable quality of brevity is indeed subordinate to some others, but it is nevertheless one of no mean importance. A long report, which owes its length to the elaborate description of normal conditions (as is very often the case with such documents) is not merely tedious; to the unskilled reader it is deceptive, and not a few pathological errors of great moment have arisen simply from the misinterpretation of facts stated, without due regard to their being natural or morbid. The writer should therefore keep constantly before him the fact that he is, or ought to be, reporting morbid appearances, *and these only;* and while he will not omit the description of anything about which doubt can be reasonably entertained, he will also recollect that if the description of an organ can be satisfactorily completed in the one word "normal," everything more than that word is an error, because it suggests doubt.

2. The report should be *exact*. Errors of commission and of omission are equally to be avoided. There is one way to secure this great and indispensable quality of the record,—to note every fact observed at the moment of observation. In cases of importance, and in all cases where the memory can-

not be absolutely trusted, this precaution should not be omitted.

Exactness of statement is much enhanced by the use of numbers in certain cases. The weight of organs; the stature, breadth, and circumference of the body; the diameter and size of tumours, cavities, openings; the situation of particular objects in relation to others, as expressed by inches upwards, downwards, &c.; all of these are cases where absolutely exact expressions may be advantageously used.

3. The expressions used should be perfectly *definite*. There is no excuse for confusion of ideas; the report deals, not with opinions more or less certain, but with demonstrable facts. Nothing therefore which is not capable of demonstration should be admitted. To say that a part is inflamed is inadmissible; because inflammation is a complex condition, not to be judged of by morbid anatomy alone, and oftentimes admitting of dispute. To say that a kidney is "in the first stage of Bright's disease," is inadmissible, because it leaves a wide margin for speculation as to the author's exact meaning. For similar reasons the words "pleurisy," "pneumonia," "pericarditis," and many others representing nosological ideas, and not simple anatomical facts, should be excluded from the vocabulary of the morbid anatomist.

Technical terms are used only to save time, and care should be taken that they are not made to serve as a veil for imperfect information. A lung has often appeared as "hepatized" in the description of one who would have been very much puzzled if asked to define the meaning of the word; and much error has been thus introduced into science.

4. The descriptions should be *graphic*. It is one thing to be accurate in description, another to be interesting. The latter object is to be sought by the use of striking, and, as it were, popular expressions. *Similitudes* form a large part of the stock in trade of the anatomical writer; and no one who has studied the writings of Laennec, or of the best of his successors, can fail to have been struck with the admirable ingenuity which is displayed in this respect. For the same reason, the pencil ought to be called in aid of the pen as much as possible, and this is particularly the case with microscopic descriptions, which are to most people barren and unreadable without such help.

5. The facts recorded should be rendered accessible for future reference by a sufficient index. I have elsewhere (Monthly Journal, xiv., 239) given full details as to a method for registering facts in pathology, whether in private or in

hospital practice. The extension of a system like this to records of morbid anatomy is not difficult, and has in point of fact been carried out in the Edinburgh Infirmary, and over a much longer period in St George's Hospital, London, under the management of Dr T. K. Chambers and others. The reader will find in the number of the Monthly Journal for March 1854, an account of Dr Chambers' method.

My own is very similar. Every morbid appearance which occurs is *posted* (as a merchant would say) into a *catalogue raisonné* or *ledger*. Under this, as a heading, are placed numbers referring to the cases in which that particular lesion was ascertained to be present. A strictly anatomical arrangement is adopted, the order of which may possibly be useful to the reader. I therefore insert it as an appendix to this paper; and I propose, in the next number of the Medical and Surgical Journal, to consider the details of a *post-mortem* examination.

Headings of Systematic Index to Records of Post-mortem *Examinations.*

I. NERVOUS SYSTEM.

1. Brain and Membranes
2. Spinal Cord and Membranes
3. Sympathetic Ganglia
4. Nerves

II. ORGANS OF THE SENSES.

1. Eye
2. Ear
3. Nasal Fossæ
4. Tongue (VI.–4)

III. ORGANS OF CIRCULATION.

1. Heart and Pericardium
2. Blood-vessels

a. Aorta and branches. *b.* Venæ Cavæ and branches. *c.* Pulmonary Arteries (Ductus Arteriosus). *d.* Pulmonary Veins. *e.* Vessels of Heart. *f.* Portal System. *g.* Umbilical Arteries. *h.* Umbilical Vein (Ductus Venosus). *i.* Capillaries and smaller Blood-vessels.

3. Blood.

IV. ORGANS OF RESPIRATION.

1. Larynx and Epiglottis
2. Trachea
3. Bronchi
4. Lungs
5. Pleuræ.

V. ABSORBENT SYSTEM.

1. Lymphatic & Lacteal Vessels
2. Absorbent Glands

a. Temporal. *b.* Occipital. *c.* Cervical. *d.* Axillary and Brachial. *e.* Anterior Mediastinal. *f.* Posterior Mediastinal. *g.* Bronchial. *h.* Splenic. *i.* Gastro-Hepatic. *k.* Mesenteric. *l.* Mesocolic. *m.* Lumbar. *n.* Sacral. *o.* Internal Iliac. *p.* Inguinal and Popliteal

3. Thoracic Duct
4. Right Lymphatic Duct

VI. ORGANS OF DIGESTION.

1. Lips, Gums, Cheeks
2. Teeth, Palate
3. Salivary Glands
4. Tongue (II.–4)
5. Internal Fauces
6. Pharynx
7. Œsophagus
8. Stomach
9. Small Intestine
10. Large Intestine
11. Anus
12. Liver
13. Pancreas

VII. Urinary Organs.

1. Kidneys 3. Urinary Bladder
2. Ureters 4. Urethra

VIII. Organs of Generation.

A. *Male.*

1. Testicles, and coverings
2. Vas Deferens and Cord
3. Vesiculæ Seminales
4. Prostate and Ducts
5. Penis (Urethra, VII.–4)

B. *Female.*

1. Uterus 3. Fallopian Tube
2. Ovaries 4. Vagina & Vulva

IX. Peritoneum.

X. Mammary Glands.

XI. The Glands without Ducts.

1. Spleen
2. Thyroid Gland
3. Thymus Gland
4. Supra-Renal Capsules
5. Pituitary and Pineal Glands

XII. Organs of Locomotion.

1. Skeleton, & Cartilaginous parts
2. The Joints
3. The Muscles. Tendons, Synovial Apparatus, Fasciæ

XIII. Integumentary System.

1. The Areolar Tissue
2. The Fat
3. The Skin, its Glands and Appendages

Art. VI.—*On the Displacements of the Uterus.* By J. Matthews Duncan, A.M., M.D., F.R.C.P.E.; Lecturer on Midwifery; Physician-Accoucheur to the Royal Dispensary, Edinburgh, &c.

The displacements of the uterus may be divided into two classes, according to the degree or amount of change of place. When this is to such an extent as to bring the organ into close proximity with the floor of the pelvis, or to expel it from that cavity altogether, then the affection is generally not described, in the ordinary parlance of the day, as a displacement, but as a prolapsus or procidentia. The former term is applied to those minor alterations of position which the uterus undergoes while within the cavity of the pelvis, and not prolapsed upon its floor. These changes in position may affect the entire organ or only a part of it. Under the name of uterine displacement, then, we include all the alterations of position of parts of the organ in regard to the rest of it, minor alterations of position of the whole organ, and the combinations of these changes. It must be understood that the term displacement, used thus nosologically, implies only changes taking place in the organ when it contains neither a polypus or ovum in its cavity, nor fibrous or other tumour in its walls, and when it is not forced to its extraordinary position by adhesions or by tumours or other disease in neighbouring parts.

Displacement, as thus limited and defined, is separated from prolapsus and procidentia, not only by the evident difference in position, but also by pathological differences to which we shall merely allude. In these latter diseases the rectum and bladder are necessarily simultaneously displaced, the vagina is more or less inverted; the pelvic fascia and other structures forming the floor and closing the outlet of the pelvis, are involved in the disease equally with the uterus.

Congenital displacements are here also excluded, being malformations and not secondary accidents. With these we include those cases of uterine distortion supposed to result from morbid involution of the uterus after delivery, the walls of the organ not being equally reduced in the absorption going on at that time.

Displacements of the uterus, although known and described by the most ancient authors, did not attract much attention from practical physicians in our days till the publication of the researches of Schmitt, Schweighauser, Boivin, Bazin, and others. In 1843, Dr Simpson's paper on this subject was read before the Medico-Chirurgical Society. He then pointed out the great frequency of displacement, and shewed how it could be diagnosed and rectified by means of the uterine bougie. Since this time the subject has been unremittingly before the profession. But the greatest difference of opinion still prevails as to the true pathology and proper treatment of this class of cases. The object of the present communication is to attempt to throw some light upon the physiology and pathology of uterine motions and displacements, with a view to the establishment of correct general principles of treatment.

Position of the Uterus.—Anatomists and obstetricians, in describing the uterus, generally assign to it a certain fixed position, and proceed to enumerate a series of ligaments to which they ascribe the function of maintaining it in this position. In discussing the question of what is a displacement and what is not, I shall have occasion to shew that difference of opinion exists as to what should be called the healthy or natural position. And there can be no doubt that the uterus has no fixed natural position, but enjoys much freedom of motion, and assumes different attitudes in its different conditions, and in the different conditions of the neighbouring organs. It is not so free and mobile as many of the neighbouring viscera, seeing that it has firm connection to the vagina and bladder, but at the same time its other attachments are such as to permit of extensive movement. The truth is that the uterus, if fixed and immoveable,[1] is *de facto* in a morbid condition, and generally therefore incapable of performing its chief function.

The position of the uterus is behind the bladder, in a plane anterior to the rectum, which lies partly behind it. It is, as it were, inserted upon and continuous (structurally, as well as anatomically) with the tube of the vagina. It lies generally near the mesial line, and more or less in the axis of the pelvis, and having the fundus near the plane of the pelvic brim. In the dead subject, the organ is generally found placed in various degrees of obliquity in relation to the axis of the brim. But there is great variety of position, according to the state of the neighbouring organs. The most common position is probably one of what would by some be called anteversion; but this depends on the empty state of the bladder. It is necessary to add that the position after death is not a good criterion of the ordinary position during life, as so much depends upon the muscular tonicity and vascular turgescence which disappear with life.

But while we discard the opinion that the uterus has a fixed normal attitude and position, it will, nevertheless, be useful in the description of the motions and displacements of the organ, to adopt a certain standard the deviations from which can be easily described in words. And for this purpose, we cannot do better than adopt any of the ordinary statements on this subject. In his work on Midwifery, &c. (p. 54), Burns describes the uterus in the erect posture as corresponding, nearly, with the axis of the brim of the pelvis, and, consequently, forming an angle more or less obtuse with the vagina, in which the os uteri is felt directed backward. Boivin and Dugés (*Traite des Maladies de l'Uterus*, p. 4) state, that the womb has its long diameter nearly in the same direction as the axis of the superior strait; its anterior surface looking downwards and forwards; the posterior upwards and backwards; the fundus upwards and forwards; the external orifice or os tincæ downwards and backwards. Mr Wilson, in his *Anatomists' Vade-mecum* (p. 618) describes the uterus as having the base directed upwards and forwards, and the apex downwards and backwards, in the line of the axis of the inlet of the pelvis.

In discoursing, then, upon this subject, we shall assume the *standard position* or site of the uterus to be when it lies nearly in the centre of the true pelvis, with the fundus near the plane of the brim; and the *standard direction* or attitude of the uterus to be when its long axis corresponds nearly with the axis of the brim of the pelvis, or with a line drawn at right angles to the plane of the brim from its centre.

---

[1] The uterus may be morbidly fixed in position by adhesion of its serous investment to other parts or organs, by inflammatory or malignant deposits in the structures of and surrounding the cervix, and by tumours of any kind situated in or near it.

In the sequel we shall attempt to shew that the position of the womb may vary to a great extent, without that variation being in any way to be regarded as a morbid phenomenon. But here it is necessary to point out, that a posterior oblique position, or a slightly retroverted position, is with justice regarded by many authors as not only not morbid, but as usual and natural, especially in women who have borne children. Astruc[1] describes the position of the uterus as being most natural and advantageous when it is in a line with the vagina. In his work on the Gravid Uterus, Burns[2] says, that in the unimpregnated state " the top of the uterus lies backwards towards the rectum; whilst its mouth is directed forwards." In more general terms, even M. Valleix[3] says, that " it is incontestable that a certain degree of inclination is perfectly compatible with health." " I am disposed (says Dr Churchill[4]) to think that the uterus, especially in women who have had children, has a wider range of position (without inconvenience) than we suppose."

Ligaments and Relations of the Uterus.—For rightly appreciating a description of the motions and displacements of the uterus, it is necessary to have a distinct comprehension of the various means by which it is maintained in its position and direction;—and, while authors are but little at variance in regard to the nature and arrangements of the tissues involved, they offer a great variety of opinions as to the part taken by one or other of these structures in this function. In the subsequent discussion of this subject, we believe sufficient reason will be found for maintaining that the vagina[5] is the principal instrument in deciding the position of the uterus *quoad* depression; whilst its attitude or direction are determined by the surrounding viscera, including the vagina, by its own condition mechanically considered, and partially by the state of the so-called ligaments.

The smaller end of the pyriform uterus is, as it were, inserted into the upper part of the contractile tube of the vagina. This tube at its upper part posteriorly is unsupported for a small and variable space, except by peritoneum. In front it is connected with and supported by the bladder, and similar connection and support is afforded to the uterus, with which it is united along the anterior surface of the cervix. On either side are the alæ vespertilionis, duplicatures of peritoneum, connecting the uterus with the lateral walls of the pelvis. From the two extremities of the fundus the round ligaments, at first inclosed in the broad ligaments, and then curving downwards and forwards, beneath the peritoneum of the pelvic walls, proceed to pass through the inguinal rings. They are composed of contractile fibrous tissue, similar to that of the uterus, along with vessels and nerves. There may also be described four cervical ligaments consisting of peritoneum, inclosing fibrous contractile tissue, two proceeding forwards towards the bladder, and two backwards towards the rectum. These are seen distinctly when the parts are stretched and forcibly separated. The uterus resting thus upon the vagina, is supported in front by the bladder, and behind by the rectum, having on either side the broad ligaments and ovaries, and is covered above by the folds of small intestine, coils of which occasionally pass between it and the other neighbouring organs. Lastly, there must be added the pelvic fascia, which, lying under the peritoneum, is reflected from the walls of the true pelvis to the bladder and rectum in front and behind, and between these, to the upper part of the vagina, proceeding upwards from which it is lost upon the uterus. This structure has probably much to do with prolapsus and procidentia of the uterus, but can exert little influence on the deviations now under consideration.

---

[1] Art of Midwifery, p. 13.

[2] On the Gravid Uterus, p. 8.

[3] Deviations Uterines, p. 167.

[4] Diseases of Women. 1850, p. 203.

[5] " The firmness and structure of the vagina support the womb."—Anat. and Physiol., &c., by John Bell. Vol. iii., p. 419.

In thus shortly enumerating the anatomical relations of the uterus, we have, according to custom, used the term ligament as applied to certain parts. But the term is unfortunately, not to say incorrectly, used, as it tends to mislead the student as to the nature and functions of the structures so denominated. These so-called ligaments have neither the structure nor functions of parts ordinarily so named, and instead of binding the uterus to a certain site, they will be found to afford scarcely any resistance to displacement of the uterus at all, unless when that arises from sudden and violent causes. The attachments of the uterus to the vagina and bladder are its chief ligaments, whilst the ligamenta lata and rotunda have only in a very slight degree this function. On the contrary, they follow the uterus in all its motions, mould themselves over it in its natural or morbid enlargements, and present but little obstacle to retroversion or anteversion by the sound, or to depression by volsella, for surgical purposes.[1] They might be more properly called gubernacula than ligaments, as the ligamenta rotunda have probably some influence upon the motions of the uterus, especially in early pregnancy; and the broad ligaments, although they do not prevent the motion of the organ, contribute mainly to these motions being confined chiefly to the antero-posterior direction. Displacements of the unimpregnated uterus have been ascribed by Morgagni, Stoll, Saxtorph, and others,[2] to relaxation of these parts, but this relaxation (or rather expansion) is undoubtedly not the cause but the effect of the displacement. The expansion of the peritoneal investment of the bladder might as appropriately be brought in as the cause of retention of urine, or the development of the uterus in pregnancy, as the result of the relaxation and expansion of the ligamenta lata and investing peritoneum.[3] Further, the fibrous tissue contained in the round ligaments, small in quantity as it is in the unimpregnated state, and having its peculiar physiological properties, can have very little influence.[4] Like all muscular tissue of organic life it enjoys great extensibility by slight causes, and its power of contraction is called into play, in ordinary circumstances, only by the natural physiological stimuli. The functions of this tissue are probably in a great measure in abeyance, like the corresponding tissue of the uterus, except during pregnancy, when it undergoes a large development.

---

[1] We quote the following illustrative remarks from Bichat's Anatomie Generale. "On voit très-souvent ces membranes abandonner et recouvrir tour à tour leurs organes respectifs; ainsi les ligamens larges, très-éloignés de la matrice dans l'état ordinaire, lui servent de membrane séreuse pendant la grossesse. L'intestin qui se distend emprunte du mésentère une enveloppe qui le quitte lorsquil se contracte. L'epiploon est tour à tour, comme l'a très bien observé M. Chaussier, membrane flottante dans le bas-ventre, et tunique de l'estomac. Souvent l'enveloppe péritonèale de la vessie l'abandonne presque en totalité. Le sac herniaire de ces énormes dèplacemens des visceres gastriques, n'a-t-il pas primitivement servi à tapisser les parois de bas-ventre? etc. . . . . . . C'est toujours une tissue lâche, facile à se distendre en tous sens, qui sert de moyen d'union, et jamais un système vasculaire sanguin, comme dans la plupart des autres adhérences." (Tome ii., p. 550.) Further on (p. 564), in discussing the properties of the serous system, he ascribes its extensibility to three causes; *1st*, the development of folds, as of the broad ligaments in pregnancy, &c., &c.; *2d*, the displacement of the membrane from one part to another; and, *3d*, distension or real expansion. The causes of its contractility correspond to those of its extensibility.

[2] Dict. de Med., t. xxx., p. 375. See also Davis' Obst. Med., vol. i., p. 524; Boivin and Dugés, Mal. de l'Uterus, p. 86, &c.

[3] "The uses ascribed to the ligaments have been to support the uterus from sinking too deep into the pelvis, and to steady it and direct it in its ascent during pregnancy. But whatever good they may do in the latter operation, they are certainly unfit for the former."—Anat. and Physiol., &c. By John Bell. Vol. iii., p. 421,

[4] "Hors le temps de la grossesse, les ligamens ne sont d'aucun usage à la matrice."—Deleurye, Traité des Accouchemens. Par. 127.

The influence of these parts can be to some extent illustrated by experiments on the dead subject. And in these it is found that in the lesser displacements downwards of the womb, the ligaments are not at all put upon the stretch, and such displacements are not facilitated by the division of the so-called ligaments. The well-known experiments of Burns, Ashwell, Kiwisch, Tanchou, and others, on the effect of artificial depression of the uterus upon the surrounding structures, confirm this statement.

Mobility and Movements of the Uterus.—But while, as already stated, anatomists and obstetricians generally fix upon a certain position of the unimpregnated uterus as its own and natural one, they are not ignorant of the mobility of the organ. Cruveilhier[1] and numerous other writers correctly describe it as floating in the cavity of the pelvis. This fact is well known to the obstetrician, for in making a physical examination of the upper part of the body of the organ through the hypogastrium with one hand, whilst he fixes the cervix and protrudes the organ as much as possible above the brim of the pelvis with the other, he finds it to float about and escape from the external hand on the smallest pressure, and this in every direction. And under such circumstances it may be necessary for rendering such investigation satisfactory, to fix the uterus by introducing a sound into its cavity. But the organ is thus moveable not only by external influences, but also by slight disturbances of the mechanism of its own equilibrium. Churchill,[2] for instance, mentions a case in which the physiological engorgement of the uterus in the menstrual molimen was enough to produce retroversion of the organ; and if this temporary cause was sufficient, how much more effect will be produced by more persistent and serious pathological changes?

The organ is not only mobile, but also constantly in motion. It is well known that the uterus is liable to be suddenly and extensively moved by accidental causes; for example, the sudden and temporary prolapsus or even procidentia induced by jumping from a height; the motions also caused by violent coughing or sneezing, are such as in some women to produce involuntary dejection of urine at each burst of cough. In women labouring under aggravated procidentia, this is well illustrated by their power of expelling the replaced organ by a slight cough, even when lying in the recumbent position. The effort also of straining depresses and displaces the uterus to a considerable extent. This may be effected at desire, and happens at every effort at stool.

---

[1] Anatomy of the Human Body. Engl. Trans., vol. i., p. 618.

[2] Diseases of Women, p. 289.

In addition to these occasional and accidental movements it has to undergo continual slight movements, from the motions of the various folds of small intestine which cover and surround its fundus. Its position is liable to be changed also by the state of the rectum as to repletion or vacuity; and partly from the mere presence of this bowel in the left side of the pelvis, partly from its frequent dilatation by its proper contents, the uterus is very generally found slightly displaced to the right of its standard position, forming an obliquity from right to left. But the most important part is taken in these movements by the bladder, to which we have seen the uterus is attached for at least a fourth of its length anteriorly. In its dilatation it carries the uterus slightly upwards, aad pushes it backwards; in its contraction it carries it forwards and downwards, into a direction of considerable anterior obliquity or partial anteversion. In many females these changes can be easily made out. We have already noticed the occasional effect produced by coughing and sneezing, and have here merely to remark that a similar but greatly slighter change is effected by respiration, the amount of which will vary with the position of the female, with the tension or relaxation of the abdomen, the state of the pelvic viscera, &c. Lastly, it is well known and easily ascertained that the position of the female affects the position of the uterus. Capuron (*L'Art des Accouch.*, p. 28) describes the direction of the uterus as being oblique, and variable according to the attitude of the woman. "In the erect position it is inclined towards the pubis, and in supination towards the sacrovertebral angle; when the body is seated it approaches the perpendicular; it is scarcely ever carried towards the right or left, but sometimes it turns upon itself, so that its surfaces look more or less to either side." In the recumbent posture, also, the uterus assumes a different position, according as the woman lies on her back or on her right or left side. As we have before stated, the extent of these various movements will be modified according to the state of the pelvic viscera. In a female whose parts are all weakened and relaxed by repeated pregnancy or by disease, they will be most extensive, whilst in a healthy female who has not been subjected to these or other causes of relaxation, these motions may be almost null.

Mechanism of Displacement.—We have already said that the vaginal tube is the chief agent in maintaining the uterus in its ordinary position. When this support is injured by relaxation, the uterus becomes depressed in the pelvis. Its own weight is sufficient to invert the upper part of the relaxed vagina, or to cause it to collapse. The uterine ligaments have no influence in preventing this, as they are not at all put upon the stretch by depression, to the extent of an inch or an inch and a half. Increase of weight of the womb may also be sufficient to cause displacement, without relaxation of the vagina. It is plain, then, that in the causation of these changes, there are two principal elements; first, the strength of the support; and, secondly, the weight of the body to be supported.

When the uterus is simply depressed or displaced downwards, it may maintain its standard attitude or inclination. Generally it does not, but assumes an inclination corresponding to the axis of the part of the pelvis in which it is lying. Thus in the minor descents of the uterus, which we are specially considering, it becomes somewhat retroverted from its standard attitude, leaving the axis of the brim and entering that of the cavity. This change is well illustrated in cases of prolapsus and procidentia, in which the uterus, in traversing the pelvic canal, does so, with its axis, in a line of direction corresponding nearly to the circle of Carus.[1] We have already noticed that some authors describe this posterior obliquity of the uterus, from its assumed standard attitude, as being normal, and when we remember that almost all women who have borne children, or suffered from uterine disease, have a certain amount of relaxation of the vagina, the opinion is easily accounted for, and is undoubtedly well founded.

Supposing the womb to be in this position and attitude of slight descent, and itself increased in weight, or subjected to weight or pressure from above, it may according to circumstances be further displaced in regard to obliquity, without farther depression, and anteversion or retroversion be the result.

Injury to the chief support of the uterus is not the only cause of displacement. The uterus may be increased in weight, or subjected to pressure. Under such circumstances we have already said that descent may be produced without morbid relaxation of the vagina. But in these conditions, whether the uterus becomes slightly depressed in the pelvis or not, the effect of the increased weight of the organ, or of pressure on it, is then to change its direction. It becomes more or less anteverted or retroverted. Anteversion will probably take place if the greater weight is in the anterior part of the organ. But it is not so frequent, and rarely so complete as retroversion. Many authors have found a most convenient explanation of this fact, in increased weight of the posterior wall alone of the uterus, caused by chronic inflammation there. But this pathological hypothesis, although sufficient, is not necessary. The lower third of the anterior wall of the uterus is supported by connection with the bladder, and the repletion of this organ tends to obviate the anterior obliquity of the uterus. The upper part of the vagina is also strengthened anteriorly by union with the bladder. On the other hand, the posterior wall of the uterus, as well as a considerable part of the adjoining vaginal wall, are quite unsupported by organic union with the rectum. The posterior wall of the uterus is larger, thicker, and more prominent than the anterior. In these mechanical peculiarities is found a sufficient explanation of the greater frequency and completeness of retroversion, especially when to this we add the relaxation of the vagina, slight descent of the uterus, and consequent slight posterior obliquity, which almost invariably accompany uterine lesions, and generally precede the occurrence of retroversion. And it must be observed that the continuance of the cause originally inducing slight anterior or posterior obliquity will always tend to increase the displacement.

---

[1] Meigs—Females and their Diseases, p. 148.

The elevated position of the increased weight of the body of the uterus, in cases of hypertrophy and engorgement, is one of the chief causes of versions and flexions. On the other hand, when the increase of weight is in the uterine cervix, there is always displacement downwards, the weight being then situated in the lowest part of the uterus.

Uterine flexions[1] are governed by the same laws as uterine versions. Regarded mechanically, they differ merely in the site of the flexion. In versions the flexion is at the top of the vagina. In flexions proper it is in or above the uterine cervix.

In flexions the uterine tissue is often relaxed, the uterus being thus, as it were, unable to support itself. This is the result of long-continued leucorrhœa, or of a morbid involution of the uterus after delivery, or it may occur without apparent cause. This uterine relaxation may frequently be ascertained to exist even during life, by the facility with which the organ is flexed under the examining finger, and especially by the use of the probe. In passing this into the relaxed uterus, it is not necessary to follow the direction of the canal, the uterus unfolding itself under the pressure of the instrument, and adapting itself to it.

---

[1] The term version implies displacement of the entire uterus, without change of form. Flexion denotes more or less of folding of the body of the uterus upon the cervix.

When flexion takes place in a distinct form, it is found to occur opposite the os internum. M. Virchow[1] has pointed out how this should be expected to happen from the anterior uterine wall being thinner at this point than elsewhere, and from the peritoneal reflexion at the same part forming a line, at which the uterus unsupported above should bend upon the lower part, which is strengthened by consolidation with the bladder.

Results of post-mortem Examination in Cases of Displacement.—The position of the uterus as observed after death, cannot be held as a criterion of its position during life. The changes which are produced by loss of vitality, besides the mechanical changes that may take place in neighbouring organs, are sufficient to produce important alterations of position; and it is unnecessary to refer to *post-mortem* examination to verify the position ascribed to the uterus, as that can be done during life with the greatest certainty.

In most cases of displacement, a *post-mortem* examination affords no additional information as to the state of the uterus and vagina. The former is found in a healthy state, or more or less enlarged and engorged. In flexions the altered shape of the uterus is in addition to be remarked. The flexion may be in the form of a curve or an angle, more or less acute. In some cases of flexion, as in two which I not long ago examined, the angle of flexion was not at all atrophied. But in many cases, a thinning of the uterine tissue adjoining the angle takes place. This thinning or atrophy is described by Virchow[2] as being sometimes to such an extent that the proper tissue has almost completely disappeared. M. Valleix pretends to have been able to feel, in rectified cases, both of anteflexion and of retroflexion, a fold or plicature of the uterine tissue at this point,—the result of the collapse, as it were, of the atrophied wall, under the weight of the body of the organ.

In examining uteri after death, it must be remembered that there exists naturally a slight anterior curvation of the uterus, which is greater in young than adult females.[3]

Great difference of opinion exists as to the comparative frequencies of the different displacements. In this question, we have the statements of practical obstetricians and of morbid anatomists. The former are conversant chiefly with those cases which are accompanied by painful symptoms; the latter have an indiscriminate field of observation.

---

[1] Verhandl. der Ges. fur Gubertshülfe in Berlin, 1851, p. 81. [2] Ibid.

[3] On this subject, see the observations of Boulard, on anteflexion as a normal condition of the uterus before pregnancy.—*Monthly Journal of Med. Sc.*, February 1854, p. 165.

Rokitansky and Virchow, founding on observations on the dead subject, agree in describing anteflexion as being much more frequent than retroflexion; while Velpeau, from his observations, is unable to ascribe to the one a greater frequency than the other. Mayer, on the other hand, has, in hospital practice, observed retroflexions to preponderate in number. But whatever may be the actual comparative frequency of the different displacements, there can be no doubt that the one most commonly coincident with severe symptoms is the retroversion; and that in the course of ordinary obstetric explorations, anteflexion is observed to be one of the most common displacements, excepting, of course, that downwards, which is found in almost every woman who has borne children, or laboured under uterine disease.

Causes of Displacement.—In accordance with the views of the mechanism of displacement which we have just enunciated, it will be found that the causes of displacement arrange themselves into two classes, which may be found operative either singly or conjointly. The first class includes all those changes in the upper part of the vagina, or in the uterus itself, which diminish the power of the former as an organ for the support of the latter, and of the latter in maintaining its normal attitude. The second class comprises all those changes which increase the pressure of the womb upon the parts supporting. These may be either changes increasing the weight of the uterus itself, or changes in its neighbourhood, which have the same ultimate effect by causing pressure upon it.

Among the first set of causes are to be found leucorrhœa, childbearing, and the relaxation consequent upon menstruation. The second class includes natural and morbid menstruation, acute and chronic metritis, whether involving the whole organ or only a part of it, and incomplete involution of the uterus after delivery or abortion. To these last must be added the bearing down or tenesmic efforts consequent on various forms of uterine disease, or on constipation, also flatulence and compression of the figure, especially of the abdomen. Elongation or relaxation of the round or of the broad ligaments we merely mention, as, for reasons already given, they are to be regarded as consequences generally of the disease, or if pre-existent to it, can have but a very trivial influence on its production. The only cases, in fact, where relaxation of these ligaments can precede displacement, are those which occur soon after delivery, and at that time the enlargement of the uterus will have immensely more influence than such relaxation, supposing this last to be present.

The first degree of descent, with or without version of the uterus, is familiarly known to all obstetricians as an almost invariable consequence of the relaxation of the upper part of the vagina, and when it is remembered that relaxation is a concomitant of diseases and accidents so ordinary as parturition, abortion, leucorrhœa, and menstruation, it will be evident how common such displacement must be. It is a very frequent statement of authors, that after parturition the uterus is found lower down than before. Indeed, Dr Hamilton, in describing uterine prolapsus, states that few women having a family attain the fiftieth year without some degree of the disease,[1] and this statement he very properly does not limit to simple descent, but includes in it also anteversion and retroversion. The influence of menstruation in relaxing the vagina is well known, as also the increased tendency to displacement from this cause.[2] The artificial withdrawing of blood, as by leeches, from the roof of the vagina, is sometimes followed by similar relaxation of the organ, and I have more than once seen prolapsus induced by too early rising after this operation.

That increased weight of the uterus is a cause of displacement is also proved by clinical facts. The increase of weight of the uterus in early pregnancy, and in cases where its walls or cavity contain tumours, always produces a certain degree of descent, and very frequently also anterior and posterior version and flexion.[3] When in pregnancy retroversion continues till the uterus has acquired such a size as to fill the true pelvis, or if it is suddenly produced at that time, then we have the accident known as retroversion of the gravid uterus. In menstruation, the uterus is by sanguineous congestion slightly increased in weight. In accordance with this we find Meissner,[4] Ashwell,[5] and many authors, describe the uterus as descending in the pelvis at this time. The same cause, with or without the coincident vaginal relaxation, may be the cause of anteversion or of retroversion, as in the case mentioned by Churchill. When the uterus is the seat of chronic or acute inflammation it is also displaced, and this happens whether the inflammation is in the cervix or in the body of the organ.[1] When the inflammatory engorgement is in the cervix the displacement is generally that of simple descent. In his work on uterine inflammation, Dr Bennet[2] has pointed out this circumstance, and adduced an extensive series of illustrative cases. This result of increase of weight in the cervix admits of an obvious explanation on mechanical principles, and it must be remembered that it is facilitated by the relaxation of the roof of the vagina, attending inflammation of the cervix of any severity or duration. When the body of the organ is the seat of the engorgement or inflammation, then the displacement is not so constantly downwards, but the uterus falls backwards or forwards,—in other words, is anteverted or retroverted. This is equally a result of clinical observation with the former, and appears susceptible of a simple mechanical explanation, by pointing out the circumstances in which it differs from the case of cervical enlargement. In the latter there is generally much leucorrhœa and vaginal relaxation; in the former disease these are less frequent. In the cervical enlargement the increase of weight is below, and will exert its power accordingly; in the hypertrophy or engorgement of the body the increase of weight is elevated, and the womb will therefore tend more to fall backwards or forwards. It falls most frequently backwards, because its posterior wall is largest and most prominent, and least efficiently supported, having no such connection as the lower part of the anterior wall has with the bladder, as well as the same wall of the vagina. For the production of these results it is not necessary to have extensive changes in the cervix or in the body of the womb, for it is known that the natural engorgement of the uterus in menstruation is sufficient to produce descent, or even retroversion.

---

[1] Outlines of Midwifery. Part I., p. 6.

[2] See Ashwell, Diseases of Women, p. 560; also Blundell, Dis. of Women, p. 26.

[3] See Nauche, Mal. prop. aux Femmes, vol. i., p. 106.

[4] Frauenzimmerkrankheiten, th. i., p. 596.

[5] Diseases of Women, p. 560.

As I have already stated, numerous modern authors have most conveniently found, or imagined they found, in retroversion posterior chronic metritis, and in anteversion anterior chronic metritis. But in the numerous examples of version and flexion which I have examined, I have never been able thus exclusively to localize the inflammatory engorgement. The enlargement of the uterus is, I have no doubt, in most cases general, and the occurrence of anteversion or retroversion is determined by other circumstances.

The occurrence of anterior or posterior flexions is explained in a similar manner; only in them there is frequently co-existent a ramollissement[1] or flabbiness of the uterine tissue. And in some cases which I have seen, this softening has been the only discoverable cause. I have more than once observed the artificial softening and relaxation of the cervix uteri and upper part of vagina produce complete anteversion, as in cases where this part was dilated by sponge tents for the discovery and removal of polypi.

---

[1] In metritis "souvent l'uterus est deplacé, abaissé ou incliné, en avant, en arriere, ou sur l'un ou l'autre coté."—*Dict. de Med.*, vol. xxx., p. 223.

[2] Prolapsus of the cervix is nearly always the result of its inflammation and enlargement, and not, as generally supposed, of laxity of the lateral ligaments." —*On Inflammation of the Uterus*, p. 305.

It is worth while noticing that the depression of the uterus in these various diseases is not only a sign of their presence, but also a circumstance favourable to the easy use of the surgical remedies that may be necessary. And it is a fact strongly illustrative of the views above stated, that as the cervical or general uterine engorgement is removed, the uterus retires in the pelvis to its healthy position. In old age the upper part of the vagina generally becomes contracted, the uterus atrophied and bloodless; and curiously in accordance with the opposite result of contrary circumstances, the uterus becomes much elevated in the pelvis.

The effect of pressure on the womb from above[2] is illustrated in cases of dropsy, of flatulence, of adjacent tumours. It is also shewn by the marked result of downward pressure by voluntary or involuntary effort. Violent coughing is sufficient in some women to produce sudden prolapse and involuntary evacuation of urine. Voluntary straining or bearing down also displaces the womb to a greater or less extent. A familiar illustration of this is the voluntarily induced prolapsus in women liable to this disease. I lately observed a curious example of retroversion which could be voluntarily produced. The woman had long ceased to menstruate, and said she was unable to work on account of violent efforts always producing what she called a falling-out of the womb. This statement was not credited by a physician who examined her, and with some justice, as investigation of the case shewed. The vagina was healthy, the os uteri in its natural situation, as also the body of the organ, at least when the woman was recumbent. Voluntary bearing down produced no prolapsus, the healthy vagina preventing this occurrence. But it displaced the uterus forcibly backwards, and a little perseverance in the straining produced a tumour resembling a vaginal rectocele, but more solid, being the fundus uteri retroverted, and pushing the back wall of the vagina before it through the os vaginæ. A somewhat similar case has lately been recorded, where the fundus uteri passed through the anus.

---

[1] "Diminished consistency (says Rokitansky) is not only presented as a relaxation of the uterus accompanied by marasmus, consequent upon the exhaustion induced by parturition, or arising from paralysis of the uterine fibre in puerperal diseases, but it occurs in a distinct form as pulpiness (marciditas), slight friability or fragility."—*Manual of Path. Anat.*, Sydenham Ed., vol. ii., p. 286.

[2] Under such influence the position of the womb may be changed from one displacement to another. Valleix (p. 65) mentions several such cases. In one he found, on the first examination, an anteverted uterus. Some time after, on examining, he found the organ retroverted. The woman died of phthisis. After death the uterus was found not increased in size, mobile, unadherent, and maintained in its retroverted position by a loop of the colon which rested on it.

Symptoms of Displacement.—In this part of our subject we shall not consider the constitutional states which frequently co-exist with uterine displacements, as their connection is only secondary, and an account of them would lead us beyond the scope of the present article.

Having thus limited our remarks to the local symptoms of displacement, we are arrested *in limine* by the question, Are there any symptoms of displacement? Do the minor displacements if simple and uncomplicated, with other disease in the organ, produce any painful sensations or derangements of function? We have already stated the circumstances giving rise to displacement. Are they the causes or not of the painful feelings and functional derangements connected with displacement? If they are, then displacement is merely a symptom of these states and circumstances. This question has bearings equally upon the symptoms and the treatment of these affections, and will be discussed under both heads. The most different opinions have been entertained on the subject. Many authors agree with Cruveilhier[1] in maintaining that the displacement is of no moment, but that the disease is the uterine engorgement causing it. Others, with Dr Simpson and M. Valleix, defend the contrary opinion. In my mind there is a conviction, based upon the study and observation of the causes, symptoms, and the results of different plans of treatment in these cases, that, as a general rule, displacement of the uterus, be it retroversion or anteversion, retroflexion or anteflexion, or the first stage of descent, is only a symptom of another affection which has pre-existed—that although there may be minor symptoms connected with the displacement alone, yet the chief symptoms are not caused by it, and the main indications of cure are not to be directed against it. This side of the question is maintained by Cruveilhier, Dubois, Velpeau, Bennet, and others.

In fact the great frequency of these minor displacements (as abundantly attested by many authors), and the frequent incidental discovery of versions and flexions in treating women, or examining them for other affections, render it certain that they exist frequently without causing any symptoms whatever, —that they are in fact consistent with perfect health. The so-called cases of displacement are those where, more or less in connection with it, painful symptoms have supervened. That the simple displacement is not the cause of its so-called symptoms appears from considering the almost invariable displacement of the womb in early pregnancy, its enlargement and pressure in every direction, and the same circumstances in cases of uterine tumours, without any symptoms whatever. Moreover, even the prolapsus and procidentia of the womb frequently produce none of the so-called symptoms of the minor displacements, and may be quite harmless, except from the unavoidable secondary distresses consequent on the exposure of the organ.

---

[1] "La deviation n'est rien—l'engorgement tout." See Valleix's Deviations Uterines, p. 6.

The symptoms usually ascribed to displacements are variously given by authors according to the nature of the displacement, but these distinctions are seldom observed in nature. The most frequent accompaniment of this affection is pain, variously described as dragging, pressing, tearing, and more or less severe in the lower part of the back, the region of the sacrum, the anus, the vulva, the hypogastrium, or the inside of the thighs. In addition, complaints of bearing down are common. There is sometimes irritability of bladder, frequent calls to make water, sometimes pain at going to stool, and difficulty in passing the evacuations. Aggravation of all these symptoms follows long continuance in the erect position, and accompanies the menstrual period.

These phenomena are described as symptoms also of chronic metritis, of uterine catarrh, and of the various forms of leucorrhœa, and consequently give us very little assistance in diagnosing the disease, or settling its true nature.

In different cases these symptoms present an endless variety of degrees and combinations. But there are two great classes of cases, the features of which are more marked in nature than we can easily make them in words. In the first the prevailing character is that of relaxation. The vagina is dilated and softened, perhaps partially prolapsed. The rectum is enlarged and partially prolapsed, and almost if not altogether powerless to expel the motions. There is much complaint of pain, which has in great part a neuralgic character. The erect posture cannot be long maintained from its causing increase of the pains and producing the prolapsus of the rectum and vagina, or tendency thereto, inducing also painful irritation and itching of the vulva and perinæum. Such women are generally in very feeble and disordered general health, and not unfrequently have borne large families. They indulge in the recumbent position as the only relief to their sufferings.

In the other class of cases the main characteristic is the engorgement or subacute inflammation of the uterus, which is enlarged and tender to the touch. The ovarian regions and the vagina itself are also painful on being pressed upon. These symptoms are greatly aggravated at times, especially during menstruation, when the tenderness may even simulate peritonitis in the hypogastric region. There is no tendency to prolapse of the vagina or rectum, although there may be hemorrhoidal irritation in the latter. Such cases often have much feeling of bearing down, but the effect of continuance in the erect posture is not so marked or distressing. This condition is not so frequently connected with undermined general health, or with repeated parturition, as the former.

In both these sets of cases many modern obstetricians would see nothing but a version or other displacement of the womb. It appears to us that it is seldom a matter of much moment except as a symptom.

Numerous other symptoms have been attributed to this affection. Some of these are so rare as not to merit consideration here. Others have no satisfactory evidence in their support. Among these latter are sterility, amenorrhœa, dysmenorrhœa, menorrhagia—states, it must be remembered, frequently connected with chronic metritis, inflammation, and ulceration of the cervix, and other uterine affections which are the common causes and companions of displacement. Lately M. Valleix[1] has in like manner included leucorrhœa among the symptoms of displacement, because he has observed it diminish after replacement of the organ. These errors it is only necessary to notice.

Although these states and symptoms are neither necessarily connected with, or symptomatic of, displacement in general, it is probable that some of them are occasionally the mechanical consequences of displacement. It thus may happen that the close apposition of the os uteri against the anterior or posterior vaginal wall in retroversion and anteversion, may preclude the admission of semen into its cavity, or the flexion of the cervix, may have the same result by closing the os internum. The flexure of the cervix may in some cases also act in closing the passage against the escape of the secretions of the cavity, whether mucous or menstrual; and the uterine efforts for the expulsion of its accumulating contents, will be called uterine colic in the one case, and dysmenorrhœa in the other.

---

[1] Deviations Uterines. Paris, 1852. P. 80.

Diagnosis of Displacements.—The displacements of the uterus are generally recognised with facility on making a tactile examination of the pelvis. In cases of depression or displacement downwards, the os uteri is approximated to the os vaginæ, by the partial inversion or simple depression of the upper part of the vagina, which is at the same time relaxed and dilated. In the recumbent position, especially on the side, the displacement may disappear, and to make sure of its existence it may be necessary to examine the woman in the erect position, and immediately after having been in that attitude for some time. But this is seldom required, as even in the recumbent position the change of place is generally easily felt, or can be produced by voluntary bearing down. The affection must be distinguished from congenital depression of the uterus, or shortness of the vagina. In this last, the uterus cannot be elevated with the finger, and replaced, and the part maintains its unnatural proximity to the os vaginæ, while the organs are in a state of perfect health. Such cases are not very rare.

In different women the length of the vagina varies considerably. The average length in healthy women is about 3½ inches. But in women who have borne children, or are the subjects of uterine disease, the distance of the os uteri from the os vagina is only about 2½ inches.

The flexions and versions of the womb can generally be recognised by a tactile examination. In the versions there is felt through the roof of the vagina a hard body, which can be traced to be continuous with the cervix; according to the position of the fundus, is the position of the cervix and the direction of the os uteri. In anteversions it will look more directly backwards than is natural. In retroversions it will be directed forwards, or the os may even look upwards and forwards, if the retroversion be very marked. In the flexions the continuity of the body of the uterus with the cervix can be less distinctly traced, a retiring angle being felt between them. The cervix may be in its natural position, or near it; sometimes it is flexed to the same side as the body; it is also frequently displaced in the pelvis, in a direction contrary to the flexion. In both versions and flexions, displacement downwards generally takes place at the same time. In examining for displacements, one hand should be applied externally over the hypogastrium, to depress and fix the uterus by slight pressure. In many women, especially the thin and relaxed, the whole uterus can be distinctly felt between the fingers of the two hands, and the absence of tumours in its walls certainly made out. But in some cases this is impossible, and then the uterine sound may be introduced, if it is ascertained that the female is not pregnant. By this instrument, the advantages of which have been so strongly insisted upon by Dr Simpson, the direction of the uterine cavity can be ascertained to coincide with that of the version or flexion, and by it the uterus can be rectified, and the abnormal arrangement temporarily removed.

By the careful use of these means the displacements of the uterus can always be diagnosed from other conditions with which it is liable to be confounded. These are small tumours in the anterior or posterior uterine wall, or inflammatory indurations felt in or through the roof of the vagina, or cysts or tumours between the uterus and bladder, or uterus and rectum.

The aid of the speculum has been invoked to diagnose displacement. The observation of the difficulty of exposing the os, of the direction of the speculum, of the view obtained of the os, and especially the relative amount of the various surfaces of the cervix exposed in the speculum, will undoubtedly supply indications. But examination by this means alone would give very doubtful results, whilst the examination as conducted without it supplies us with the only trustworthy information.

Treatment of Displacement.—In discussing this subject we are met by the question—In cases where the uterus is found displaced, is there any treatment applicable to the simple displacement? Is not the displacement merely a symptom or result of some other morbid change, and ought not the treatment to be directed against the latter? In the case of procidentia, the uterus lying externally is the cause of extreme inconvenience and of suffering to the patient—just as a dislocated shoulder is—and one of the main objects of treatment is to replace it, and keep it within the pelvis. The pessaries used for preventing the prolapse are inimical to the complete recovery of the patient,—that is, to her coming to be able to walk and work without the prolapsus recurring. But the evils attendant upon procidentia are so great as, in many cases at least, to overcome any scruples as to the treatment by pessary, to prevent its recurrence, even at the risk of a failure of complete cure.

We have already stated that in the great majority of cases of the minor displacements under discussion, the dislocation of the uterus goes for very little, and that the disease to be cured is not the change of place in the uterus, but the changes in structure and function that have taken place in that organ and the vagina.[1] In conjunction with these morbid states we frequently observe hysteria, dyspepsia, and various forms of deranged general health, besides various uterine affections which are, to say the least, not necessarily connected with the displacement. These concurrent diseases complicating cases of displacement, as they frequently do, render it difficult to judge what symptoms are to be ascribed to the uterine, and what to the general complaint. And the difficulty does not end there, for the treatment being necessarily directed simultaneously against both conditions, it is difficult to adjudge to the different items of the treatment the different benefits which may have accrued. It is only by a careful observation of the circumstances of numerous cases, and of the results of different modes of treatment, that the true therapeutics of displacement can be arrived at.

The only treatment for the mechanical phenomenon of displacement is the rectification of the organ. This it has been proposed to effect in various ways. Many different vaginal pessaries have been proposed, especially by French and German obstetricians. Some have been intended to lie in the rectum, to prevent the motion of the cervix backwards in anteversion, and of the body of the uterus in retroversion. Others have been placed in the vagina, in front of or behind the cervix, to prevent its displacement. Others have been so formed as to seize the cervix (in a cup and ball fashion), and maintain it *in situ*. None of these has, however, come into use in this country, as, indeed, none of them deserved. A discussion upon their merits would occupy too much space, and I shall therefore pass them by with this remark, that they not only fail in curing, but also in fulfilling the indication of replacement. The only effectual instrument for maintaining the uterus in its position of replacement is the intra-uterine pessary of Dr Simpson. The uterus is fixed by a metallic stem introduced through the cavities of the cervix and body of the organ. This metallic stem is, as is well known, connected by a vaginal stem with additional metallic framework applied over the pubis. This last, aided by the support afforded to the other parts of the instrument by the vagina and uterus, maintains the whole *in situ*. The difficulty, then, of retention in a replaced position is overcome in this manner. But it remains to be seen whether the use of the instrument affords the advantages which may at first sight be expected from it.

---

[1] This opinion is tersely expressed in the words of M. Cruveilhier—"La deviation n'est rien, l'engorgement est tout;" Ajoutant—"Faites cesser l'un, et l'autre disparaitra spontanément." See Valleix; Deviat. Uter., p. 6.

The instrument maintains the uterus in a fixed, and therefore an unnatural position. In many females its introduction and retention are accompanied with so much pain that its use is impossible. Not to speak of the physical annoyance to the wearer, the instrument is necessarily a source of constant irritation to the mucous membrane of the vagina and uterus, inducing leucorrhœa, ulceration of the vagina and cervix, uterine catarrh, and profuse and frequent menstruation, in most cases where its use has been continued above a few days. Besides these results, the instrument is occasionally the cause of inflammation and abscess in the deep-seated generative organs. But the most serious objection to the use of these instruments is founded on a consideration of the causes of displacement. The presence of the pessary is not favourable to the disappearance of the relaxation of the vagina, and the presence of the intra-uterine stem in the womb cannot fail to produce or maintain the engorged or hypertrophied condition of that organ, which is one of the most frequent causes of displacement. In accordance with these views we find Dr Bennet,[1] when speaking of the treatment of inflammation of the uterus accompanying retroversion, says that "it is the inflammatory disease that requires to be treated. If the contrary opinion (he adds) prevails now with some practitioners, it is because they are under the influence of erroneous theoretical opinions. Overlooking the real disease, they merely treat the imaginary one, and thus do more harm than good. . . . . What proves retroversion of the uterus to be merely an epiphenomenon in the class of cases to which I am now alluding,—those in which it is accompanied by some inflammatory condition,—is, that when the latter is thoroughly cured, all morbid symptoms disappear, without any therapeutic means having been directed to the retroversion; and that, in very many cases, the uterus gradually resumes, partly or entirely, its natural position. But, even if it does not, the circumstance is of little or no consequence."

Entirely opposite opinions have been expressed by Drs Simpson and Valleix, and have led them to recommend the mechanical treatment of cases of displacement. These authors agree with Desormeaux in regarding the congestion and hypertrophy of the uterus which so frequently accompany displacement, as being more frequently the effects than the cause of the retroversion,[1] and M. Valleix goes the length of asserting that under the use of the intra-uterine pessary he has observed the uterine enlargement to diminish.

---

[1] On Inflammation of the Uterus. Third Ed., p. 346.

Such opinions are evidently opposed to the entire train of argument followed by me in this discussion, and I shall therefore not weary the reader by recapitulation. I shall only observe that cases of displacement, of whatever kind, have been in very numerous instances deprived of all their painful symptoms, by treatment directed solely against the uterine inflammatory engorgement, and that with or without the disappearance of the displacement; further that cases of displacement caused by uterine hypertrophy or vaginal relaxation, or both, abound without any symptoms whatever, and without any inflammatory engorgement and its attendant painful phenomena being induced.

The use of intra-uterine pessaries has been so highly recommended, in the treatment of the anterior and posterior displacements, by Drs Simpson, Valleix, Rigby, Protheroe Smith, and others, that it demands the most careful consideration. Abundant experience shews that these instruments may be worn for long periods, in many cases, without compromising the safety or even the general health of the patient. But it is another question to decide whether their use is followed by cure. Some of these authors maintain, with Dr Simpson, that the uterine congestion and hypertrophy which coexist with the displacement are most frequently the result of the malposition, and require for their successful treatment that the malposition be first of all rectified. Others, with M. Valleix, holding the same views, believe that the uterine engorgement and hypertrophy may be diminished by the use of the instruments,—a doctrine which I am convinced, on theoretical and practical grounds, to be wrong. M. Valleix's own experience, indeed, goes far to confirm this. For we find in his cases, that after removing the instruments, if the displacement backwards or forwards were rectified, it was only to be supplanted by displacement downwards. After "redressement," says M. Valleix, "there remains a certain amount of depression." And again we find this author stating that after the removal of the instrument a certain amount of engorgement remains, the removal of which is necessary for the relief of the original symptoms. "Then[3] (says he) the uterus, becoming smaller, remounts to the place which it ought normally to occupy, and there remains no trace of the uterine lesion. The diminution of volume of the uterus under the influence of its new position takes place generally sufficiently rapidly; but in cases where the engorgement was very great, four, five, and six months have been necessary for obtaining this desirable result." "The proof (he properly adds) that in this case the cure is really complete, is the cessation of the pains and of all the other general or functional symptoms by which the malady made itself known."

---

[1] "More frequently (says Dr Simpson) they (congestion and hypertrophy of the uterus) are the effects of the retroversion." Dublin Quarterly Journal, May 1848.

[2] Deviations Uterines, p. 166. [3] Ibid., p. 167.

If we further remember that M. Valleix's cases were poor women coming into hospital with chronic inflammatory uterine complaints, that in hospital they enjoyed good general medical treatment, diet, and regimen, with the advantages of continued recumbency, it will be difficult not to conclude that the cases would have reaped as much advantage from these alone without the instruments,—especially if we observe that in these cases the instruments produced the ordinary amount of inconvenience and injury attending their use, that after their use the symptoms for the most part remained, and ultimately were relieved by the ordinary treatment of chronic uterine inflammation.

My own experience as to the curative effects of these instruments has not been favourable to their use. In many cases, especially those characterized by vaginal relaxation, the displacement has returned as soon as the instrument was removed. A case of this kind was lately under my care, where for very aggravated symptoms of chronic uterine irritation, with retroversion and extreme vaginal relaxation or paralysis, the intra-uterine pessary was tried on several occasions. During its use none of the symptoms were relieved, but the reverse. On the last occasion it was retained with comparatively little annoyance for ten weeks, but immediately on its removal the displacement returned In other cases where vaginal relaxation was not so strongly marked, the displacement may have continued rectified, but only for a short time, and that without corresponding relief of symptoms.

Cases, however, do occur where the displacement continues rectified after the instrument has been retained for a longer or shorter time; such cases are however rare, and do not belong to that class of cases of chronic uterine disease which forms almost the entire category of cases difficult of cure, with or without these instruments.

The cases curable by pessary may be those of uterine softening and atrophy, in which the irritation induced by the instrument may be sufficient to produce engorgement or irritation, sufficient to prevent the recurrence of flexion or other such displacement, after the instrument's removal.

Before leaving the consideration of the use of these pessaries, I must state that there can be no doubt cases have occurred in which their use has been beneficial. From what has already been stated it is evident that they can be of no great advantage to the great mass of cases of displacement, whatever may be the cause. But I have seen examples, and heard of more, where irritation of the rectum, and still more of the bladder, concurrent with displacement, have been so great as to render their relief by removing the pressure of the uterus a great boon—even at the expense of the inconvenience and irritation induced by the instrument. That there may be other peculiar circumstances under which a careful trial of such instruments is justifiable, is quite possible, but at present no farther precise indications for their use can be given. M. Valleix[1] ascribes the general symptoms of displacement to the pressure of the uterus upon the neighbouring organs, because, says he, they are relieved on rectifying the organ. But there are good reasons for rejecting the premises as well as the conclusion.

Cases of uterine disease complicated with displacement are of various kinds. In discussing the symptoms (see p. 337) we have divided them into two great classes,—first, those characterized by relaxation of the vagina ; and, secondly, those characterized by uterine enlargement. In practice, however, the characteristics of these two classes are often found combined. And in a therapeutic point of view, cases of uterine disease with displacement admit of a simple classification, viz., into those that are curable without much delay or difficulty, and those which are not so. In speaking of a cure in such cases, it is not to be supposed that we imply rectification of the position of the uterus, but only the dispelling of the painful symptoms.

The obstinate cases which are so frequently met with, searching for relief in the various metropolitan cities of this and other countries are well known to most obstetricians. In some the main disease is menorrhagia or dysmenorrhœa, in others the chief complaint is sterility, in others recurrent abortion, and in a large class there is inability to walk, with aggravated lumbar and pelvic pains. In the worst of these patients there is generally found in addition a shattered state of the general health. In this class of cases, if the uterine disease is the chief malady, we generally find the body of the womb in a state of chronic inflammatory engorgement, and often also of hypertrophy, and the vagina in a state of relaxation. They frequently can be traced to an incomplete recovery from abortion or delivery at the full time, or to a recurrence of abortions, or to pregnancy and delivery repeated with short intervals in women whose constitutional strength is unfit for the task.

---

[1] Deviations Utcrines, p. 152.

The cases which admit of rapid amelioration are generally of recent occurrence, and owe their origin to an incomplete recovery after abortion or confinement, to temporary disorder of the menstrual function, but most frequently to disease of the upper part of the vagina, or cervix-uteri, parts which are easily accessible for treatment, and whose affections are comparatively easily removed.

It would occupy too much space to enter upon the various methods of treatment, applicable to the numerous different conditions of the vagina and uterus, connected with displacement. Our object has been to attempt a correct appreciation of the value of the phenomenon of displacement in these cases, and to point out its real causes. Besides, the treatment of leucorrhœa in its various forms has of late years been so copiously and ably described, that no apology is required for silence on the subject.

In the course of this article we have discussed all the minor displacements together, believing that any separation of them would lead only to artificial distinctions, and to useless repetition. We have already stated that the aggravated cases of displacement are generally retroversions or retroflexions, and attempted to explain why hypertrophy of the body of the womb should lead to this form of dislocation. Before concluding, we shall make a few remarks upon the treatment of the chronic inflammatory condition which lies at the foundation of the malady.

In this, as in many other chronic complaints, the treatment resorted to must be modified, changed, given up, and resumed, according to a variety of circumstances, especially the state of the constitutional strength and of the general health. In the case under consideration, nothing is more frequent than an extremely depraved and depressed state of the latter, revealing itself in all the varied forms of dyspepsia and hysteria. The treatment applicable to these various states we must entirely omit. But it must never be forgotten that the restoration of the general health forms one of the most important elements in successful treatment, and regard must constantly be had to it in pursuing therapeutic plans in regard to the uterine disease. This remark holds good especially in regard to position and bleeding. The former is one of the most valuable palliatives of the complaint, and contributes greatly also to cure. Many females suffer so much from the erect position in this disease as to be unable to maintain it with comfort, even for the shortest time. In such cases the recumbent attitude is not only expedient but necessary. In all cases it should be resorted to as much as possible. The chief effect of it is to relieve vascular tension. Its influence in this respect is abundantly proved in cases of another description, when there is a tendency to hemorrhage. In these the erect position always aggravates or induces the bleeding. Moreover, it will be remembered, in illustration, that the "getting up" after delivery is generally accompanied by the appearance of colour in the discharges. The only danger in regard to recumbency is, that it may be continued to the injury of the general health, from the want of exercise and of exposure to the open air.

For the purpose of removing inflammatory engorgement two of the most useful remedies are counter-irritation and local bleeding. The former may be effected by means of any of the ordinary irritant ointments or liniments. The use of large cantharides blisters is contraindicated, on account of its tendency to induce vesical irritation. The means which have been most useful in my hands are the croton-oil liniment and tartar emetic ointment. By means of these a pustular eruption or issue may be kept up over the hypogastrium for a length of time.

In cases where the chief morbid phenomenon is the inflammatory uterine engorgement, nothing gives more relief or is more beneficial than the use of local bleeding. This is best done by applying leeches directly to the uterus through the speculum, or to the vagina by an ordinary leeching tube. The misfortune in regard to this is, that from the first the patient may be unable to bear loss of blood; or partly from the loss of blood by the repeated leeching, partly from imperfect digestion, a state of anemia is induced before a cure is effected, and thus the practitioner is deprived of his most valuable resource. To prevent this it is necessary to inculcate the propriety of taking as little blood as possible at one time, for very little is necessary to disgorge the vessels and produce relief. It is necessary also to prevent the female from assuming the erect position for some time after the bleeding. Practitioners, I am sure, frequently err in applying a large number of leeches, and after their removal encouraging the bleeding by placing the patient in a sitting position over hot water, or in it, In this way I have sometimes seen women prostrated by the loss of blood at a single bleeding. Now the repetition of the bleeding in the intervals of the menstrual periods is useful, and to allow of this a small bleeding is sufficient, by means of a couple of leeches. Generally, after these come away, several cloths are required for the spontaneous discharge from the leech-bites, and this amount is fully sufficient. In lieu of the internal we may resort to external leeching in the perineum or groins.

Vaginal injections are also often of service. They may be emollient and sedative, to remove irritation and pain. But they are of more service against the vaginal relaxation. For this, mild astringent lotions of solutions of alum, or of sulphates of zinc, or acetate of lead, may be freely used with an ordinary Reid's syringe. But nothing is more serviceable than the vaginal douche of cold water used daily (avoiding the proximity of the menstrual period), and for ten or fifteen minutes at a time.

Sometimes it is useful to establish an issue upon the cervix uteri by deep cauterization with potassa fusa, or potassa cum calce. The derivative action of this upon the body of the uterus is sometimes effectual when other means fail. M. Amussat and Dr Simpson have recommended the formation of the issue upon the posterior lip of the uterus, for the mechanical benefits expected from the contraction of the resultant cicatrix. The internal remedies of most service in cases of chronic uterine engorgement and inflammation are iodine and mercury. The latter has been especially recommended by Dr Oldham in the form of minute doses of the bichloride continued for some time. The former has received the special commendation of Dr Ashwell, who uses it also locally, both to the os uteri and over the hypogastrium, in the form of ointment or tincture.

Lastly, the abdominal supporter of Hull is often of great service, relieving the pelvis of much of the weight and pressure of the intestines, and diminishing or removing the painful sensations of bearing down,—a result which is promoted by the upward pressure of the perineal pad generally attached to the instrument. By wearing this support, women often recover not only feelings of comfort and security, but the ability to take healthful exercise.

The occurrence of pregnancy is often a cause of anxiety in cases of chronic uterine disease such as we have been describing, from the danger of abortion occurring. But if the female goes to the full time, and is carefully managed after confinement, it often forms the happy termination of her sufferings.

(The second part of Mr Hare's paper is unavoidably postponed.)

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *A Treatise on Diseases of the Heart.* By O. B. BELLINGHAM, M.D. Dublin, 1853. 8vo, pp. 252.

2. *The Diseases of the Heart and Aorta.* By WILLIAM STOKES. Dublin, 1854. 8vo, pp. 689.

If we compare the earlier literature of such a subject as gout, or indigestion, or liver disease, with that which treats of the diseases of the important organs by which circulation and respiration are maintained, we are struck with the comparative meagreness of the latter. And although, since the discovery of Laennec, a new stimulus has been given to the investigation of all thoracic disease, yet the Lungs have benefited by this much more than the Heart.

It is indeed true that the admirable talent for observation with which Laennec was gifted, which displayed itself no less in his zealous prosecution of morbid anatomy than in his investigation of the signs and symptoms of disease, enabled him to present to the profession a vast material for future writers; but still, judging by what has been since accomplished, he has left more to be done by his successors in the investigation of diseases of the heart than in those of the lungs.

Of late years our knowledge of the pathology of this important class of diseases has undoubtedly been advanced, and several points of doubt and difficulty have been cleared away; but still it can scarcely be said that our knowledge of these diseases is complete; indeed it is scarcely equal to that which we possess on many of much less importance.

Before the time of Lancisi heart affections seem to have attracted but little attention, and he first pointed out the fact, subsequently pursued with his accustomed zeal and caution by Morgagni, viz., that many affections of the breathing, formerly attributed to disease of the lungs, or to asthma, were in reality due to affections of the heart. About the time of Morgagni (1749), Senac published the first separate treatise "On the Diseases of the Heart."

The method of percussion originally introduced to the profession by Avenbrugger, though soon presented to the world through the medium of a French translation of the original work, never attracted much attention until the publication, in 1811, by Corvisart, of his work on the heart, in which, while specially directing attention to that means of diagnosis, he exhibited particularly the results which it had accomplished in his hands. To him we are indebted for the foundation of much of our present information on pericarditis and contraction of the valvular orifices; and his treatise first awakened attention to a class of affections to which the subsequent investigations of Bertin (1822), Testa (1823), Burns and Kreysig (1814), gave precision and value. Seizing and turning to advantage the discoveries of Laennec, with all the power of an original and most ardent mind, Dr Hope, first in the pages of the Cyclopædia of Medicine, and subsequently in his separate work, did much to advance the study of these diseases, and presented to the profession what has for years been regarded as the standard authority on the subject. Nor should the name of the veteran Bouillaud be forgotten in this hasty sketch. First appearing as joint author with Bertin, he has since published his Clinical Treatise on Diseases of the Heart (Paris, 1835), which has done much to clear up many previously doubtful points in pathology. Perhaps the length to which he carried his *coup sur coup* treatment, and his heresies in pathology, have prevented this work from being valued as it deserves. Lastly, the names of Williams, of Latham and Walshe, of Corrigan, Pigeaux, and Carlisle, will occur at once to our readers as zealous cultivators of this department of practical medicine. And with these names before us, we are not likely to forget what we owe to the Dublin school of medicine, from which has emanated both the treatises the names of which stand at the head of this article.

For some time past the profession has been led eagerly to anticipate a work on this subject from Dr Stokes. Attached to a large hospital at a period of life when, in the full vigour of youth, he could devote nearly all his energy and time to cultivate the wide field of observation which it afforded, Dr Stokes, by his many excellent clinical reports, which have appeared at intervals during the last thirty years, by his valuable practical treatise on diseases of the chest, and by his varied isolated papers on the diseases of the heart, acquired a European fame, and secured a welcome reception for any practical work he might publish; a reception which, we are sure, will appear amply justified by a perusal of the present volume. The student will certainly not find in it a systematic treatise on diseases of the circulating system. It does not aim at this. But the physician who has felt at the bedside the difficulties of diagnosis and treatment will best appreciate its value, and will be prepared to render homage to the thoroughly practical character which stamps its every page. It is eminently *factual* in its character, and yet it is no mere register of facts, strung together in unmeaning sequence, or to accommodate some arbitrary system. In the true spirit of inductive philosophy, the author has taken a higher range; with him facts are but the instruments which he uses in high philosophic generalizations. The passion which a few years ago existed among a certain class of pathologists, for degrading medicine altogether from the rank of a science, and for reducing it to a mere *catalogue raisonné* of barren facts, is passing away; its zealous cultivators are becoming every day more aware, that just as the observation of the stars could never of itself have constituted the science of astronomy, so the mere recording of facts can never form a system of medicine; and although there are still to be found, in every medical school, men of quick observational powers, but utterly destitute of the higher qualities of mind—mere anatomists, mere histologists, mere stethoscopists,—yet these are every day losing the prestige which they once enjoyed, and giving place to the men who possess not merely an ordinary capacity for the observation of fact, but the rarer gift of inductive power. In fact, Dr Stokes, more than any living author, with the exception, perhaps, of Sir H. Holland and Dr Latham, possesses the qualifications so well described in the following passage, which every one who is ambitious of extending the domain of scientific medicine would do well to read and ponder:—

> "Medicine is a progressive science, and the pathological anatomist is truly the pioneer of the physician; and the latter, if he desires to advance his science, must train his mind to a higher exercise and a more comprehensive thought than that required for the observation and recording of isolated facts in pathology. He must not only give due weight to the existence of separate phenomena, and to the results of combinations of diseases, but he must study conditions which are beyond the reach of the pathological anatomist, and learn to

ignore the existence even of manifest organic alterations. While he bears in remembrance the ascertained facts supplied by modern investigation, he is to go beyond them, having an eye to the general, rather than the particular, and to found his diagnosis, prognosis, and treatment on that broad basis which includes, not alone the effects, but the causes and special modifications of disease. Thus only can the new medicine be advantageously combined with the old. Thus only can that kind of skill which is, as it were, the instinct of the thinking man, be produced—the true *mens medica*—enabling us to act rapidly and successfully in opinion and practice, unconscious, too, of any effort of memory or complicated reasoning process. This is the great desideratum in medicine, when we are called on to deal with varying combinations of local diseases, and general morbid conditions. Accurate observation and accumulated experience are necessary for its attainment; and yet it is more accessible to some than to others. A few, indeed, seem incapable of acquiring it; and hence it is that, with a more limited experience, the mind of one man shows more brightly than that of another. In medicine we have to deal with ever-varying phenomena; the vital condition of organs cannot always be inferred from their physical state, nor the influences which act in the entire economy be explained by anatomy. Were it otherwise, medicine would be an easy pursuit, but no field for this exercise of mind which is at once its difficulty and its glory."—(Pp. 342, 343.)

It is thus that, as Dr Stokes himself explains in the preface,—

"I have sought to give to this work an essentially practical character; and at the cost of omitting much of what is new and interesting, I have made use of pathological anatomy, and the physical diagnosis founded upon it, only so far as these subjects bear on the everyday practice of our profession. The work, then, is not intended as a full treatise on cardiac pathology, nor yet on physical diagnosis, but it aims at the rational application of these branches of knowledge to practical medicine. Such a book ought to be useful, and is perhaps required; and whether the present attempt be successful or the contrary, my readers may be assured that few of them will be more convinced of its imperfections than I am myself."—(Preface, pp. 9, 10.)

Dr Bellingham, the author of the other treatise at the head of this article, has not yet achieved the world-wide reputation of Stokes, but is favourably known to the profession by his modest and unpretending, but valuable paper on the treatment of aneurism by compression.

This treatise on the heart is not entirely new, much of it having already appeared in the form of clinical lectures delivered at St Vincent's Hospital, and published in the weekly journals. They have been republished, we are told, by invitation; and have been carefully revised, added to, and in part rewritten. The object the author has kept in view is as different as possible from that of Stokes. With infinitely less experience, and therefore with infinitely less of his own to communicate, he has attempted a complete treatise on the diseases of the heart, embodying the best received opinions of others, which he has brought to the test of his own observation and experience. The work is intended to consist of two parts, of which we have as yet only received the first, and which

> "Contains a description of the healthy heart, its size, weight, and the measurements of its chambers and orifices; followed by sufficiently full details respecting its motions and sounds. The examination of the heart in disease is then entered upon, commencing with the physical signs; the general signs, and the secondary or remote symptoms of cardiac disease are described in succession."—(Preface, pp. 7, 8.)

In the second part Dr Bellingham intends to consider the individual diseases of the heart. On the whole, the work, as far as it has gone, is worthy of the school from which it proceeds, is admirably suited to the use of students, for whom it was originally prepared, and will form an excellent introduction to the less systematic, but more practical work of Stokes.

After these preliminary remarks we shall endeavour to present our readers with an analysis of the contents of these volumes, in which we shall follow the order adopted by Dr Stokes, only referring to Dr Bellingham where we think he can advantageously supplement what we cull from his colleague.

The first chapter of the work of Dr Stokes is devoted to *inflammation of the heart and its membranes*, and in it the subject of *pericarditis* is discussed at a length due to its importance, occupying the first 96 pages.

The rarity of inflammation of the muscular substance alone (*myocarditis*), leads to its being very shortly noticed, while endocarditis, apart from valvular disease, is considered to be generally associated with pericarditis.

Following the plan indicated in the extract from the preface, Dr Stokes plunges at once into the heart of his subject—the varieties of the disease as indicated by its symptoms,—varieties too, not originally of pathological import, and founded merely on the intensity of the affection. And yet with what interest do we observe that the degrees of what, for want of a better expression, we may call clinical intensity, correspond with the degrees in which the inflammatory products are developed. The three forms are thus tabulated by the author, and he shews that as there is a progressive scale in the intensity of the symptoms, so there is a progressive scale in the pathological lesions; the great characteristic of the second form being the effusion of fluid, while that of the third is inflammatory irritation of the muscles, inducing often paralysis of the left ventricles, and thus causing death from syncope.

First Form.	*Second Form.*	*Third Form.*
Absence of pain or local suffering frequent. No sign of muscular excitement, nor any special character of pulse. No increase of dulness over the heart.	The local and general symptoms more decided though often very trifling. Irregular action of the heart and pulse, often more manifest in the advanced periods. Remarkable increase of dulness over the heart.	Local distress, often extreme, even at the outset. Tumultuous action of the heart. Irregularity of pulse. Dyspnœa, orthopnœa, œdematous swellings, syncope, death.

(P. 4.)

Of the peculiar and orginal seat of the disease not a word is said; the structure of the pericardium is not alluded to, and the interesting investigations of Dr Robert Lee (Phil. Trans. 1848), which by demonstrating the *cardiac fascia* shewed the alliance between rheumatism of the heart and that of the muscles, and directed attention to the interesting fact that in both the seat of the disease is in all probability the aponeurotic fascia, and not the muscular fibre, are not referred to—all this is, however, most fully treated of by Dr Bellingham.

Dr Stokes is at some pains to explain that death in pericarditis is not caused, as is generally supposed, by the pressure of effused fluid, but where it does not arise from some complication, it is due either to paralysis of the muscles, consequent on irritation, or to myocarditis with destruction of the muscular fibre. On the frequency of complications, and their dangers, Dr Stokes dwells at considerable length, and presents (page 6) a table of complications divided into two classes—"1*st*, complication with general diseases; 2*d*, complication with one or more local diseases of structures unconnected with the heart." At the head of the first table stands rheumatic fever, while to our surprise, among the seven diseases of which it consists, no mention is made of Bright's disease. This indirectly confirms a suspicion which we have more than once entertained, that granular degeneration of the kidney is by no means so common in Dublin as it is found to be in London, Paris, and Edinburgh. Bright's disease was found by Professor Taylor to be the cause of pericarditis in more than one-third of his cases. If we except acute rheumatism, Bright's disease produces pericarditis 2½ times as often as all other diseases put together. But we suppose that, in Dr Stokes' table "Dropsy" must stand as the representative of this formidable complication. In passing, it may be remarked that all diseases complicating pericarditis seem to be divisible into two classes—1*st*, diseases having a tendency to alter the character of the blood; and, 2*dly*, diseases of a similar type, transferring themselves, in obedience to metastasis, not in the vulgar sense, but agreeably to those laws which have been such fruitful subjects of discussion from the time of John Hunter.

The review of the symptoms of pericarditis, to which Dr Stokes next directs attention, will prove as disappointing to the student of systematic works, as it will be consolatory to the conscientious practitioner. Those doubts and uncertainties with which the latter is so often visited, troubling his mind and obscuring his judgment, have no place in these routine works. In them symptom after symptom is set down in logical order, formidable array, and with clear precision, so that the wonder comes to be with the student, as he rises from their perusal, how any one should ever mistake a disease at all. It is the peculiar comfort of such treatises as those of Dr Stokes, to give us a faithful transcript of the real state of mind of a practitioner, as he comes by slow and painful experience to be convinced that disease is not a unity, or separate existence which can be accurately defined or rigidly noted, but is a combination of ever-varying phenomena, so that, as Grimaud (*Cours de Fièvres*, t. i., p. 2) has taught us, " properly to understand a disease, it must be studied in every age, in every sex, in every temperament, in every climate; wherever, in short, we learn by experience that it can occur." To those, then, who have been frequently puzzled by the symptoms of a disease which, in ordinary cases, would not appear difficult to detect, it is consolatory to find that as the result of his vast experience and unrivalled diagnostic powers, Dr Stokes has shewn that there is no one symptom the presence of which can be constantly relied on in pericarditis, and that he more than confirms the observation of Dr Mayne, " that in the present state of our knowledge there is no stethoscopic sign which can be considered pathognomonic of the first stage of pericarditis, which is the more to be regretted as this is, of all others, the period at which most benefit might be expected from antiphlogistic treatment." This however, as Dr Stokes shews, is the case in all acute thoracic diseases, where a physical change must have occurred before a physical sign can be produced.

From this fact, however, our author deduces two important practical lessons.

1*st*, That in the early stage of suspected pericarditis the absence of friction sounds must not lead us to conclude that the pericardium is safe.

2d, That in violent and dangerous pericarditis, special signs will generally appear to guide us, before the friction sounds are audible.

The following is the classification given of the physical signs of pericarditis :—

"*First.*—Sensations of friction communicated to the hand. To these we may give the general term of tactile signs.

Second.—Friction sounds; the '*attrition murmurs*' of Hope.

Third.—Extension of dulness over the heart, resulting from liquid effusion.

Fourth.—Friction signs attended with or preceded by valvular murmurs.

Fifth.—Signs of eccentric pressure analogous to those of empyema.

Sixth.—Signs of excitement of the heart.

Seventh.—Signs of weakness or paralysis of the heart.

"It may be stated generally, that the tactile and acoustic signs vary according to the following circumstances:—

(1.) The state of the effused lymph.
(2.) Its extent.
(3.) The existence or non-existence of fluid.
(4.) The advance or arrest of the progress of organization.
(5.) The process of obliteration of the cavity.
(6.) The repetitions of inflammation.
(7.) The existence of air in the pericardial sac.
(8.) The distension of the stomach with air.
(9.) The combination with pleuritis of the left lung.
(10.) The force and volume of the heart.
(11.) The combination with recent or previously existing disease of the valves."—(Pp. 15, 16.)

In regard to the most important of these—the friction sounds,—for the elucidation of which the profession are chiefly indebted to Dr Stokes,—a mere hint of their existence having been previously given by M. Collin (*Les diverses methodes d'exploration de la Poitrine*, 1824),—we may present the following "conclusions" from the work of Dr Bellingham :—

"*Conclusions respecting the pericardial friction sounds.*

When I come to speak of pericarditis, I shall have occasion again to recur to these sounds. It will be sufficient here to observe :—

(1.) That they are almost limited to cases of inflammation of the pericardium; that they are pathognomonic of one form, and constitute the most valuable diagnostic signs of it.

(2.) That they have, more or less, a friction or attrition character.

(3.) That they are generally double, and are sometimes louder during the diastole than the systole of the ventricles.

(4.) That they appear to be superficial and near, and are seldom udible beyond the limits of the præcordial region.

(5.) That they do not, like the endocardial murmurs, replace the ordinary sounds of the heart, but are independent of them.

(6.) That their duration is usually short, frequently ceasing entirely after having been heard for a few days, and not unfrequently changing their character or seat within the period that they are audible.

(7.) That they are frequently accompanied by a peculiar vibratory thrill, sensible to the hand laid upon the parietes.

(8.) That a *bruit de soufflet* at one of the orifices of the left side of the heart (the result of endocarditis) not unfrequently coincides with these sounds."—(P. 136.)

The most intense friction sounds are, as Dr Stokes explains, found under two conditions.

1*st*, Where there is great induration of lymph.

2*d*, Where the surface is very dry.

In certain cases where doubt exists as to the nature of the sounds, they may be rendered more audible by pressing firmly with the head on the earpiece of the stethoscope, or by causing an assistant to make firm pressure with the hand over the cardiac region during the application of the stethoscope. This method, long practised by Dr Stokes and others, has been recently appropriated as a discovery by Sibson.

Dr Stokes is disposed to think that the difficulty of establishing a diagnosis between pericardial friction and valvular murmur has been overrated, and recommends us to depend on the following circumstances:—

> "The actual acoustic character of the signs; on their diffusion or concentration at a point of greatest intensity; on their being superficial or deep-seated; on their amount of extension over the thorax; their double or single character; their transmission or non-transmission along the course of the vessels; on the presence and character of the tactile signs; on their constancy or variability in character and seat; and on the effect of treatment in their modification."—(Pp. 38, 39.)

Notwithstanding the value of these rules, and the confidence with which the possibility of making the diagnosis is stated, we are somewhat inclined to think that its difficulties are underrated, and some of Dr Taylor's cases are pregnant examples of the almost impossibility of distinguishing the two, even with the admirable rules laid down in the work before us. Dr Taylor, indeed, gravely admits that there are cases where he has been unable to determine the true seat of the morbid sound; and we have heard of strange mistakes in this respect, made by those who pride themselves most on their auscultatory powers.

Dr Stokes, like all other recent writers on this disease, insists on the importance of percussion, and the value of the results which it yields, both negatively and positively. He agrees with Dr Taylor in his observation that the dulness cannot, from its mere character, be distinguished from that of empyema or consolidation of the lung, and that its origin can only be determined by the circumstances which precede and accompany it. In regard to what we are inclined to consider the much more difficult diagnosis between dulness arising from hypertrophy and that from pericardial effusion, we are given only the old and not very well established observation of Hope, that the dulness of effusion mounts higher up in the direction of the large vessels. Dr Taylor is much more clear on this subject than any writer whom we have consulted, and he lays down the following rules for the distinction :—

> "1*st*, That in pericardial effusion, the dulness extends *upwards* to the level of the second rib or clavicle, and but little below the healthy limits *downwards*. 2*d*, It extends farther to the right side than that of hypertrophy of the left ventricle, and farther to the left side than that of hypertrophy of the right ventricle. 3*d*, It changes in its extent from day to day, and sometimes rapidly."

Where time is given to watch a case, the rapid progress of effusion, as contrasted with the slow growth of hypertrophy, affords a good ground of distinction.

In speaking of the vital symptoms and history of pericarditis, Dr Stokes adopts the practical division into three classes.

1. Latent and trivial.
2. Latent and dangerous.
3. Manifest and dangerous.

In regard to the first form, he is disposed to consider pericarditis as "one of the most frequent of the unrecognised and often harmless diseases which affect the human body." The stethoscope is the only means of detection, and Dr Stokes is inclined to regard the white spots as the results of attacks of the first class, distinguished as they always are by "localization, absence of essential disease, and, lastly, a slight or feeble inflammatory action."

The second class he subdivides into complicated and uncomplicated. The frequency of the occurrence of the latent form with articular rheumatism leads to this rule being laid down, "that in any case of acute articular rheumatism, we cannot be certain that the heart is safe, unless by the careful employment of the stethoscope." But we must not forget to put alongside of this fact another one to which no allusion is made by Dr Stokes, but which is, we think, amply warranted by the memoirs of Dr Taylor,—that there are some cases of pericarditis, in which no physical signs are apparent, though dissection has afterwards revealed conditions leaving no room to doubt the previous existence of an attack. The general condition in Dr Taylor's cases on which this seemed to depend was the uniting of the two surfaces by old adhesions, and probably similar cases had suggested to Dr Latham to state, "in the first inflammation of the sound heart, they (the physical signs) were everything; in all after inflammations of the unsound heart, they are nothing." Dr Stokes dwells upon the necessity of considering rheumatism as an essential disease not necessarily co-existing with arthritis, and thinks pericarditis more allied to the essential state than to the mere local affection. In proof of this he refers to two cases where the heart affection preceded that of the joints. This observation would appear to have been anticipated by Professor Taylor, who gives several cases of the same kind, as well as of rheumatic fever without arthritis, and who refers in particular to one, where he thinks it probable that there may have been acute rheumatism, and in which there was pericarditis, but no affection of the joints throughout the entire case.

The description of the *general symptoms of pericarditis*, apart from rheumatism, is ushered in by the following truthful and refreshing remarks, worthy of being well weighed and pondered by every student, who, fresh from systematic teaching, regards himself as armed *cap-a-pie* to detect and encounter every possible variety of disease.

> "The system so long adopted by writers on medicine, of specifying a group of symptoms as indicative of a particular disease, has led to errors in diagnosis and practice. Hence, in attempting to describe or enumerate the symptoms of pericarditis, it must be understood that none of them are constant; and, further, that there may be great variation in the mode of succession of the phenomena in different cases."—(P. 49.)

The symptoms then enumerated at length are pain, epigastric tenderness, dyspnœa, character of pulse. Two cases are quoted from Testa, where dysphagia was the most prominent symptom. Dr Stokes has observed dysphagia in other thoracic inflammations, and is disposed to regard it less as the result of any mechanical condition, such as pressure on the œsophagus, than of some excited irritability, either of that tube or of parts immediately in contact with it.

The pain accompanying pericarditis has appeared to us to be more often felt between the scapulæ, or under the left scapula, and shooting down the arm, than in the præcordial region: this, however, is not observed, though its frequent entire absence is noted.

In regard to the pulse, an observation is made which will be concurred in by all who have had any experience in the disease.

> "Great stress has been laid on the character of the pulse in this disease, yet clinical experience establishes that no special condition of pulse can be described as belonging to any one form or stage of the affection."—(P. 51.)

To another remark we must also direct attention, as forming an interesting subject for future investigation.

> "There is a symptom in this disease referable to the arterial system, which, though of great value, has been unnoticed, namely, an increased action of the cervical vessels. As to its actual frequency I cannot speak positively, but I have observed it in two remarkable cases."

* * * * * *

> "In both these cases there was valvular murmur, and it is worthy of inquiry whether this increased action of the carotids may prove available in determining the presence or absence of endocarditis in such cases. But it may be laid down, that if the symptom be recent, and the constitutional state indicative of irritation or inflammation, this visible pulsation in the arteries of the neck, while the remaining vessels act feebly, should lead us to suspect some form of carditis."—(P. 52.)

In four classes of cases is this symptom met with—

1. Concussion of the brain (Sir A. Cooper.)
2. Earlier stages of permanent patency of aortic valves.
3. A special form of chronic disease attended with palpitation of heart, enlargement of thyroid gland and of eye-balls.
4. The cases of pericarditis alluded to.

Treatment of Pericarditis.—While the modern practice of medicine would seem to discard, in a great measure, the large, and to our ideas, terrific bloodlettings which our forefathers indulged in, there are one or two diseases in which such treatment still lingers. One of these is pericarditis, and perhaps a chief cause for the activity with which it is usually treated is the example of Dr Hope, whose immortal work is still the chief text-book among British practitioners. Not that this eminent man was by any means an indiscriminate advocate for the lancet in all and every case of pericarditis, but still he does advocate an active and strenuous use of it in the main, and the young practitioner is too apt, in the hurry and anxiety of an urgent case, to remember only the chief directions, and to forget the numerous qualifications and cautions with which they are propounded. Dr Stokes furnishes us with some statements which may well make us pause before carrying out even the modified activity of Hope.

> "It is my conviction that the fatal result of some cases of pericarditis is mainly attributable to the perseverance, beyond the proper time, in the antiphlogistic treatment; the practitioner looking at the disease

> merely as a case of serous inflammation, and forgetting, not only the results of irritation on muscular fibre, but the effect of great losses of blood in producing reaction."—(P. 83.)

And again, a few sentences previously:

> "In such cases the boldness of treatment often betrays the timidity of the practitioner; he is terrified at discovering the disease, and his mind is more occupied with its name than its nature or actual condition. In this way great mischief is done, for the debility thus produced disposes the disease to change from the dry and comparatively innocuous form, to an unhealthy inflammation, attended with liquid effusion."—(P. 82.)

Now, let such a statement as the foregoing be followed by the "sturdy" facts furnished by the memoirs of Dr Taylor. In four cases pericarditis got well without treatment, being in one instance complicated with dysentery. "Both diseases," says Dr Taylor, "had been severe, yet both, I think, had been cured by the powers of nature. *Such cases are very common.*" And the result of the statistical researches of the accurate Louis was, "that pericarditis entirely uninterfered with, does not prove fatal in more than one in six cases."—(Mem. Anat. Pathol., p. 229.)

After this, consider the treatment of Bouillaud. Within the first three or four days three to four venesections to ℥xii. or ℥xvi. each, together with twenty or thirty leeches, or cupping, to ℥viii. to ℥xii. This conjoined with low diet, setons, issues or moxas!!

Our author judiciously rules as follows:—

> "In a case of uncomplicated pericarditis in its earlier stages, and occurring in a patient whose strength is but little impaired, a single bleeding from the arm appears, on the whole justifiable, but its repetition will be a matter for careful consideration. Under these circumstances we must examine the force of the heart, not only as indicated by the pulse at the wrist, but by the actual strength of the impulse, and the character of the first sound especially. If the impulse continues vigorous, and the first sound undiminished, we may be less apprehensive of the use of the lancet. On the other hand, if, after depletion, the impulse has manifestly declined in force, while the first sound is lessened, great caution must be used before we repeat the general bleeding."—(P. 84.)

And again—

> "But our great reliance is to be placed on local bleeding, and the best mode appears to be the employment of leeches, in relays, beginning with twenty or thirty, and gradually reducing the number on each application. Two or three applications may be made in the twenty-four hours, a warm poultice being employed during the intervals."—(P. 85.)

Like Dr Graves, and the Dublin School generally, he strongly advocates the induction of mercurialization, and prefers the method first advocated by Dr Johnson, of giving scruple doses of calomel once or twice daily.

Now, we have very great doubts, indeed, of the existence of any necessity for so poisoning the system, either from the difficulty of curing pericarditis without it, or from the evidence of its success even when employed. M. Valleix, in his *Guide du Medecin Practicien*, quotes Dr Hope's opinion in favour of mercury, at considerable length, and then sets over against it that of M. Gendrin, who denied that mercury finally was of any use whatever, and sums up with the pertinent question—"How are we to decide in this clash of opinions." Our reply to this question of M. Valleix would be, "We are not yet in a condition to decide at all." And yet, we fear, neither Dr Stokes nor his late lamented colleague, Dr Graves, had succeeded sufficiently in shaking off the influence of authority, to weigh impartially the claims of mercury. Dr Latham, quite as great, and perhaps a more ingenious advocate for the use of this poison, interpolates two whole lectures (xiii. xiv.) into his work on the heart, to prove its antiphlogistic character, and yet, after all, there is a misty vagueness about the whole matter, not calculated to do much to convince an unprejudiced observer. After setting up a high claim for its powers in the general, he drops at last to this small demand in the particular:—

"These cases claim for it a reparatory, not an antiphlogistic power. They do not satisfy us that they had anything to do in counteracting the progress of the inflammation. They only shew us that it came in aid of nature in restoring the endocardium to its integrity after the inflammation had ceased." The result of Dr Taylor's cases tends to shake all confidence in the opinions usually held on this subject. He gives an analysis of his experience in thirty-six cases; and the general result would appear to be, that in one case only was ptyalism followed by speedy relief, in two cases by a diminution of symptoms, while in twelve there was no abatement; in six ptyalism was followed by pericarditis, while in others it was followed by pneumonia, pleuritis, erysipelas, and laryngitis. What little experience we have ourselves enjoyed has tended greatly to abate that confidence in mercury and heroic treatment generally with which we commenced practice. One by one those remedies whose effects were the most marked, and therefore the most trusted in, have been dispensed with; and in the rheumatic variety of the disease, we find as happy a result to follow the employment of depuratives, using the kidneys and the bowels as emunctories, and giving opium at the same time to relieve pain; occasionally a few leeches may be advisable, though even they may be dispensed with oftener than is generally supposed.

The view we have taken of this subject is strongly supported by a paper read before the Medical Society of London by Dr Risdon Bennet, and published in the *Lancet* (1851, p. 541). He gives a narrative of rheumatic pericarditis treated most successfully by lemon juice and purgatives.

The lemon juice, as Dr Owen Rees has well shewn, only acts as a supercitrate of soda; and we think it of little consequence what salt we employ, provided it act on the kidney. Citrate of potass, citrate of ammonia, acetate of ammonia, acetate of potass, nitrate of potass—all answer well.

We owe to Dr Taylor our knowledge of the interesting fact that the pericardial inflammation attendant on rheumatism is much less fatal than that which occurs without it; and Dr Stokes has thus shewn that practical sagacity which is so marked a characteristic of his book, in separating the treatment of the rheumatic from that of the other varieties; and very valuable are the conditions which he lays down as, among others, indicating suspicion of cardiac disease in a case of rheumatic fever.

> "(1.) Excitement of the heart's impulse, without any corresponding state of the pulse, unattended by endocardial or exocardial murmurs.
>
> (2.) Excitement of the heart and pulse, attended with a ringing sound of the ventricular contraction appearing for the first time.
>
> (3.) Sudden depression of the heart's action in force or rapidity. The first character may not be revealed by the pulse.
>
> (4.) Sudden irregularity, without any other morbid sign.
>
> (5.) Doubling of one of the sounds of the heart. This is not uncommon; it is much more frequent with respect to the second sound, and I have observed it to disappear on the patient assuming the erect position.
>
> (6.) Prolongation of the first sound. This sign appears to depend, not on any valvular affection, but on some altered condition of muscular contraction."—(P. 93.)

He adds little, however, to our knowledge of the treatment, unless perhaps the testimony of his experience against the use of colchicum.

On one point of treatment we are under deep obligations to Dr Stokes, for he appears to have been the first in a published volume to call our attention to the use of stimulants in this disease, though those who have followed the clinical teaching of Professor Alison of Edinburgh, and still more those who have been privileged to see any of his private practice, will not admit the novelty of the doctrine which is proclaimed in the following sentence:—

> "It may be laid down as a general principle that there is no local inflammation whatever, the mere existence of which should prevent the use of wine, if circumstances require it. In two cases especially, namely, cerebritis and pericarditis, we find the greatest timidity in practice with respect to the use of wine. Yet even in the first case it may be required, and in the second its employment is imperative, when, as too often happens, excessive depletion has been resorted to. Again, if the signs of muscular weakness, such as we have indicated, have appeared; if there be evidence that the heart, previous to the attack, was in a weakened state; and lastly, when a collapsed or typhoid condition of the system exists, we must give wine, quite irrespective of the physical condition of the heart. This may be done safely and with great advantage.
>
> "In the following case wine was employed with the best effects." —(Pp. 87, 88.)

With this quotation we conclude the subject of pericarditis. We have reviewed this part of the volume of Dr Stokes at great length, because we felt that the subject was one peculiarly his own; for next to the valuable paper of Louis, we were most indebted to the original papers of Dr Stokes (*Dublin Journal of Medical Science*, 1833–34, vols. iii., iv.) for our knowledge of this important disease. Our readers will see to what extent we have supplemented these observations by those of others, and especially by the clear, definite, and precise information which is derived from Dr Taylor's papers. That this lamented author is not once alluded to by Dr Stokes is a circumstance to us perfectly unaccountable; and we feel very sure that had Dr Stokes been familiar with his contributions, he would have brought to bear all the powers of his observant and philosophical mind on many of the points which he has too summarily handled. On the whole, were we from the first chapter to make any remark at all disparaging to a work which we prize so highly, it is that its author has most scrupulously avoided all notice of our current literature, and seems almost to have limited his acquaintance with his subject to a few chosen authors and the great book of nature. Yet this, though in some respects it enhances the charm of his work, in others becomes a serious fault, as we shall more than once have occasion to shew as we proceed.

Endocarditis forms a fitting transition from pericarditis to valvular diseases. Dr Stokes takes exception to the term endocarditis as used to imply valvular disease, or rather the state antecedent to valvular disease, holding that it is equally objectionable with the term gastro-enterite as applied to fever. We have no right, he thinks, to regard valvular disease as invariably the result of inflammation; it is rather to be considered "as an affection *sui generis*, into the treatment of which the question of existing inflammation does not necessarily enter."

The following are a few of the more important practical conclusions which the vast experience of our author has led him to adopt on this subject:—

> "(1.) That endocarditis is a disease more frequently met with in combination with pericarditis than as an isolated affection.
>
> (2.) That it may arise simultaneously with pericarditis, constituting the true endo-pericarditis; it may follow, or sometimes precede, the inflammation of the pericardium.
>
> (3.) That the tendency to its production in rheumatic fever must be considered less decided than that of pericarditis.
>
> (4.) That its symptoms can scarcely be said to differ from those of pericarditis.
>
> (5.) That there is no pathognomic sign of its existence.
>
> (7.) That where the symptoms of pericarditis are developed, but with absence of attrition sounds or evidences of pericardial effusion, we may make the diagnosis of endocarditis, especially if those be the recent development of valvular murmur.
>
> (12.) That although many cases of valvular disease evidently spring from an endocarditis, yet that we are by no means justified in attributing all valvular lesions to this cause, nor are we right in considering and treating such cases, even when they become chronic, as examples of chronic inflammation. It is true that the first morbid changes may have been inflammatory; but this state ceases, and is succeeded by new pathological conditions of deposition and transformation of tissue.
>
> (13.) That we cannot distinguish between endocarditis affecting the right side of the heart and that of the left cavities."—(Pp. 107, 108.)

In the few pages which are devoted by Dr Stokes to *myocarditis* we do not find much that is new. His remarks on polypi in the heart are meagre in the extreme, and far behind our present information. An interesting case is given of a coagulum following an attack of arteritis, and filling the common iliac and the greater part both of the external and internal iliacs.

Two important subjects are referred to in the Appendix—1*st*, *Doubling of the sounds of the heart*, on which Dr Stokes has the following remarks:—

> "The doubling of one of the sounds of the heart cannot be considered as any special sign of any of the forms of carditis, for we meet it in

cases of a different kind. It may be observed in nervous and chlorotic patients; and I have lately found it in the case of a man very far advanced in life, who was labouring under the symptoms of Peripneumonia Notha. It is, then, clearly only an indication of a special form of disturbance of the action of the heart. What its origin may be is difficult to declare; but that it is to be attributed to valvular rather than to muscular action appears more than probable. The greater frequency of its occurrence with the second sound, and the fact recorded in the last case of its disappearance in the erect position, seem to point to this conclusion. I do not know of any condition which would be adequate to explain the occurrence in question, except a want of synchronism in the action of the pulmonary and systemic portions of the heart."—(P. 119.)

2d, *Purulent cysts in the heart.*—Dr Stokes inclines to the doctrine that "coagula being formed from whatever cause, they become purulent, owing to the existence of a pyogenic diathesis," although he does not altogether set aside the view of Prof. Smith, which regards them as the result of a true cardiac phlebitis.

The next chapter, "ON DISEASES OF THE VALVES OF THE HEART," we regard as a peculiarly valuable one. Dr Stokes stands too high as an admirable stethoscopist, and is too well known for the carefulness of his diagnosis, to have any fear about confessing the impossibility of carrying the discrimination of minutiæ in valvular disease to the absurd length which some authors pretend. We know nothing more likely to prove injurious to the ardent student than the quantity of vapid trash which has for some time inundated our journals under the head of clinical lectures. A careful perusal of many of these will shew that the cases have evidently been "*cooked*," and the diagnosis been written after the *post-mortem* examination had been made. This manifest disadvantage arises from the fact, that a great portion of our medical literature is the production of men striving to write themselves into notice, and the fecundity of whose productive powers produces bantlings too weakly to dispense with artificial support. Reprehensible as such practices are in a moral point of view, this is not the only evil. These *impossibly* accurate details tend to damp the ardour and impair the accuracy of diagnosis of those who are not skilled in the secret of their production. They are led to long after unattainable perfection, and because they fall far short of it, they desist from every effort, and become careless and routine practitioners. But such maxims as the following from the pen of Dr Stokes ought to reassure them:—

"It too often happens, when the existence of a valvular disease is determined, that great labour is expended in ascertaining the exact seat and nature of the affection. Long and careful examinations are made to determine whether the disease exists at the right or left side of the heart; whether it be a lesion of the mitral, tricuspid, or the semilunar valves; a contraction or dilatation, an ossification, a permanent pateney or warty excrescence. Now, though in some, we might say in many cases, these questions may be resolved with considerable accuracy, it is also true that in a large number their determination is of comparatively trifling importance; and the two great practical points to be attended to are, first, whether the murmurs really proceed from the organic cause, and next, what is the vital and physical condition of the muscular portions of the heart; for it is upon these points that prognosis and treatment must entirely depend. There is, indeed, no other organ whose affections more fully illustrate the truth of this principle, that in dealing with the diseases of adjacent structures, diagnosis is easy where it is important, and of little value where it is difficult or impossible."—(Pp. 131, 132.)

The difficulties of the real diagnosis in valvular disease appears to be referable to one or other of two heads—

1*st,* The doubt still existing as to the real causes of the sounds of the heart;

2*d,* The rare occurrence of isolated lesions in chronic cardiac disease.

In regard to the first source of difficulty, our author deals with it with his usual practical sagacity. He begins by shewing that there are twenty-two (reducible, however, when the heart is regular, to eleven, from the simultaneous action of the pulmonary and systemic portions) physical circumstances taking place in the short interval of time occupied by each complete action of the organ; a consideration sufficient to make us pause before attempting to determine with too great precision what is the actual operation on which the sound depends.[1] Dr Stokes, for practical purposes, divides the sounds into two classes—

1*st,* Those produced while the blood is still in the cavity of the heart; intrinsic, arising probably from three conditions, the contraction of the muscles, the closing of the valves, and the current or wave of blood passing from one cavity into another.

2*d,* Those produced after the departure of the blood from the heart—extrinsic—arising from the action of the arterial valves.

It is evident that the first class of these will be strong or weak, according to the contractile power of the heart, which will not so powerfully affect the second, or extrinsic sounds, and thus, in the extract already given, Dr Stokes lays such stress on ascertaining the vital and physical condition of the muscular portion of the heart. Laennec, indeed, observed that valvular disease had but little influence on health, when the muscular condition of the heart remained sound, and this remark might be advantageously extended to many other cardiac affections,—pericarditis and endocarditis, for example, which are so often latent, until irritability of the muscle is excited.

---

[1] After summing up all the changes which take place during a ventricular systole, Dr Davies observes (Lecture xiv., page 231)—"And all these actions are commenced and terminated in a period of time, which, on the average, does not exceed the $\frac{1}{140}$th part of one minute, *i.e.*, $\frac{3}{7}$ths or less than $\frac{1}{2}$ of a second, supposing the pulse to beat at the rate of 70 times per minute."

With regard to the second source of difficulty, the infinite variety of combination which cardiac disease exhibits, baffles all the *book-rules* with which the student sets out in practice, and it must comfort him to know, that the determination of the exact valve, or even of which side of the heart is affected, is of comparatively little value, as the rules of treatment are in reality deducible from the vital and mechanical conditions of the cavity of the heart. " It appears certain," says our author, in another place, " that we must be guided in our treatment of valvular disease, less by the condition of the valves than by that of the muscular portions of the heart." (*On the Pathology of Valvular Disease*, p. 134.) His remarks are therefore not very detailed. He contents himself with protesting against the theory which would regard all these change as commencing with, and maintained by, inflammatory action. This is an error which we owe to the physiological school, and from which we wish the minds of our students were thoroughly purged. While we admit that many changes in the valves are of inflammatory origin, as vascularity, opacity, softening, one variety of vegetation, and also that inflammation may often produce, as a permanent result, thickening with or without shrivelling of the valve, we would still give along with this admission the caution of Rokitansky, "Inflammation of the valves, and its results, must not be confounded with hypertrophy of the valves, with excessive endocarditic deposition on them, and its metamorphoses" (cartilaginous, osseous, steatomatous), and even in those cases which are of inflammatory origin, a period arrives, when, the disease still progressing, all inflammatory action has ceased. How long a disease of the valves continues, before it produces an effect on the valvular opening, it is impossible to determine, but the ordinary result is a permanently patent state of the orifices, which may be attended at the same time with contraction. It is the opinion of Dr Stokes, that the distinctness of valvular murmur bears no proportion to the amount of disease. We are disposed, however, to think that the *character* of the murmur has some relation, and that, generally speaking, the harsher murmurs betoken a peculiar advanced state of disorganization.

Dr Stokes is of opinion that cessation of a murmur at an advanced stage of organic disease is observed in cases of contraction of an orifice. There is one circumstance which is dwelt upon at considerable length, the observation of which is new to us, and which has thrown light on several cases, the history of which has often been a sore puzzle. We doubt not others in similar circumstances will thank us for reprinting the passage entire.

> "The recent development of the signs of a chronic, long pre-existing disease is a circumstance which should be known to all who are concerned in the medical examinations for life insurance. Thus, it may happen, a life is passed as insurable after a careful examination. The insurance is effected, and yet, in a short time, the individual exhibits all those signs of morbus cordis which are supposed to indicate chronic disease. He may die of this disease within a few months after the completion of the insurance, and the payment of the sum insured to them be contested on the ground that the disease was overlooked. I have known all the signs and symptoms of permanent patency of the aortic valves to occur within a few months after the effectuation of a large insurance, and yet, at the period of the medical examination, which was made by one of the best observers in this or any other country, no sign of disease of the heart existed. In the same way, I have known the signs of chronic mitral disease become most strongly developed in the course of a few days. These facts are of practical importance, for in the case of a judicial trial, on the ground of the incompetency or neglect of the medical examiner, many professional men would incline to the opinion, that the affection had been overlooked, rather than that it had become developed in so short a time after the examination. They would be influenced by the opinion that the development of disease and of its symptoms and signs are concurrent, a doctrine which we have seen to be untenable in acute, and, of course, far more so in chronic disease."—(P. 147.)

We have not perused any medical treatise with more thorough satisfaction than that part of Dr Stokes' work which treats of the diagnosis of valvular disease, which is dealt with in the plain and common-sense way which displays the hand of a master.

Admitting that "the diagnosis of valvular disease depends on the existence and application of certain physical signs," he nevertheless argues that "the questions of prognosis and treatment depend upon the condition of the muscular portion of the heart," p. 160. The following are his practical rules:—

> "The practical physician, on being called to a case of valvular disease, having satisfied himself of the existence of an organic change

in the mitral or aortic valves, or, as it may be, in both, will then direct his attention to the following points, which are the important subjects of consideration. These are:—

(1.) To determine whether there is much obstruction to the current of the blood.

(2.) To determine whether the permanent action of the heart is augmented or depressed.

(3.) Whether actual enlargement of the cavities of the organ has taken place.

(4.) Whether the action of the heart is regular, or the contrary.

(5.) To ascertain, as nearly as possible, the duration of the disease.

For it is on these points that his treatment must turn, and his prognosis to a great degree be founded."—(P. 158.)

The extreme rarity of diseases of the valves on the right side leads him to limit his consideration to those on the left side exclusively. When disease does occur on the right side, it is usually associated with an open foramen orale, or a deficient ventricular septum. In the opinion of Dr Stokes, "our knowledge of clinical medicine does not justify a diagnosis of the diseases of the tricuspid or the pulmonary valves," p. 163.

We are not sure if the infrequency of valvular disease on the right side has not been somewhat overrated. We are disposed to attach considerable weight to the recent observations of Dr Blakiston, and to believe that it is often overlooked, though perhaps we could scarcely go the full length of his theory, and admit that it is the chief cause of general obstruction. The following sentence, which we quote from Dr Blakiston, gives a summary of his views on the subject. We should have liked to have seen Dr Stokes deal with them in his own practical way. As usual, however, we are disappointed in any reference to contemporary authorship. After stating that the tricuspid valves are as liable to disease as the others, Dr Blakiston continues—"the disease in the mitral valves is generally palpable, while that of the tricuspid valves may very often escape the eye of a superficial observer; and yet the shortening of the chords of the latter, so easily overlooked, would probably have a much more serious effect on the circulation than an obvious thickening and narrowing of the mitral orifice, or a deposition of vegetations on its valves." The correctness of this view derives additional confirmation from the recent observations of Dr M'Dowell. Dr Blakiston in effect admits that the disease of the tricuspid valves to which he alludes usually accompanies dilatation of the right ventricle. In his most recent paper (*Dublin Quarterly Journal*, xxxiii., p. 91), Dr M'Dowell contends that "progressive dilatation of the cavities of either side of the heart eventually leads to dilatation of the orifices by which they communicate;" and he gives several cases in which "dilatation of the right auricle and right ventricle was complicated by a permanently patent condition of the right auriculo-ventricular opening, leading to regurgitant disease."

Dr Stokes has also omitted all reference to rupture of the cordæ tendineæ of the tricuspid valve, as inducing regurgitation, of which Dr Robert Todd has recorded an interesting case (*Dublin Quarterly Journal*, vol. v., p. 1).

Dr M'Dowell is of opinion, from a review of the recorded cases, that "the signs of tricuspid regurgitation are directly deducible, and may be thus enumerated:—

"1. Venous congestion (and the various morbid conditions which it gives rise to, as dropsy, epistaxis, hepatic engorgement, &c.)

"2. Jugular turgescence and pulsation.

"3. Strong impulse of the heart, coinciding with feebleness of the radial pulse.

"4. The signs which indicate the enlargement of the right side of the heart.

"5. A bellows murmur, accompanying the systole of the heart, and situated over the region of its apex, but extending towards the right rather than towards the left side."

Dr M'Dowell, however, notes that an audible beat is often prevented from the dilated condition of the right side of the heart, and its consequently feeble action.

Passing then to the consideration of the valves on the left side, we return to the work of Dr Stokes. He commences by again drawing attention to the numerous causes of disease in them apart altogether from inflammatory action. His summary does not, however, evince much acquaintance with the more recent views of pathology, and we would refer any one interested in the subject to the work of Zehetmayer (Grundzüge der Percuss und Auscult:) a valuable paper from which, bringing together in one view the numerous internal structural lesions capable of causing an endocardial murmur, is translated in the excellent work of Dr Davies (Diseases of Lungs and Heart, p. 260). Among the causes of valvular disease, granular kidney is omitted altogether by Dr Stokes, though the elaborate statistical paper submitted to the Medico-Chirurgical Society in 1848, by Dr Whyte Barclay, shewed that one-half of the fatal cases of this disease were coincident with valvular disease.

The Symptoms of Disease of the Mitral Valve are the first which our author considers. He is of opinion that "there are no symptoms proper to mere disease of the mitral valve," and that those which "have been given by various writers belong to complicated rather than to simple disease of the valves" (p. 174)—a doctrine, indeed, propounded by Laennec, though not generally acknowleged at the present day.

We have been accustomed to consider mitral insufficiency to exist (mitral contraction is more difficult to diagnose), when we had the following physical signs.

First sound masked by a murmur having its maxmium intensity over the left apex—a thrill at the apex. A feeble, small, unequal pulse. The murmur, according to Dr Davies, is sometimes audible in the axilla. But how admirably does Dr Stokes deal with all this.

> " A striking defect of many modern works on diseases of the heart is, that the authors assume not only that each disease of the heart has its special phenomena, but that no difficulty attends the determination of those accompanying circumstances by which the seat of the abnormal signs is to be settled. The real difficulties of the subject have not been fully stated, and hence one cause of the differences of opinion as to the exact nature of a particular case. It happens fortunately that if the general diagnosis of organic disease be correct, the special diagnosis is of little value. This point has been already insisted on ; but to return to the subject in hand. We read that a murmur with the first sound, under certain circumstances, indicates lesion of the mitral valves. And again, that a murmur with the second sound has this or that value. All this may be very true, but is it always easy to determine which of the sounds is the first, and which the second ? Every candid observer must answer this question in the negative. In certain cases of weakened hearts acting rapidly and irregularly, it is often scarcely possible to determine the point. Again, even where the pulsations of the heart are not much increased in rapidity, it sometimes, when a loud murmur exists, becomes difficult to say with which sound the murmur is associated. The murmur may mask not only the sound with which it is properly synchronous, but also that with which it has no connection ; so that in some cases even of regularly acting hearts, with a distinct systolic impulse, and the back stroke with the second sound, nothing is to be heard but one loud murmur.
>
> So great is the difficulty in some cases, that we cannot resist altering our opinions from day to day, as to which is the first, and which the second sound."—(Pp. 179, 180.)

And again, though the extract is a long one, its value demands its insertion.

> " But does the state of our knowledge of the signs of cardiac disease, and of vital acoustics in general, justify us in making an absolutely positive diagnosis, not only of the seat of the murmur, but of the nature of the disease, and the calibre of the orifice ? This question must be answered in the negative, and we must receive as unproved, and calculated to throw discredit on the science of diagnosis, all those rules and descriptions of special phenomena, supposed to apply not only to almost every pathological change of the valves, but every

possible combination of these changes. In the ordinary cases of mitral murmur we cannot say whether the murmur is "constrictive" or "regurgitant," or constrictive and regurgitant; and we must reject a large proportion of descriptions of phenomena which, although the changes they are supposed to indicate be familiar to anatomists, are themselves of doubtful value. To the inexperienced, the detailed descriptions of such phenomena as the intensification of the sounds of the pulmonary valves,—of constrictive murmurs as distinguished from non-constrictive,—of associations of different murmurs at the opposite sides of the heart,—of pre-systolic and post-systolic, pre-diastolic, and post-diastolic murmurs,—act injuriously; *first*, by conveying the idea that the separate existence of these phenomena is certain, and that their diagnostic value is established; and *secondly*, by diverting attention from the great object, which, it cannot be too often repeated, is to ascertain if the murmur proceeds from an organic cause; and again, to determine the vital and physical state of the cavities of the heart."—(Pp. 180, 181.)

Dr Stokes abandons the division of cases of mitral valve disease into the regurgitant and non-regurgitant forms, because the effect of organic change in the valves must be in most instances to produce regurgitation to a greater or less degree. He observes,—

> "The following division appears more justifiable.
> "1. Contraction of the orifice.
> "2. The diameter of the orifice remaining unchanged.
> "3. The orifice dilated."

Every one who has seen much disease of the heart must have been struck with the great rarity of obstructive mitral disease. We are scarcely disposed to concur in the opinion of Dr Stokes, that all disease of the mitral valve must necessarily be regurgitant. If we must adopt a theory on the subject we would prefer that of Dr Davies, who says,—

> "A probable reason for its rarity may be found in the fact, that the current of blood to which it is due flows into the left ventricle through the mitral opening at the period ofthe heart's diastole, when the organ is at some distance from the chest-wall, and when any vibrations produced in the interior of the chamber cannot be expected to be communicated with much facility outward."

Dr Davies has never met with a simple diastolic mitral murmur; Dr Ormerod (Gulstonian Lectures, 1851), met with one instance. He records, however, eighteen cases where a double murmur existed at the apex. Of course neither of these authors are alluded to by Dr Stokes.

The most ardent admirer of Dr Adams cannot fail to be satisfied with the ample justice done to his researches on the safety-valve function of the tricuspid valves, as well as to his theory of venous pulsation. It is the warm tributes of a sincere and admiring friend.

But we must hurry over much that is important and signally deserving of deep and earnest study in this truly valuable book, for our space is well nigh exhausted.

The distinction which is drawn between the physical signs of the permanent patency of the mitral and that of the aortic valve differ in this, that in the former case the murmur is single, in the latter double.

The physical signs of aortic disease are a murmur, loudest at the base of the heart, and propagated into the arteries ;—single and systolic when the valves are competent ; generally double, but occasionally single and diastolic, when they are inadequate. Aortic regurgitation is often accompanied with the signs noted by Dr Corrigan, viz., the visible arterial throbbing, the collapsing pulse, and the fremitus attending the pulsations of the arteries in the neck.

The attention which Dr Bellingham has paid to the pulse in valvular disease naturally leads us to yield great deference to the results of his observations, of which we here give his summary *in extenso.*

" Summary of the *principal* varieties of the pulse. In the following summary of the more prominent characters of the pulse, in the several diseased states we have been considering, the varieties of the pulse are arranged according to their regularity or irregularity. The first division includes the cases in which the pulse is usually regular ; the second, those in which the pulse is usually irregular.

Pulse Regular.

(1.) In plethora the pulse is full, hard, and resistant, but not more frequent than natural.

(2.) In anæmia the pulse is smaller, weaker, and quicker than natural, and in addition has a jerking character.

(3.) In hypertrophy of the left ventricle the pulse is "strong and prolonged." When dilatation is combined with the hypertrophy, the pulse is in addition full.

(4.) In dilatation of the left vetricle without hypertrophy, the pulse is large, but weak.

(5.) In dilatation, with attenuation of the left ventricle, the pulse is weak and soft, easily quickened, and occasionally intermittent.

(6.) In patency of the aortic valves, the pulse is jerking, visible, and locomotive, but its frequency is not increased.

(7.) In diseased states of the coats of the arch of the aorta, with increase of the calibre of the vessel, the pulse has the same characters, but in a somewhat less marked degree.

(8.) In aneurism of the arch of the aorta, the pulse may have a different strength in each wrist, or it may be absent altogether in the radial artery on one side. It might also have a jerking character, resembling that of aortic regurgitation.

Pulse Irregular.

(1.) In extreme contraction of the mitral orifice, the pulse is small, weak, intermittent, irregular, and unequal.

(2.) In regurgitation through the mitral orifice, when slight, the pulse is little altered; when extreme, the pulse has the same characters as in considerable contraction of the orifice.

(3.) In contraction of the aortic orifice, the pulse presents no peculiarity unless the degree of contraction is extreme, when it becomes small, irregular, and intermittent, rarely slower than natural.

(4.) In fatty degeneration of the muscular tissue of the left ventricle, the pulse, in the advanced stages, is small, weak, irregular, and unequal, sometimes slow.

(5.) In pericarditis, with copious liquid effusion, the pulse presents somewhat similar characters.

(6.) In cases where fibrinous concretions form in the cavities of the heart, the pulse suddenly becomes small, weak, intermittent, and irregular.

(7.) In adhesion of the pericardium, the radial pulse is occasionally intermittent and unequal.

From what has been said, it may be gathered, that in several forms of cardiac disease, the characters presented by the pulse are of a nature materially to aid the diagnosis; and if the different cardiac lesions were met with always distinct from each other, and uncomplicated with other affections, the signs derived from the pulse would be most valuable. Unfortunately, however, this is not often the case. Two or more forms of structural disease are frequently combined; for instance, hypertrophy and dilatation of the left ventricle are very frequently associated; and, as these morbid conditions are the ordinary result of disease of the valves, we may have obstructive or regurgitant disease of the aortic or mitral valves, or of both, combined with hypertrophy, or dilatation of the left ventricle, or with the two latter states. In addition, disease of any part of the heart may be associated with a state of anæmia or hysteria, with functional derangement, a gouty or a nervous habit, or with some of the other morbid conditions that have been alluded to; when the pulse, if trusted to alone, would be more likely to lead into error than to assist the diagnosis. It must be borne in mind, too, that intermission or irregularity of the pulse may ensue in any diseased state of the heart, when the pulmonary circulation becomes greatly obstructed. It should be also recollected, that a very trifling morbid alteration of the valves at the left side of the heart, which may be accompanied by a loud bruit de soufflet, often causes no alteration of the pulse; while, in a very advanced stage of the same disease, when the pulse is intermittent and irregular, an abnormal sound may be absent. Finally, as the systole of the right ventricle exercises no *direct* influence upon the pulse, the walls of the right side of the heart may be hypertrophied, or their cavities dilated; or its valves or orifices might be diseased, without the pulse indicating, in the slightest degree, the morbid alteration."—(Pp. 193, 194, 195.)

On the whole, from a consideration of this chapter, it would appear that, in the opinion of Dr Stokes, the only forms of valvular disease, the existence of which can be determined with anything approaching to certainty, are,—

1. Uncomplicated mitral disease.
2. The disease of the aortic valves described by Corrigan.
3. The disease of the aortic valves without permanent patency.
4. Extreme ossific disease of the aortic orifice.

We anticipate with extreme satisfaction the pooh-poohing which these candid admissions will receive from a certain class of juvenile stethescopists—who never knew a difficulty, and who will regard this as a virtual abandonment of the outposts of science. Be it so, if it enables us to take up a stronger though less advanced position.

We would be inclined to recommend the student of valvular disease to supplement the valuable and copious remarks of Dr Stokes by a careful study of the papers by Dr Ritchie, published in the former series of this Journal, more especially of the one in which the effect of the diseases on the right and left sides are compared (No. 185); of the work of Dr Bellingham, and of his clinical lectures in the *Dublin Medical Press* (1845); of a valuable summary of the signs of valvular lesion by Dr Parkes, *Medical Times* (February 23, 1850); and of the Gulstonian Lectures of Dr Ormerod, *Medical Gazette* (1850); and of the works of Davies and Wardrop.

(*To be continued.*)

ART. II. *The Dissector's Manual of Practical and Surgical Anatomy.* By ERASMUS WILSON, F.R.S., Second Edition, 8vo, pp. 626. London, 1853.

Since the first edition of this treatise was published, in 1838, several text-books on anatomy have appeared, and become the favourites with the student. As systematic works, we have had the translation of M. Cruveilhier's anatomy, and the new edition of Quain's anatomy, by Mr R. Quain and Dr Sharpey, both of them complete and authoritative works; and, as less comprehensive works, the Manual of Anatomy, by Dr Knox, besides our author's well-known systematic manual, entitled " the Anatomist's Vade Mecum." As dissecting room guides, treating anatomy regionally, and describing the parts as they lie in relation to each other, we have had the work of Mr Ellis, and also that of Mr Holden; and, besides, we have the new and enlarged edition of the long famous Dublin Dissector, in which there is, to some extent, a combination of these two methods, by the introduction of dissections along with the muscles.

We feel somewhat at a loss to which of these classes to refer the work now before us. In the preface we are told the work is devoted to display by demonstration, "the component structures in their relation to each other, and their complicity in the injuries and surgical operations to which the body may be subjected," whilst the systematic anatomy is "treated of in my other anatomical work, the Anatomist's Vade Mecum." In accordance with this, the work is divided into chapters, embracing different regions of the body,—as Inferior Extremity—Superior Extremity—Thorax—Back—Head and Neck, &c; and we hoped, on seeing this arrangement, that the work was intended truly to supply the deficiencies of the author's Vade Mecum as a trustworthy guide to the student of anatomy; but have been much disappointed to find that this work does not treat essentially of regional, or at least of relative, anatomy, or "of the component structures in their relation to each other," but is arranged in a manner peculiar to itself, and not so as to be incompatible with the greater part being a reprint from the author's *Vade Mecum*,—the 1845 edition of which we happened to compare with it.

The description of each region commences with some general remarks on the parts to be found in it, and on their surgical interest; but from this fair beginning, the description soon breaks down into systematic anatomy; first of the muscles, and then of the vessels and nerves, each of these being usually described not only separate from the others, but without special reference to them. There may be exceptions to this general statement, but we are correct in saying of the extremities and head and neck, where relative anatomy should be more specially studied, that the above remark regarding the arrangement is correct. For instance, in the region of the palm of the hand, we find, after the fascia, first the muscles systematically described, down even to the interossei, then the arteries, and then the nerves, whilst no description is given here of the long flexor tendons, either in the palm or within their fibro-synovial canals along the fingers.

Of all parts of the body the neck is that which separate systematic descriptions fail to convey a correct knowledge of, and where the relative anatomist is most at home, with his various triangles and their contents, one following the other. But we find our author, after describing the platysma and fascia, giving first a separate description of the muscles down even to those of the tongue, without regard to the nerves and arteries, which follow, also systematically, in separate sections, whilst the relative anatomy in each triangle is given, if at all, in such a way that the parts are rather mentioned as if to be committed to memory, than described so as to add to the student's knowledge.

This we need hardly say is not relative anatomy. We admit that, in describing a region, there must or should be more or less systematic notice of the muscles, vessels, and nerves in it; but this is very different from giving an altogether separate and systematic description of each, and is compatible with a full account of the relative anatomy of each region, and a particular account of the relations of each muscle, vessel, and nerve, to other muscles, vessels, and nerves, —such as may be found in the works of Ellis and Holden, already referred to.

We have said that the greater part of the work before us appears to be a reprint of the author's *Vade Mecum*. The same paragraphs are used sometimes in the same, and sometimes in a different order. The description of the muscles especially is the same, only, singularly enough, omitting here the paragraphs on the relations. The arteries and nerves are occasionally altered, and more fully described. In the description of the viscera of the thorax aud abdomen, the brain and spinal cord, the organs of the senses, and the ligaments, there is very little alteration. It is right, however, to mention, that it is the first edition of this treatise, which appears to contain the original descriptions, which have been subsequently transferred to the author's systematic treatise, and this may explain why the three books so closely resemble each other.

Perhaps our author may reckon it an advantage that the same descriptions should be given in both of his text-books, as the same muscles, vessels, and nerves form the theme of both; but, being so, one of two conclusions are evident,— either that the descriptions which are appropriate in a short systematic manual, cannot be so in a manual professing relative anatomy, or, if they are so, that the appearance of the second book is altogether uncalled for.

But, as it may not be fair to condemn a book merely because the title is inappropriate, or the plan which the author has set for himself may not meet our approval, we shall now proceed to notice how the author has acquitted himself in the execution of his plan. On this score there should be little room for comment. In a student's manual, treating of matters of fact, which have long been familiarly known, and compiled by one who, even before writing the first edition, has had the "experience of seven years, engaged in the active duties of the practical teacher in the dissecting room," the descriptions could scarcely fail to be correct, and suited to meet the difficulties which the student feels in his progress. Taken by themselves, many of the descriptions and hints for assisting the memory are good enough; and most of the illustrations are useful, well designed, and certainly almost all of them well executed by the artist; so that, indeed, the book might be recommended to the student as a good and useful one, were it not that we already possess several which are much superior to it, both in plan and execution.

But, while we think the descriptions well executed according to the plan of the work, we at the same time can by no means give to them our unqualified approval. There are not a few errors in point of anatomical fact; there is frequently a want of precision, and not a few omissions, unpardonable in any text-book, but more especially in one professing to treat of surgical anatomy.

We shall now notice a few of these, in the order in which we have happened to meet with them, and make such comments as may serve to furnish some information, as well as to point out the deficiencies of this treatise.

Inferior Extremity.—On page 8 there is a diagram of the thigh, of which we observe first, that the saphenous opening is placed too far inwards, as, when it is dissected, the outer edge of the opening corresponds usually to either the middle or the inner edge of the femoral artery; and secondly, that the apex of Scarpa's triangle is placed on the inside of the thigh nearly half way down to the patella, instead of at four inches below Poupart's ligament and just over the femoral artery. It is the upper, not the lower, border of the adductor longus that we have always been accustomed to consider as forming the internal boundary of the femoral triangle proper, just as we take the upper and not the lower border of the sartorius for the outer boundary.

The true or deep layer of the superficial fascia of the groin is not correctly described. It is described as a deep portion of the superficial fascia, separated from the other at the groin by the glands and superficial vessels, adhering to Poupart's ligament, passing down and becoming lost on the fascia lata, and also covering in the saphenous opening. Now, this fascia (which we shall call Scarpa's) is an important one, simple in its arrangement, and easily displayed if intelligently dissected. Divide the common superficial fascia about two or three inches below Poupart's ligament, and dissect it up over the glands and superficial vessels, to about an inch above Poupart's ligament, beyond which point the farther separation becomes artificial. Now divide the fascia on the abdominal wall by a curved incision two or three inches above Poupart's ligament; dissect it down, close off the external oblique tendon, and we are stopped at, or rather close below, Poupart's ligament. Now hold the lower end of the dissected fascia in one hand, and the upper end in the other, raising them up, and on looking below, the fascia of Scarpa will be seen fully displayed. It is a thin but strong fibrous membrane running across the whole breadth of the groin; abruptly arising from, or continuous with, the fascia lata, immediately below Poupart's ligament; superiorly continuous with the common superficial fascia, an inch or less above Poupart's ligament, the glands and superficial vessels lying between; and, internally, it is continued towards the scrotum, covering and guiding the cord, and runs directly into the true superficial fascia of the perineum.

Our author gives a correct description of this latter fascia (fascia of Colles), and explains how it determines the course of effused urine; its attachment behind and at the sides, preventing the urine from passing backwards to the nates, or downwards along the inside of the thighs. But it is not explained why the effused fluid, after infiltrating the scrotum should continue to ascend over the cord up to the abdominal wall, and not pass down the front of the thigh. This is prevented by the fascia of Scarpa, which, directly continuous with the fascia of Colles, forms a continuation across the groin of the barrier, by which the effused urine is prevented from gravitating downwards.

The fascia of Scarpa, having this anatomy, has likewise important relations to the herniæ at the groin. It covers the inguinal hernia, and assists in directing it towards the scrotum; and its union with the common superficial fascia a little above Poupart's ligament assists in limiting the upward course of the complete femoral hernia. The reason why this fascia is not generally understood and so described, is, we believe, that, in dissecting downwards, its origin from the fascia lata is divided, under the impression that this is only the separation of an adhesion of the fascia to Poupart's ligament. Unless the dissection is conducted in the method implied in the general directions we have given above, the true relations of the fascia of Scarpa will not be understood.

Important muscles and tendons are described without any reference to their surgical anatomy. Thus, there is no mention of the mode of effecting complete relaxation of the rectus femoris, as in the treatment of fractured patella,—that the hip must be somewhat flexed, in addition to extending the knee;—the anatomy of the tendo Achillis is meagrely described, and there is no mention of operation of division of the tendon, or of the necessity of flexing the knee as well as extending the ankle, in order to relax the gastrocnemius completely, as in treatment of the accident of rupture of the tendon; nor is any allusion made to operations on any of the tendons of the ankle; all points of importance according to our understanding of "surgical anatomy."

Superior Extremity.—In connection with the axillary and brachial arteries, we find no allusion to their varieties, or even to the fact that there is often a high origin of one of the arteries of the forearm, although this occurs as often as in one in every five subjects. Nor can we appreciate the author's objections to the operation of tying the third portion of the axillary artery, which may be done with ease and safety, where it lies in front of the conjoined tendon.

Another blameable omission is the absence, in the description of the hand, of all allusion to the thecæ, or fibro-osseous canals, with their synovial linings, for the flexor tendons. The tendons are briefly noticed with the muscles of the forearm, but without any allusion to the thecæ or the synovial lining and nutrient vinculæ within the canals; and with the ligaments of the hand, there is but a short notice of them, without any reference to their surgical anatomy. It is surely within the province of surgical anatomy to point out that these canals shut in the tendons along the fingers; that the incision for the relief of tension, or evacuation of matter, in whitlow, must be deep enough to lay this canal more or less open, without which the incision will be of no use; that an incision made longitudinally, and in the centre, will effect this purpose, and avoid the bloodvessels and nerves, whilst one made across, or obliquely, would divide the vessels and nerves, and, if it went deep enough, the tendons also, besides insufficiently opening the canal. If anatomists do not point out the reason for it, the student is not unlikely afterwards to forget the surgical direction as to incision here, for want of the free practice of which so many fingers are lost, or remain stiff after the loss of the flexor tendons.

Errors are made in the description of the passage of the tendons beneath the posterior annular ligament of the wrist. As our Edinburgh students know very well, this ligament is like a bridge with six unequal arches, each without any variation, transmitting its tendon or tendons, with their synovial linings; and they could tell our author, as well as we can, that the tendons of the extensor longus digitorum and extensor indicis lie in the same groove and canal, not in separate ones; and that the extensor minimi digiti lies, not in a groove on the ulna with the extensor carpi ulnaris, but in a canal of its own, without contact with either bone.

In the description of the *thorax*, we happened to observe that no notice is taken of the dilatation of the last part of the great coronary vein into a muscular sinus, as was particularly described more than twelve years ago by John Reid. At the beginning of this sinus, some distance down the vein, we have found one or two valves, as described by Reid, as well as by several of the older authors; notwithstanding which, the remark has crept into several text-books, that the veins of the heart are destitute of valves.

We may here also remark that we have always found a pair of valves apparently unknown to most anatomists, at or near the lower orifice of the internal jugular vein, and also in the subclavian vein. This does not accord with the common statement, that there are no valves in the veins of the head and neck, except in the external jugular vein; or again, that "the subclavian vein is the continuation of the axillary, but, unlike it, has no valves" (Quain's Anatomy, new edition). But our author does not mention that they exist even in the external jugular. The valves of the internal jugular vein we have always found to occur close above its termination in the innominate, or from half an inch to an inch above its lower orifice. They are two in number, either transverse or oblique, and one of them is apt to be divided in slitting open the vein to search for them. We have found valves also in the trunk of the subclavian vein, near to its termination in the innominate, and are not sure but that, in the instances in which we met with one only, the other had been destroyed in slitting the vein. We have long been in the habit of pointing out these valves at the root of the neck, and find that they were known to former anatomists, as to Sir Charles Bell, who has indicated their existence by the swellings he has figured on the veins in this situation. The point comes to be one of practical interest. Can the distension of the right side of the heart and neighbouring great veins in asphyxia be relieved backwards by an opening in the external jugular vein? This point had attracted the notice of Dr John Reid, who has shewn in his paper, "On the effects of venesection in renewing and increasing the heart's action under certain circumstances," that the heart's action may be renewed in asphyxiated animals, by an opening made in the external jugular vein. That there are valves in these veins there is no doubt, but they do not appear usually to resist injections, and would, therefore, probably allow of a return of the blood, especially in the then distended state of all these veins. The question is one deserving of farther investigation on the human subject, and it would be interesting to institute a series of experiments to ascertain whether water thrown up, for instance, by the abdominal veins, would freely escape by a wound in the external jugular.

Neck.—Here we find a passage indicating a startling boldness in practice, upon the fascia of the neck, p. 231. "When, however, tumours form beneath it, as bronchocele, enlargements of the lymphatic glands, aneurism, &c., the pressure which it then exerts may be fatal to the patient, from compression of the trachea, larynx, and nerves, *unless* (the italics are our own) *the tension be relieved by incision.*"

The list of primary branches of the external carotid artery is made up to *ten*, by the addition of sterno-mastoid and parotidean branches. *Eight* we hold to be quite enough for the learner to master, besides being the correct number of regular primary branches. The sterno-mastoid occasionally no doubt receives a separate branch from the external carotid, but is usually supplied chiefly by two twigs, one from the superior thyroid, which crosses the carotid sheath to reach it, and a larger one from the occipital artery. The parotid gland, again, is supplied by several twigs, coming both from the external carotid and its temporal branch.

We observe the omission of all reference to varieties of the subclavian artery, or its branches, except in the statement that "when a fifth artery exists, it arises from the third portion, and is the supra-scapular.". Now all anatomists who have observed these arteries in the dissecting room, or those who have studied Mr R. Quain's work on the arteries (*London* 1844), know that it is very common to find a large branch coming from the third portion of the subclavian artery, as often probably as once in every three subjects, very near where a ligature would have been placed on the artery; and that this branch is seldom the supra-scapular, but is an artery which thereafter dips backwards, usually between the two trunks of the plexus, to reach the upper angle of the scapula under cover of its levator muscle, and is therefore, not the supra-scapular, but the posterior-scapular artery.

We observe here that the vertebral vein is now correctly placed, not behind, but in front of the first portion of the right subclavian artery, and do the author the justice to say that the statement of this relation is *not* a reprint from his other book.

Brain.—The corpus callosum is stated to be four inches in length. It may be so in a brain soft and flattened out on a table; but the brain it will be granted is not longer than the cavity in which it lies, and at most the seven inches thus given are thus distributed;—corpus callosum three inches;

from corpus callosum to front of brain, an inch and a half; and from corpus callosum to posterior end of brain, two inches and a half. The position of the corpus callosum bears an interesting relation to its relative thickness at the middle, and at the anterior and posterior extremities. Such points are easily and naturally settled by the application of a foot-rule, which, in a small form, the anatomist will find a very useful instrument.

The *foramen of Monro* is described as "transmitting" the plexus choroides, and the veins of the corpus striatum, pp. 317 and 321. Now, the foramen of Monro does not transmit any vessels, or anything at all but serum, being merely an open passage between the lateral and third ventricles, and formed simply by perforation or continuity of the lining serous membrane. It is not sufficiently expressed in descriptions of the brain, that the foramen of Monro is not, as the communication between the third and fourth ventricles is, a hole or passage in the nervous substance, but in the lining serous membrane only. Were it not for the reflection or continuation of the internal arachnoid, from the upper surface of the fornix outwards across the rest of the floor of the lateral ventricle, there would be a foramen or fissure of Monro all the way back beneath the edge of the fornix, and the lateral and third ventricles would form one cavity, by continuity both above and below the choroid plexus and velum interpositum; but the lining serous membrane prevents this, and only where the crus of the fornix bends round the shoulder of the thalamus is it perforated, so as to be continuous with the lining membrane of the third ventricle below.

It was, we presume, the want of appreciation of these points regarding the nature of this hole, formerly so interesting to the Edinburgh student, which for some time after it was brought into notice by Monro *secundus*, led to a discussion among anatomists as to whether the hole really existed; the point lying in this, that it is not a hole in the nervous substance or brain proper, but only in the lining serous membrane, as is still better seen in cases of chronic collection of serum in the ventricles, when we have seen it large enough to admit the point of the finger. As the foramen of Winslow does not transmit the vena portæ, but is a serous passage between the lesser and greater sacs, and is separated from the vein by a serous layer, so is the foramen of Monro an opening between the serous cavities of the third and lateral ventricles, and the plexus choroides does not pass through it, but bounds it behind, with a delicate serous layer between.

It may be here observed that, although this foramen is named after Monro, it appears to have been well known to previous anatomists. Indeed, it is not easy to understand how it could have been overlooked. In the dissection of the base of the brain, when the gray matter in the floor of the third ventricle has given way so as to expose the ventricle, the two openings are so plainly visible behind the pillars of the fornix, that they could not have escaped the notice of the many accurate anatomists who described the brain before the time of Monro. The claims of that anatomist to the discovery, now just a century ago, of these communicating apertures, may be found in his work on the Brain, Eye, and Ear (1797); and the facts shewing their existence to have been well known to previous anatomists, may be found in Sir Charles Bell's separate work on the anatomy of the brain, with engravings (1802), in which it is remarked—" the only difference is that Winslow says it is a passage betwixt the third and the two lateral ventricles, while Dr Monro says it is a passage betwixt the two lateral ventricles and the third."

The division of the middle portion or lobe of the cerebellum into several parts, each with a distinct name, is adopted; the superior into three parts, or at least three names, and the inferior into four, the names of which we abstain from repeating, as a step towards their suppression. We cannot see any good purpose to be served by this subdivision, the greater part of which, besides, is unnatural or at least not very apparent. We wish anatomists would agree to abandon the use of these terms, as serving only to dishearten the student in his efforts to master the already overburdened nomenclature in connection with the brain.

The decussation of the anterior pyramids of the medulla oblongata is mentioned merely as a free communication of the pyramids across the fissure by a decussation of their fibres. No mention is made of the interesting fact, as fully described by John Reid twelve years ago, that each pyramid, in chiefly decussating, passes, not merely across, but also backwards, so as to be continued into the lateral column of the opposite half of the cord.

There is a diagram, fig. 48, of a transverse section of the spinal marrow, in which the gray matter is very erroneously figured like two new moons joined across by a very broad gray commissure. The commissure should be narrow, and the anterior end of the crescent large and rounded, compared with the posterior. However correct the text may be, the student will still rather take his impression from the diagram.

Perineum.—The fascia covering the levator ani is not noticed in the description of the ischio-rectal fossa. Now this fascia is of much importance in connection with the fossa. By its union with the obturator fascia it closes the fossa above, and, by lining the levator ani, it effectually prevents, in abscess in the fossa, the passage of matter into the rectum, at least earlier than close above the sphincter, an inch or little more above the anus, where the internal opening in complete fistula may be constantly found.

When in the neighbourhood of the obturator internus, we are reminded to remark, that if our author, in the next dissection he sees of the deep gluteal region, will divide the tendon of that muscle and lift it inwards, he will see behind it not only the grooved cartilage and synovial membrane covering the lesser notch, but an appearance so striking that we are sure he will give it a prominent notice in future editions.

Of lithotomy we read, that "the proportional distance between the tuberosity of the ischium and anus is the Scylla and Charybdis of the operation; for approaching nearer the former would endanger the internal pudic artery; and the latter the rectum;" &c. This remark regarding the former danger may be a common one, but still we think the pudic artery so safe, in an operation with any ordinary knife, making its way towards the bladder, that the incision might come close to either the tuberosity or ramus of the ischium, or even shave off a slice of the bone, without touching the artery. Even when the ischio-rectal space and the anterior part of the perineum are deeply dissected, there is considerable difficulty in bringing the trunk of the artery into view: it lies bound down by the obturator fascia, and concealed above or within the edge of the tuberosity and ramus; so that to succeed in wounding it in the operation would require a considerable amount of design and dexterity with any ordinary lithotomy instrument.

There is no allusion to the supposed danger of dividing the reflection of the pelvic fascia on the side of the prostate, so as to endanger infiltration of urine below the peritoneum; and indeed the relations of the prostate and the formation of its sheath are but meagrely described.

We have heard a great deal said about the danger of dividing this reflection of the fascia, but to us it appears the danger is an imaginary one, if the lateral operation be performed according to the common directions. The line of the reflection crosses the prostate obliquely, downwards and backwards, leaving in front about three-fourths, and behind two-thirds of the prostate, below this line. If, when the section of the prostate was being made, the knife was directed upwards and outwards, and carried at the same time deeply back through and beyond the neck of the bladder, the reflection of the fascia might be divided; but if the knife cuts horizontally, or, as is usually directed, downwards and outwards, the reflection of the fascia, though, like the pudic artery, not very far away, is still perfectly safe.

Anatomy of Fœtus.—In the description of the fœtal vessels and heart, there is no direct statement of the fact that the point where the ductus arteriosus joins the arch of the aorta is invariably beyond the origin of the left subclavian, a fundamental point in connection with the fœtal circulation; nor is it mentioned with the anatomy of the thorax, that the left recurrent nerve hooks round the arch to the *left* side of the remains of the ductus arteriosus, an important point in connection with the early development of the arteries and nerves. Nor is the valve of the foramen ovale sufficiently described, as lying to the left side of the aperture, serving to prevent the blood from passing from left to right during fœtal life, as well as to close up the aperture after birth, by becoming glued to the edges of the hole on its left or posterior aspect.

Of the fœtal liver we read, "the left lobe is as large as the right." This is a common remark, but an erroneous one, at least as applied to the fœtus near or at the full time. The left lobe is considerably, apparently nearly a half, smaller than the right; though of course proportionally to the right it is larger than in the adult, which we presume is the truth intended to be conveyed by the erroneous statements such as the above. Nor is the distribution of the umbilical vein correctly given by our author. It is said to give branches—"1, two or three which are distributed to the left lobe; 2, a single branch, which communicates with the portal vein in the transverse fissure, and supplies the right lobe; and, 3, a large branch, the *ductus venosus*, which passes directly backwards, and joins the inferior cava"—p. 557. Now we have always found the arrangement to be this. It gives three or four branches on each side, those to the left lobe being larger than those to the right, and some of these, or others, may be concealed, coming off between the vein and the fissure; and, having reached the transverse fissure, it breaks up into three branches, one to the left lobe first; one twice as large to the right lobe, which after a course of half an inch is joined by the vena portæ; and a third, in the middle, the *ductus venosus*, not larger than the branch to the left lobe, which, after a course of three-fourths of an inch, enters, not the vena cava, but the left hepatic vein, about a quarter of an inch before the latter ends in the vena cava.

The ductus venosus is not a "large branch," but a small one; it cannot carry above a sixth or an eighth part of the blood of the umbilical vein straight on. Far too much importance is usually given to it in descriptions of the fœtal circulation, as if it were of equal importance with the ductus arteriosus and foramen ovale. In the fœtal horse it is even altogether absent. In man it forms an escape or direct passage to near the heart for but a small part of the umbilical blood; the great part must first traverse the liver, where some important change, probably of the nature of purification, is no doubt effected on it by this organ, so large in the fœtus although it is not yet engaged in the secretion of bile.

Of the membrana pupillaris of the fœtal eye, our author says "sometimes it remains permanently, and produces blindness." In the next edition this passage may stand thus—"It has been supposed that the membrana pupillaris sometimes remains permanent, and produces blindness; but this is an error, as no authentic cases are on record, to prove that it has occurred as yet."

Orbit and Eye.—On pp. 219 and 370 we have the old and erroneous description of the tendons of the recti muscles forming an expansion at their insertion; thus—"*The insertion of the four recti muscles into the globe of the eye forms a tendinous expansion, which is continued as far as the margin of the cornea, and is called the tunica albuginea.*"

How this mode of description has come to be so generally adopted in text books on anatomy we are at a loss to understand, as it is so easily seen to be erroneous and entirely without foundation. This supposed coat was the tunica tendinea, or tunica albuginea of the old anatomists, the discovery claimed by Columbus, but before "known" to Galen. As long ago shewn by Albinus and Zinn, not only is there no such membrane, but absolutely no arrangement in the least approaching to the formation of such a membrane. The error may have arisen from the use of the term "tunica albuginea," by which some may mean simply the white appearance of the sclerotica, while others fancied the supposed tendinous expansion of the ancients to be referred to in the use of that term. The term "tunica albuginea" ought, therefore, to be altogether abandoned. But this source of confusion is scarcely an apology for the error of some modern anatomists in describing a tendinous expansion to be formed by the recti tendons, and "continued as far as the margin of the cornea." A very slight examination will satisfy any one that these tendons are inserted abruptly into the sclerotica, having become only a very little broader, but far from anything like approaching each other. Their edges are as neat and clean cut as those of the muscles which end in them; their abrupt termination in the sclerotica is a little curved with the convexity towards the cornea, and about three lines from it; and they dip in and are soon lost by becoming blended with the fibres of the sclerotica, but neither in it, nor over it, is there anything like a membrane formed by the expansion or union of the recti tendons. In propagating this error in modern times, our author, though by no means singular in this respect, must be held to have had a principal share. We have not only the description, but, on page 371, a diagram representing the tendons joining each other, with beautifully curved edges, and forming a sheath over the front of the sclerotica; and, contrasting this with the statement in the preface to the first edition, that the descriptions "have not been copied from the works of his predecessors, but have been penned from the great book of nature," we trust we have convinced our author of his fallibility, and that he shares in common with many of us the tendency to see things as others have described them, rather than as they really exist.

We have equal fault to find with our author's statements regarding the action of the oblique muscles, besides that he places the insertion of the superior too far back in saying that it is inserted near the entrance of the optic nerve. But, as the action of the oblique muscles may be regarded by many as debatable ground, we shall remark only, that, whatever their actions may be, our author can give no proof that they are such as he describes, and that he seems to be quite unacquainted with all that has of late years been written in Dublin and in Edinburgh on this subject, all tending to establish the view of John Hunter and others, that they are provided for the purpose of rotating the eye on its antero-posterior axis.

We observe a diagram of the nerves of the orbit (fig. 27), in which the roots of the fifth nerve are traced to before and behind the olivary body, as if this were an admitted point in anatomy,—in which the third nerve is the same size as the sixth, instead of being four times larger,—and in which the long root of the ciliary ganglion is an eighth of an inch, instead of half an inch, in length, and the little ganglion itself is actually as large as the first division of the fifth pair.

The structure of the cornea is more fully described than in the first edition, but we find no mention of the anterior elastic lamina, discovered and described by Mr Bowman several years ago. We are informed that "the opacity of the cornea, produced by pressure on the globe, results from infiltration of fluid into the cellular tissue connecting its layers. This appearance cannot be produced in a sound living eye, although a small quantity of serous fluid (liquor corneæ) is said to occupy the areolæ of the cellular tissue." Can our author furnish any proof of the correctness of either of these three statements?

In connection with the choroid, we have a diagram (fig. 52) of the venæ vorticosæ, quite unlike any venæ vorticosæ we have ever seen; and we read—"in animals, the pigmentum nigrum, on the posterior wall of the eye-ball, is replaced by a layer of considerable extent, and of metallic brilliancy, called the tapetum." On the contrary it will be seen, if the choroid be seized with the forceps near the tapetum, and pulled inwards, that the pigment is not here replaced, or wanting, but exists plentifully behind the tapetum, which is merely a brilliant arrangement of fibres on its anterior or internal surface.

The ciliary ligament is figured like a belt drawn round the eye, with an abrupt posterior edge; and nothing is said about the deeper and posterior part of it being muscular, as several observers have of late years pointed out; although we may observe, in passing, that they are wrong in attributing the view of its muscularity to Porterfield, who scarcely mentioned the ciliary ligament, but described the ciliary processes or body under this name, and strongly advocated the view that they were muscular in their nature.

Of Jacob's membrane we read, "Dr Jacob considers it to be a serous membrane." Now this implies the idea of a double reflected serous membrane, as Mr Dalrymple believed it to be when, in 1834, he wrote his book on the anatomy of the eye; but Dr Jacob, although originally he may have used the term serous, is careful to explain, writing in 1839, that it is not a serous membrane, part of his words being—"I have above stated my reasons for considering it a single layer, and not a double serous membrane;" and as the nature or minute structure of the membrane is now well known, and bears out Dr Jacob's description, it is hardly fair to assign to him, a living man in 1853, the antiquated opinion of another, which he had written to correct fifteen years before.

The retina is described, after Treviranus, as presenting minute papillæ on its internal surface, and our author would therefore seem to be unacquainted with the microscopical researches of Hannover and Bowman.

The capsule of the lens is mentioned without any reference to the important fact that it is several times thicker in front than behind; and the old error, that it "contains a small quantity of fluid, called *liquor Morgagni*," is reproduced, although followed by the saving remark that "Dr Jacob is of opinion that the lens is connected to its capsule by means of cellular tissue, and that the liquor Morgagni is the result of cadaveric change," p. 378. Now Dr Jacob not only expressed his belief, but clearly demonstrated the fact, that there is a strong adhesion between the lens and its capsule, that there is no fluid here in a recent eye, and that it is incorrect to attribute to Morgagni the deliberate belief that there is fluid here in the living healthy eye. That the capsule of the lens should allow a fluid to pass in after death, but exclude it during life, is not more wonderful than that the same capsule should so easily become opaque during life, but resist all means to make it so after death; or that the cornea should not absorb and swell out with the aqueous humour during life, as with water after death, the difference lying in the possession or loss of vitality; but whatever the explanation may be of the formation of the fluid which may always be seen to escape on puncturing the capsule of an eye not recent, especially if it has lain a day in water, it does not alter the fact, which we had thought every anatomist now knew, that in the recent eye there is absolutely none; and the single layer of cells by which the union between the capsule and the soft exterior of the lens is effected our author will find described and figured in Mr Bowman's lectures on the eye, published four years ago.

In fig. 54 the lens is drawn like that of a very old person, the axis or thickness little more than a third of the diameter or breadth. It is very common to find the lens figured out of all proportion to the eye, either too large or too small, too convex or too flat; but the proportions are easily struck if these points be recollected—that the breadth or width of the lens is one-third of the axis or diameter of the eye, and measures one-third of an inch, whilst the depth or thickness of the adult lens in the middle is one-sixth of an inch, that is, half the breadth.

We read that the free border of the ciliary processes "rests against the circumference of the lens," but in the diagram they are made to overlap in front of the rim so far, that were the pupil fully dilated they would be clearly seen from without in the living eye. The exact relation of the tips of the ciliary processes to the lens is a point of much interest. The above description accords rather with the view of Galen than that of modern anatomists, who are aware that the tips of the processes are free, not involved behind in the plaitings of the hyaloid membrane, but floating loose in the posterior chamber; not touching the rim of the lens, but lying a little in front of it; but how far they actually overlap in front of the rim is not quite certain.

The fact that they are not visible when the pupil is fully dilated might lead to the supposition that they were retracted along with the iris, from their connection with it, or from the action also of the ciliary muscle; but that they are not so altered we have found, on examining the eyes in the horse within half an hour, and the cat within a few minutes, after death, one eye having been previously brought fully under the influence of atropine, when no difference could be detected between the two eyes in the position of the tips' ciliary processes. Besides, the pupil of the human eye dilates only sufficiently far to equal the size of an average lens.

Of the chambers of the aqueous humour we read—"the two chambers are lined by a thin layer, the secreting membrane of the aqueous humour," p. 378.

How long will this error be continued in descriptions of the eye? No one ever saw or traced such a membrane, and we submit that, in a question of anatomy such as this, what has never been seen either with the naked eye or microscope, does not exist, or must be held not to exist. The membrane of Descemet or Demours, so well described by Dr Jacob as the elastic cornea, is not, as some suppose, what is meant by the membrane of the aqueous humour, as it is a layer of the cornea, not reaching farther than into the ciliary ligament and origin of the iris. The single layer of cells on the surface of the cornea, in contact with the aqueous humour, extends no farther; if it did, the cells would be seen elsewhere as well as on the cornea, but there are no cells on the front of the iris, or on the capsule of the lens, nor on the back of the iris, distinct from those holding the pigment of the uvea. The foundation for the view that there is a lining membrane to these chambers, is the supposition or idea that there must be one to secrete and contain the fluid, a serous membrane being, on the ground of analogy, supposed to be required. Analogy, we may remark, is at best not a proof or ground for inference, but merely suggestive of research to determine the fact. But we may ask, in what does the supposed analogy consist? Are there any serous membranes in the body, with the cavities distended with or containing a fluid? The halitus or steam of the older anatomists is now met with in a condensed condition only, as a moisture on the opposing surfaces of the serous membranes, and more than this would render them less fitted for their purpose. The only serous cavity naturally holding any serous fluid, and that but in small quantity in health, is the ventricular space, divided into the several communicating ventricles of the brain. On the other hand, there is only one instance of a considerable collection of serous fluid naturally existing in health, namely that around the spinal marrow and certain parts at the base of the brain, and there it exists, not in a serous cavity, although there is one near enough to it, but loose in the sub-arachnoid spaces, secreted by the vessels of the pia mater, as the aqueous humour no doubt is by the vessels of the iris and ciliary processes. The analogy then is rather the other way, agreeing with the fact, that no membrane can be traced lining these chambers.

The Joints.—The last chapter embraces the anatomy of the joints, and is all but a literal reprint from the author's systematic manual, and also corresponds closely to the text in the first edition. So far from disputing the propriety of introducing the joints into a manual for the dissecting room and of surgical anatomy, we are of opinion that there is no part of anatomy of greater importance to the surgeon than that of the larger joints, and perhaps there is none more neglected; but we do not think such descriptions as those with which we are here presented can serve any practical or surgical purpose. The author's views regarding the study of the joints may be gathered from the circumstance that the chapter is headed, not of the articulations, but " Of the *Ligaments;*" and so it is with many, who seem to think that, in connection with the joints, they have only to commit to memory the names and attachments of the ligaments. Now, the study of the joints should embrace, first, a careful consideration of the form and adaptation of the articular surfaces, then the ligaments, then the synovial membrane, with the extent and recesses of its cavity, and lastly, and of great importance, the natural motions of each joint; not merely the names of these motions, but the nature and direction and extent of each. In connection with the hip-joint, for instance, it is not enough to mention, as we find here, that *rotation* is one of its motions, but the kind of motion which rotation is, inwards and outwards, the effect it has in rolling the whole limb, and the method of moving the limb so as to perform it, as in the examination of the limb after an injury, are all points quite as worthy of notice as the fact that there is a capsular or a round ligament. Farther, we conceive the surgical anatomy of the joints to embrace the notice of the osseous prominences outside and near the joint, and the relative position of these in the different motions. With such a knowledge of the joints the surgeon can usually see at a glance, when there is no fracture or dislocation, the natural figure of the limb and motions of the joint remaining; and when there is, he readily detects the nature of the injury.

We pass this general condemnation on the whole of this chapter, on the ground of its deficiency of practical and surgical bearing, and besides, notice several unpardonable omissions. Thus, in connection with the shoulder-joint, we are left to infer only that the long tendon of the biceps lies above the head of the bone. There is no allusion to the fact that it passes over the ball of the humerus, like a belt, serving, when the biceps acts, to keep it from rising up against the acromion process; or that the tendon lies in a grooved recess in the capsular ligament, the anterior boundary of which is so prominent that some have considered it as analogous to the round ligament at the hip; which, however, it is not, even although a probe may sometimes be passed round it, free within the joint, close to its attachment to the lesser tuberosity, and just in front of the commencement of the bicipital groove. There is consequently no allusion to the occurrence of dislocation or rupture of this tendon in the joint or groove, of which so many cases have been brought together in Mr Callaway's treatise on the shoulder-joint (London, 1849), which we would recommend to our author's notice.

Of the hip-joint it is enough to say, that not only is there no mention of the strong periosteum, with the raised fibrous bands on the neck of the thigh-bone, which may retain the fractured ends in apposition, as we have seen, but there is not even any direct mention of the pregnant fact that the neck of the thigh-bone lies within the cavity of the joint.

We would suggest for insertion in future editions, the general information as to the direction of flexion and extension of the great joints of the extremities, that, in the superior extremity, flexion is forwards and extension backwards at all the joints, whilst in the lower extremity, flexion and extension are *alternately* forwards and backwards, as at the hip, knee, ankle, and toes; from which it follows, that when a muscle crosses two joints in the upper extremity it either flexes or extends both, if in the lower extremity it flexes one and extends the other, except in the cases of the sartorius and gracilis muscles.

We would also venture to recommend to all anatomists the omission of such terms as flexors or extensors of the thigh, or flexors or extensors of the leg, and to substitute for them the expressions flexors and extensors of the knee, or of the ankle, as the flexors and extensors *of* the thigh are not the extensors *in* the thigh; the use of such terms leads to confusion, but no one can mistake what is meant when we speak of the flexors or extensors of any particular *joint.*

In connection with the knee-joint, the synovial pouches at the *side* of each condyle (into which a loose cartilage may be pushed and fixed by operation) are not mentioned; nor is there any mention of the prolongation of the synovial membrane carried out of the joint by the tendon of the popliteus muscle, which, passing close to the back of the superior tibio-fibular joint, so readily explains how there can be, as there occasionally is, continuity of the cavity of this joint with that of the knee, a circumstance, which otherwise it would be difficult to account for.

The *bursa* is described as "placed between the anterior surface of the patella and the fascia lata. It is the latter which is inflamed in housemaid's knee." We have always found this bursa to be placed in the cellular tissue immediately under the skin, and to correspond to the lower part of the patella, that part of it on which we rest in kneeling, and not under the fascia lata. The facility with which swelling takes place in effusion into it, and with which, after suppuration, pus makes its way to the surface, corresponds to this superficial position of the bursa. We have, at the same time, often noticed a bursa, or apparent bursa, still deeper, and between the prolongation from the fascia lata and the fibrous covering of the patella; but never without a free communication with the regular sub-cutaneous bursa; and crossing the interior of this bursa, and also of that between the trochanter major and the gluteus maximus, may be often seen a series of soft fibrous bands, like chordæ tendineæ, giving a complicated appearance to the bursa.

In now concluding our notice of this manual, we trust that our readers will forgive us for the length of some of the digressions we have been tempted to make, on the ground of the information furnished, and the interest of the points discussed. We also trust that our author will be duly sensible of the obligations he is laid under to us for the corrections and hints we have ventured to bestow; and, for ourselves, we shall not grudge the trouble this has given us, as, besides, it has afforded us the opportunity of warning the student against being misled by a new and attractive title to an old inferior book.

ART. III.—1. *The Pathology and Treatment of Pulmonary Tuberculosis; and on the local medication of Pharyngeal and Laryngeal Diseases, mistaken for, or associated with, Phthisis.* By JOHN HUGHES BENNETT, M.D., F.R.S.E., Professor of the Institutes of Medicine and of Clinical Medicine in the University of Edinburgh, &c. &c. Edinburgh, 1853. 8vo, pp. 142.

2. *Clinical Lectures on Pulmonary Consumption.* By THEOPHILUS THOMPSON, M.D., F.R.S., Fellow of Royal College of Physicians, Physician to the Hospital for Consumption. London, 1854. 8vo, pp. 211.

The teeming literature of tubercular phthisis will not suffer the reviewer's pen to rest. Although many months have not passed since we devoted a long article to this subject, we feel called upon to notice two very remarkable works which have recently emanated from the London and Edinburgh press. We shall do so, however, as briefly as possible, lest the reader whom we have, perchance, persuaded to peruse Mr Ancell's excellent and comprehensive volume be tempted to quarrel with us for disturbing the repose which he may naturally feel to be necessary after so considerable an intellectual repast.

We may begin by telling him for his comfort, that both the works before us are, in some degree, republications, and that their leading principles are already tolerably familiar to the reading public. Dr Bennett's volume is a connected narrative of researches, which have extended over upwards of twelve years, which have in a great degree contributed to raise their author into eminence, and have already, in various forms, through numerous lectures and memoirs, attracted a large share of public attention. The lectures of Dr Theophilus Thompson were delivered, and were afterwards published in the *Lancet* for 1851. Nevertheless we think that both authors have done well to give the profession an opportunity of calmly and deliberately considering the net result of their labours. We desire also to express our admiration of the earnest and intelligent spirit which has evidently presided over both works,—the condensation of style, the clearness of conception and expression, the abundance of materials for thought, which distinguish these volumes from the crowd of ephemeral productions which load our table. No one can doubt that they are the works of men who have seen and heard for themselves, and who, by means of labours conducted in a scientific spirit and under favourable circumstances, have earned the right to submit their conclusions to the public with somewhat more of authority than we should be disposed to allow to the majority of medical authors.

Dr Thompson's lectures are, as indeed clinical lectures ought always to be, for the most part a series of running commentaries on the results of direct experience in practice. For this reason, no doubt, he has avoided theoretical considerations, even when, according to our views, his case might have been strengthened, and his illustrations rendered more impressive, by a more free appeal to general principles. It is not fair, perhaps, to be severely critical in regard to those attributes of a work which spring necessarily from its plan, unless we are prepared to condemn the plan; but we confess to a wish that Dr Thompson had, in collecting his "clinical lectures," made them a little less clinical and somewhat more systematic, even had this reformation resulted in the suppression of the present title and the remodelling of the entire work. The "clinical" plan is no doubt the best of all for fixing details on the mind of a student, when in view of the cases on which the lectures have been founded, but we have strong doubts whether it ought ever to be attempted in print, and especially in the form of a book. There may be exceptional cases; but as a general rule, good clinical lectures are sorry reading, and well-digested writing assumes a form totally unclinical. The really valuable matter of Dr Thompson's lectures was worth the time and labour necessary for changing its form, and would have been, we are satisfied, more acceptable to the public, had more care been taken to collate the author's opinions and experience with that great mass of knowledge which, borne to us in part by literature, and in part by tradition, constitutes the science of medicine. We trust that in the event of a second edition being demanded, or indeed in any case, we may hope for something more complete on the subject of phthisis from the pen of Dr Thompson than the present volume. It would be a noble task for the accomplished author of the "Annals of Influenza" to give to the public, in a practical and concise form, a history of phthisis, and of its treatment, connecting the present with the past, experience with reasoning, observation with careful and select reading of the best authors. The present work is too much "a thing of shreds and patches" to take permanent hold of the public mind, or to admit of instructive analysis; although there are many of the author's opinions with which we agree, and some of his observations which are so interesting and novel, as well to deserve a place in our columns. Such are the cases of hydatids of the liver simulating phthisis, in Lecture I., and of gangrene of the hand from obstructed arteries, in Lecture II. Such also are the investigations on the variations of the pulse in phthisical persons under changes of posture; on the prolonged expiratory murmur, and on the wavy inspiration; on the appearances of the alveolar margin in phthisis (founded on the well-known statements of M. Fredericq of Brussels); and on mental depression as a cause of phthisis. In all of these inquiries, if they are borne out by those of others, Dr Thompson is justly entitled to the praise of having studied nature for himself with good effect; and in the meantime, at least, we see no reason to doubt these observations and inferences, although the numerous possible fallacies preclude us from giving a too ready assent to the whole of his doctrines. In a few instances we think he has obviously deceived himself, as in his parallel of phthisis and rheumatism from very limited chemical data (p. 80); in his too confident diagnosis of phthisis from a microscopical examination of the sputa, supposed to present "the peculiar granular character (?) distinctive of tubercle" (p. 48, and fig. 1, plate 1); and in his assertion that the blood-corpuscles are physically altered in phthisis, in regard to which the evidence given is very unsatisfactory, and not consistent with our own observation. These, however, even if we are correct in considering them as blemishes, are small blemishes in a work of so much merit.

Dr Bennett's work is by no means chargeable with that desultory character which we have ascribed to Dr Thompson's lectures. It is, on the contrary, devoted, with great singleness of purpose, to the exposition of a peculiar theory of phthisis, which the author holds to be both new and important, as leading to certain fixed principles of treatment not usually recognised as such by the profession. The method of the work is synthetic, the pathology of the disease being first laid down as the result of investigations into its histology and chemistry, and the treatment being not so much regarded from an empirical point of view, as deduced from the author's pathological position. It is true that the use of cod-liver oil in tubercular diseases, the most important and peculiar element of the treatment recommended by Dr Bennett, was adopted by him in the first instance from his experience of its results in German practice; but we believe that we rightly estimate the doctrine of this treatise, when we indicate that this innovation in British practice is only part of a system to which the author has been led, in the main by pathological considerations, and with which he is desirous that his energetic and successful advocacy of cod-liver oil should be associated. How far this body of doctrine is correct, how far it is complete, and how far it may have influenced, or may be calculated to influence favourably the current practice in phthisis pulmonalis in this country, are the questions which Dr Bennett "leaves to the candour of his medical brethren;" and if indeed he could establish to the satisfaction of the profession that the nature and treatment of this dire disease were finally settled, we may admit at once that he would deserve a civic crown. But here we take leave to observe, that the appeal must be, in the first instance, not to our "candour," but to our judgment.

In bringing the theory of tubercular phthisis adopted by Dr Bennett before our readers, and in submitting it to such criticism as our space will permit, we are greatly assisted by the praiseworthy clearness with which the author has stated his views, and the mode in which he has everywhere appealed directly to facts for their support. In the preface the peculiarities of the system of Dr Bennett are announced in a few sentences, which, as tending to narrow the discussion to a point, we submit to our readers:—

> "The author has attempted to shew, 1*st*, That tubercular diseases will heal of themselves, if the faulty nutrition of the system can be removed. 2*d*, That with this object our efforts should be directed to the digestive, rather than to the respiratory system. 3*d*, That the kind of abnormal nutrition which exists is dependent on increased assimilation of the albuminous, and diminished assimilation of the fatty portions of the food. Hence he recommends that the general plan of treatment should be to cause the reception of the deficient elements of nutrition, and is therefore not tonic nor stimulating"—(nor yet, we might add, antiphlogistic) "but *analeptic* (from *αναλαμβανω*, to restore)."—(P. 9.)

Of these three propositions, the first was so clearly demonstrated by Laennec (to go no farther back in the history of opinion as to the cure of phthisis), that we can only wonder at the manner in which the opinion of Bayle, as to the absolute incurability of tubercle, maintained its ground in France, and was propagated in this country long after the appearance of the "Auscultation Mediate." This is the more remarkable because every anatomist of eminence (with, perhaps, one or two exceptions) who has given particular attention to the subject, has testified, like Laennec, to the healing of tubercular deposits, and of excavations in the lung; and the works of Cruveilhier and Carswell have placed graphic illustrations of these facts in all our public, and in many private libraries. The labours, in these later times, of MM. Rogée and Boudet, and of Dr Bennett, will, we trust, contribute to the final extinction of an opinion which was well calculated to paralyse the energies of the practitioner, in dealing with a disease always obstinate and often fatal. That there is nothing in the nature of an individual tubercular deposit which can prevent its undergoing cicatrization, and that such evidences of healing are manifested in a considerable proportion of cases dying in our hospitals, whether of tubercular or of other diseases, are truths which can no longer be doubted. The excellent and striking woodcuts in Dr Bennett's work will, we trust, go far to carry confidence, where even multiplied verbal testimony may have failed to do so.

It becomes us to recollect, however, that the cure of an individual deposit of tubercle is not always a proof of the complete eradication of the tubercular tendency. Laennec, indeed, pointed out that tubercular diseases present, in the majority of cases, the evidence of an intermittent progress; and that the suspension of their course may take place for an indefinite period, while the seeds of disease still lurk in the constitution. Nay, the disease may even undergo a complete suspension in the lungs, while its progress in other organs leads to a fatal result. Of this we have seen repeated instances, and Dr Bennett records a most striking example in his third case, in which a partial cure in the lungs and intestines was followed by rapid tuberculization of both kidneys. Add to this, that the tendency of tubercle to a retrograde course becomes, in all probability, less with each successive deposit, and that an immensely large proportion of the cases of permanent suspension of tubercular disease are examples of its slighter degrees only, and it will be seen at once that the perfect cure of a "confirmed phthisis" must still be considered one of those rare events for which we are not, indeed, forbidden to hope, but of which we can indulge only a very moderate expectation. On the other hand, the considerable amelioration of phthisical symptoms, and the restoration of the patient to a state of health, or, at the worst, of comparative comfort, for a period more or less protracted, ought to be looked upon as one of the most probable results of treatment in the early stages, and as being, at no period of the disease, beyond the combined resources of nature and art.

On the subject of the cure of phthisis and tubercle, we present a brief abstract of Dr Bennett's experience, as recorded in this work.

Case 1. A man, at the age of 22, labours under symptoms of phthisis, but recovers, and dies at the age of 50 from meningitis. A cicatrix, three inches long, evidently the result of a tuberculous cavity, is found in a perfectly healed lung; in the other lung some concretions and puckering. This case is in its anatomical evidence complete. Treatment unknown.

Case 2. A man, aged 28, passes three months in the Infirmary, and afterwards is more than a year under dispensary treatment by cod-liver oil, for phthisis, with signs of excavation in left, and induration in right apex. After marked improvement, he dies of fever, when tubercle in process of healing is found in both lungs.

Case 3. We have already alluded above to this case. A man, aged 19, had scrofulous sores of the wrist, excavated lungs, and marked constitutional symptoms. He repeatedly rallies under cod-liver oil, &c.; undergoes fever and small-pox, but ultimately is seized with renal symptoms, and dies exhausted, after an exacerbation of them, attributed to an injury of the back. Excavations in the lungs mostly cicatrized; ulcers of intestines partially cicatrized; tubercular abscesses of the kidneys.

Case 15. A medical student, aged 20, has incipient but well-marked phthisis in 1842; cod-liver oil and general hygienic treatment. With long intervals of improvement, the disease makes several aggressions during the entire period of medical study. After examination in 1845, amendment; cretaceous matter expectorated in 1846; on entering business as a general practitioner, continued amendment; recovery and good health to 1853. In this case, there was repeated hæmoptysis and *probable* auscultatory evidence of excavation in one apex, which evidence continued at the last examination in 1846.

Case 16. A lad, at the age of 15, presents marked constitutional symptoms, necrosis of the thigh, evidence of pulmonary condensation of apices with probable excavation. Slight endocardial disease. Marked improvement under cod-liver oil. Pulmonary symptoms reduced to slight cough in the morning, after 2½ years' treatment, in December 1851. Health good in 1853.

Case 17. A student, aged 17, presents well-marked symptoms and signs of incipient phthisis, with condensation of both apices and probable incipient excavation. Fluctuations in health for four years, while pursuing medical studies, without any systematic treatment. Then after an aggression of the disease, treatment by cod-liver oil, &c. Improvement; calcareous expectoration; complete restoration to health in three years (1850); continued good health on foreign service in the army, 1853.

Case 18. A man, aged 35, presents in 1843 symptoms of incipient phthisis with condensation of right apex. Treated by cod-liver oil. The disease arrested in nine months; good health, except slight asthmatic tendency, to 1853.

Case 19. A man, aged 22, intemperate, marked phthisical symptoms in 1844, of 2½ years standing; pectoriloquy, &c., in right apex; excessive weakness, sweating, and diarrhœa. Great improvement under cod-liver oil, &c.; continued to June 1846, when he was able for work as a labourer, but breathless on going up a stair.

Case 20. A milliner, aged 22. Symptoms of phthisis with condensation of left apex, in 1844. Physical signs had disappeared, and she felt well in 1845.

Case 21. A man, aged 24, has hæmoptysis and hectic fever in 1836, with incipient condensation of the right apex. In 1837, recovers under iron, &c., with tonic regimen, milk and light animal diet, &c.; good health to 1853.—(Dr Evans of Birmingham.)

Case 22. A medical man had phthisical symptoms well-marked, and signs of excavation very distinct (Dr Corrigan) in left lung. Under iron, counter-irritation, and occasional leeching, and good diet, improvement. From 1841 to 1850, good health.

The above cases form a most interesting and valuable series, and for no part of Dr Bennett's work do we anticipate a more attentive and grateful consideration by the profession. The details are, in many of them, exceedingly instructive; and had Dr Bennett done no other service to science than this accumulation of facts, the work in which they occur would have been entitled to more than usually detailed notice at our hands. No doubt, the difficulty of tracing the histories of protracted cases of disease, and the small confidence possessed by many physicians in their own diagnostic powers, are the main causes why, amid so much charlatanism and self-assertion, we have had so little faithful and exact observation on the cure or arrest of tubercular disease. Every physician has met with parallel cases, but few have taken the trouble, or perhaps possessed the necessary qualifications for tracing them carefully, and recording them in a convincing manner. "As to the particular facts," says Laennec (alluding to *nine* cases of this kind) "which I have brought forward, to prove the possibility of the cure of phthisis pulmonalis (*i.e.*, of tubercles in the lungs having undergone softening and excavation), "I think that every attentive observer who will employ the same means that I have employed, that is to say, mediate auscultation and the opening of dead bodies, will frequently meet with the like. Everything leads me to believe that these cases are extremely common. The examples which I have brought forward have offered themselves to me in the space of a few months; and in the same time, or since, I have seen many others."

We do not think, however, that the facts either of Laennec, or of any of his followers in this inquiry, will ever justify a sanguine prognosis in individual cases, where the phthisical tendency has been inveterately manifested at an early period of life. The traditions of science, and the common sense of the world and of physicians, have long settled the question both of the *possibility* and of the *improbability* of the cure of what has been called *confirmed,* in opposition to *incipient* phthisis; and we do not see that modern science, in inculcating the healing of tubercular cavities, has done much more as yet than correct its own errors. The incurability of *tubercles,* and even of *vomicæ,* was in fact never a part of the creed of the practical physician, though it was, during the beginning of the present century, the doctrine of the morbid anatomist; and, from its extensive diffusion, has no doubt been productive of much mischief. If we look at Dr Bennett's cases, with the view of discovering a new era in the cure of this disease, we shall be disappointed; inasmuch as it is very certain that they disclose nothing as regards the *phthisis confirmata* or *inveterata* of our fathers, which has not been at all times observed, recorded, and freely admitted. Let us see what deductions have to be made from these eleven cases, considering them as cases, not of *tubercular disease,* but of *confirmed* and unequivocal *phthisis.*

Case 3 must be entirely laid aside. Not only was there no cure, but the patient was only, "by great care, *kept alive* for two years," when the disease, according to its treacherous habit, took on a new form, and carried off the patient.

Cases 2, 16, 19, 20, are imperfect or questionable cures. The arrest of active but limited disease took place satisfactorily so far as it went, and the subsequent progress of the case is removed from observation before its final issue can be predicted. These are cases of threatening and intermittent tubercular disease, but not of confirmed and inveterate phthisis. Case 16 is, however, in all respects a remarkable and most interesting case, considering the number of serious affections, both external and internal.

In Cases 15, 17, 18, 21, 22, there is a longer period of health after the arrest of active symptoms (2 to 15 years).

In several, however, the period is too short to afford permanent security, and in none of them had the disease at any time displayed that obstinately progressive and rebellious character under proper hygienic management, which was conceived by the ancient physicians to denote confirmed phthisis, as distinguished from vomica and other affections from which it was supposed to take its rise. These are cases of limited tubercular disease, satisfactorily arrested, and altogether similar to those recorded by Laennec.

Case 1 stands by itself as presenting at once the longest period of immunity from symptoms, and the most accurate data for estimating the extent of the material disease. It will be observed by those who are practically acquainted with auscultation, that the amount of tuberculization in all cases not completed by a *post-mortem* examination must necessarily be inferential only, and that in most of the above cases there is reason to doubt whether it had ever proceeded beyond a limited infiltration of one or both apices. Now, in Case 1 we readily admit the complete suspension of the progress of tubercular disease. But we do not by any means admit the cure of a confirmed phthisis in the sense in which that term was habitually employed before the pathological anatomists of France began to take undue liberties with our nosology. It will be obvious at once that the tuberculization must have been limited to the apices of the lungs in this case; and although a considerable cavity had formed, yet we know, both from personal experience and from facts recorded by Laennec, that this may take place without any constitutional symptoms beyond those of severe and protracted catarrh, or at most of pneumonia, on which no careful physician would have pronounced the verdict of *phthisis confirmata.*

We have considered it requisite to direct attention to these points, because it would be a most erroneous inference, either from Laennec's cases or from those now before us, that modern and ancient experience are at all at variance as to the cure of phthisis. The fact is, that until a comparatively late period, phthisis was regarded as incurable only when it had resisted all the remedial measures applied to it, and thereby betrayed that character of obstinacy and inveteracy which was described as the distinction of its most formidable varieties. That such cases still occur, and still prove a source of enormous mortality, notwithstanding all the resources of modern science, is a fact too melancholy and too true to be questioned. The healing hand of time, the progress of civilization, and the growth of the human mind, have failed to arrest, in any remarkable degree, the ravages of phthisis;

and while new pestilences appear among us, and old ones die out,—while intermittent fever, dysentery, and smallpox, are disappearing in this country before the cunning and foresight of the human race,—it would be hard to say whether the 14th century or the 19th has abounded most in tubercular disease. Certain it is, that so far as specific remedies are concerned, the cure of phthisis has done no credit to our art; and that the greatest physicians have been content to refer the recoveries which they observed to a yet greater physician than themselves, and to confess that art could do no more than watch the footsteps of nature, and remove the thorns out of her path. Such was the opinion of Laennec, whose practice, as we have seen, abounded with proofs of the occasional recovery from phthisis.

The recoveries recorded in the present volume are ascribed by the author mainly, or at least in no small degree, to the beneficial influence of cod-liver oil administered internally, along with such hygienic regulations as appeared calculated to strengthen the system against the inroads of disease in general, and a very sparing use of those narcotic and other palliatives, which form the chief resources of some practitioners. We agree with Dr Bennett in thinking, that the use of these palliatives is often carried much too far. As it is always easier to give a draught than to withhold it, and as the habit of attention to details often predominates over the strongest convictions founded on general principles, we doubt not that there are many things to amend in the treatment of phthisis in this country; and we willingly accord to Dr Bennett the merit of having taken the right side, in fixing attention strongly on the constitutional phenomena, and disregarding, in comparison, the local symptoms. But Dr Bennett seems to think that the invigorating or restorative mode of treatment has been a rare exception in professional practice, and that it "was never based upon pathological principles, or advocated as the only method capable of arresting and overcoming the disease."

> "In consequence, the general treatment of the profession has, on the whole, been antiphlogistic, to combat supposed inflammation, lowering, to subdue febrile irritation; and palliative, to relieve occasional symptoms."—(P. 82.)

And thus, he goes on in the next chapter:—

> "It is *by no means uncommon* to meet with patients who are taking *at the same time* a mixture containing squills and ipecacuanha, to relieve the cough: an anodyne draught, to cause sleep and diminish irritability; a mixture containing catechu, gallic acid, tannin, or other astringents, to check diarrhœa; acetate of lead and opium pills, to

> arrest hæmoptysis; sulphuric acid drops to relieve the sweating; quinine, iron, or bitters, as tonics; wine to support the strength, and cod-liver oil in addition. I *have seen many persons taking all these medicines and several others at one time*."—(P. 84.)

We have italicized a few of the above clauses, because they demand animadversion. Even Dr Bennett will scarcely, we think, peruse them deliberately, without seeing that they stand self-condemned, as a gross and almost ridiculous caricature. If it be true, as we think it is, that no one will read these descriptions without a smile (certainly not at the expense of those at whom they are pointed), it can scarcely be necessary for us to pass any severer criticism, either on these particular fancy-pictures, or on the extraordinary assertions with respect to the treatment of phthisis in this country, which appear in various parts of this book. We feel in both cases a little amazed, but by no means put out of countenance, at the mirror which Dr Bennett holds up to nature; we cannot congratulate him on its even surface, nor on the truth and propriety of its reflections. We shall continue to indulge our own conviction, that the general treatment of phthisis, in this and other countries, has been based, in a considerable degree, upon very plain "pathological principles;" and that the nourishing, restorative, or "*analeptic*" plan, is at once the oldest and the newest, the most necessary and the most obvious, the most clear in principle, the most difficult in execution, of the means which the common-sense of mankind, and the traditions of medicine, have carried down to us as applicable to phthisis. That there have been differences of opinion as to how this indication should be fulfilled, may well be admitted; but that any one, except a pedant or a schoolman, ever proposed to himself a treatment of phthisis which did not include the idea of causing "the reception of the deficient elements of nutrition," we do not believe. In becoming the advocate of this recondite principle of *feeding the emaciated body*, Dr Bennett will, we are persuaded, find that he has only adopted "the general treatment of the profession;" though possibly many contradictory statements on this point may lie rotting in Latin folios, or enshrined among those curiosities of medical practice which have been at different times proposed as being in a special sense "based upon pathological principles," or as the "*only methods* capable of arresting and overcoming the disease."

But how are we to feed the emaciated body? How to supply the deficient elements? We are willing to confess that we have hitherto proceeded for the most part upon a tentative, and, in some degree, empirical plan; and that our efforts have been directed rather to the maintenance of a general tonic regimen, with due regard to the peculiarities of individual cases, than to any dietetic or therapeutical indication of a more specific kind. Such, we believe, has also been the ordinary practice in this country, wherever it has not been injuriously interfered with (as we grant, has too often been the case) by exclusive theories or fashionable remedies. We likewise confess to an unwillingness to depart, without very strong grounds, from this eclectic practice; believing, that in a disease where all the elements of a healthy nutrition are so manifestly prone to disappear, and where fat, muscle, fibre, membrane, skin—all the tissues, in short, whether oily, gelatinous, or albuminous—undergo simultaneous atrophy, we cannot do wrong in ministering to all these deficiencies, and leaving to nature the work of arrangement and reconstruction, In other words, it has been our object rather to enable the patient to outlive the disease, than to cure it. Nor has the late *run* upon cod-liver oil in the treatment of phthisis led us to alter this opinion. We have never indeed ventured to trust the cure to this remedy unaided by other means, but we have used every opportunity of becoming familiar with it, and have failed to discover in it any other or more specific power, in the diseases of man, than has lately been attributed to it in the fattening of healthy cattle—that of being, in some circumstances, an excellent addition to the diet, and a powerful fattening agent. We have never seen it of the slightest service, when the general powers of digestion and assimilation were prostrate, until time or careful management had brought about some degree of renewed power of general alimentation; and we have many times known large quantities of it consumed daily under a strong persuasion of its power to cure, without appreciable effect on the course of the disease, and sometimes without in the least arresting the process of gradual emaciation. Nay, under such circumstances, it has appeared to us to be by no means free from injurious properties, as it often interferes seriously, when taken in excess, or in an impaired state of the appetite, with the digestion of other and more important elements of nutrition.

We do not fear to be regarded as too dogmatic in making these statements, for they are consistent with many of the facts, and even the opinions, recorded by Dr Bennett in the work before us; and we believe they are not less so with the general, and now somewhat extensive professional experience of this remedy. In p. 86, Dr Bennett remarks :—

"If food, or its substitute, cod-liver oil, cannot be taken and digested, it is vain to hope for amelioration in any of the general symptoms of the disease. . . . . . . In all such cases, the stomach should be allowed to repose *before anything be administered, even cod-liver oil.* Sweet milk, with toasted bread, and small portions of meat nicely cooked, so as to tempt the capricious appetite, should be tried. Then ten drops of the Sp. Ammon. Aromat., given every four hours in a wine glassful of some bitter infusion, such as that of Columbo or Gentian, with a little Tr. Aurantii, Tr. Cardamomi, or other carminative. In this way the stomach often regains its tone, food is taken better, and *then cod-liver oil may be tried, first in teaspoonful doses, cautiously increased. Should this plan succeed, amelioration in the symptoms will be almost certainly observed.*"—(P. 86.)

Most true; for amelioration has already begun, when the first and worst symptom has given way, and thus allowed cod-liver oil to be administered without injury, and with possible good effect. But what are we to say of an "analeptic" remedy, which thus deserts us precisely at our sorest need, and cannot exercise its restorative powers until sweet milk, bread, and meat (the most ancient of all "analeptics"), have been tried and found not wanting? Is such a remedy anything more than an ordinary form of oily aliment, and in all probability not the best and most generally useful form? And if so, can the use of cod-liver oil be ever more than a very subordinate part of the treatment of phthisis?

Let us turn to two of Dr Bennett's cases which he gives in great detail as peculiarly illustrative of the effects of cod-liver oil. The first (Case 15, p. 93), is that of a medical student, in whom the general and local signs of phthisis were developed, and the appetite and strength considerably reduced, when he was put under general treatment, and ordered to to take a table-spoonful of cod-liver oil three times a-day. He improved; but "it became necessary to suspend the use of the oil every now and then, on account of the nausea it occasioned." He seems, therefore, to us, to have improved in spite of the remedy. During the next three years several very formidable exacerbations are recorded. Each of these has, as regards remedies, nearly the same history—a period of febrile derangement treated by tartar emetic and opium, succeeded by one of irritability of the stomach, and prostration of the whole system; after the removal of which formidable symptoms, by tonics, &c., and suitable mild nourishment in small quantities, it became possible to administer cod-liver oil; and amendment kept pace with the power of digesting this remedy. In one of these attacks, besides tartar emetic and opium "in large doses,"—"effervescing powders, hydrocyanic acid, creosote, various anodynes, stimulants, alkalies, and bitters," were the precursors of the "analeptic" treatment. In another, after carbonate of magnesia, sal-volatile,—aromatics, astringents, and opium for diarrhœa, an unsuccessful attempt was made to introduce cod-liver oil and animal food; milk and rice pudding were substituted, and even after two days of this regimen, with liquor potassæ, vegetable bitters, and effervescing draughts, it was found that the stomach would not tolerate the smallest quantities of oil, even when skilfully disguised; and only after a fortnight, when several partially successful trials had been made, alternated with the use of naphtha, cardamoms, and columbo, was the "analeptic" practice fairly begun. It is impossible to avoid in this case the conviction that if this patient ultimately improved under cod-liver oil, the improvement was at least *begun*, in every instance of exacerbation of the disease, *before* the use of this remedy. It is equally plain to us that it would have continued, even if this particular form of restorative had been withheld in favour of a carefully managed diet, such as has been used in these cases from time immemorial.

The second case given by Dr Bennett as a special illustration of the results of cod-liver oil, is that of Patrick Barclay (Case 16, p. 101), a tuberculous lad, extremely exhausted from necrosis of the thigh, but able to take on admission full diet, with half a pint of sweet milk twice a-day. To these abundant "analeptics" the usual dose of cod-liver oil was added. After a fortnight's treatment "he began slowly to improve, but was ordered to omit the oil as it occasioned nausea."* The improvement, nevertheless, advanced with more rapid strides; in a month he had got rid of the suffocative cough and vomitings, which had interfered with the oil, and his "general appearance was much improved." At this period he began again to take the oil, became very fat, and, notwithstanding various fluctuations in health, caused mostly by exposure to cold and privations, made good his recovery in about two years and a half. That in this case nothing more than ordinary treatment and good diet were required for the cure, we conceive to be proved beyond question by the results of the treatment during the first six weeks, when the disease may be supposed to have been at the worst, and when, nevertheless, the patient took with excellent results the full diet of the hospital, with extra milk. The oil at this time seemed undoubtedly to be adverse to the restorative process; and it may fairly be questioned whether the patient owed much to it afterwards, even when " he became so stout that it was feared the oil would occasion obesity," and it was on this account suspended. The bare possibility of inducing this condition, and the rapid recovery from the state of prostration at admission, together with the unimpaired appetite, seem to us to show that most of the general symptoms were due to the necrosis rather than to the limited pulmonary lesion, or to the true phthisical element, which was manifestly not very strongly marked in this case.

---

* In his summary of this case Dr Bennett states (apparently by an oversight), that *at no period* did this boy "experience any difficulty in taking the oil . . . . . although ordinary food was at one time frequently vomited." The facts in the record bear in the opposite direction.

Now let it be remarked (as the result of the preceding, combined with other observations on this subject) that cures of phthisis have taken place and been recorded by many of the best authors and most trustworthy physicians in all ages; that they were at least as familiar to Laennec as they are to any one in our own day; that tubercular affections are healed, as our hospital records shew, amid great varieties of condition and circumstance; that, conversely, they fail to be healed, and lead to deaths in all ranks of society, in almost all countries, and under every possible mode of human life; that there is as yet no evidence of a marked diminution of the deaths from tubercular disease since the introduction of cod-liver oil as a remedy in phthisis, although the use of it is now more widely diffused than that of almost any other recent addition to the pharmacopœia; that under cod-liver oil in large doses, some cases of tubercular phthisis are observed to get continuously worse; that without cod-liver oil many of the best cures which we observe in the present day have been effected (as in cases 1, 21, and 22 of Dr Bennett's work); and that where this remedy produces most benefit, the tendency to amendment generally precedes its administration, and is indeed one of the essential conditions of its usefulness. If these facts be carefully considered, we think that it will clearly appear, 1*st*, that cod-liver oil is in no sense of the word a *specific* against tubercular disease; 2*d*, that it is not even a remedy of *primary* importance, but altogether subordinate to proper diet and general tonic regimen; 3*d*, that its claims, even in this point of view, have to be reconsidered, and are as yet undecided by experience. In making these remarks we have no desire whatever to invalidate the striking testimony borne to its advantages in particular cases by many distinguished physicians, nor indeed to enter at all into the question of its real therapeutic value, which is much too large to be discussed profitably at present.

We have noticed in a quotation from the preface to Dr Bennett's work, that one of its objects is to recommend, from a pathological point of view, the use of oil as an aliment in phthisis, on the ground that the essence of tubercular disease consists in "increased assimilation of the albuminous, and diminished assimilation of the fatty, portions of the food." It is not easy to judge how far the theory of phthisis formed by Dr Bennett is the offspring of his experience of the effects of cod-liver oil in that disease, and how far the latter is conceived by him to derive support from the former. That Dr Bennett most earnestly and faithfully advocated the claims of this remedy on the ground of experience gained in Germany, at a period when scarcely any one thought of it in this country; that he has seen it triumph over ridicule and neglect, and take a place among the established resources of the physician; and that in this work, which is in some measure its "currus triumphalis," he has a right to claim the merit of its re-introduction into British practice, and of its first introduction among us as a remedy for tubercular disease, are facts to which we are happy to bear testimony.[1] The theory, however, of its action, which, founded originally on Ascherson's undoubtedly erroneous hypothesis of the formation of organic cells from the direct physical union of oil and albumen as contents and cell-wall, has since undergone, in Dr Bennett's successive publications, various modifications, and is here evolved in a more elaborate form, is evidently one to which he attaches considerable importance; and although we cannot see our way to adopt it, we shall endeavour to lay a very slight sketch of its principal features before our readers, referring them to the work itself for further details.

The first proposition from which Dr Bennett deduces his theory is, that tubercle is an albuminous matter of low organization. This we may readily grant. The inferences from this statement are, however, more questionable. We cannot for a moment allow that tubercle, inflammation, cancer, present *three successive grades* of organization; nor can we regard it as "probable, that while the blood is *normal* in simple exudation, it contains an *excess* of nutritive materials in cancerous, and a *deficiency* of them in tubercular, exudation." Indeed this statement, given without proof, is entirely invalidated by all the researches that have been made of late years into the chemical pathology of the blood. That the blood is often impoverished in tubercular disease, we admit; but who will venture to establish that it is not so in the other diseases named, and in nearly the entire tribe of chronic affections of the system?

---

[1] Dr Theophilus Thompson is singularly silent upon this point. One who professes so large an amount of faith in the oil ought not to have been niggardly in awarding the credit due to the energy and perseverance of Dr Bennett, especially considering the conduct of some of his London brethren, who, owing everything they knew of the remedy and its action to the monograph of Dr Bennett published in 1841, most unjustly endeavoured to pass it *sub silentio*.

As "a healthy nutrition of the body cannot proceed without a proper admixture of mineral, albuminous, and oleaginous elements," Dr Bennett next gives a detailed view of the mode in which he supposes these elements to be combined to form the organic molecules of the healthy body. This we shall not enter upon, because it appears by no means clear, after perusal, what is the precise nature of the change effected in the molecular base of the chyle (which Dr Bennett describes as an emulsion) by which it is transmuted into tissue-elements. We could more easily raise doubts here than lay them. That oil and albumen (for the mineral elements are at this point lost sight of by Dr Bennett) are in some way concerned in healthy nutrition, and therefore in tubercular and all other diseases wherein nutrition is impaired or altered, we are no way disposed to deny. That, therefore, the intestinal canal, the chylopoietic viscera, the blood, may be the starting points of such diseases, we could easily be persuaded, upon anything like sufficient evidence. Whether the following sentences, which contain the entire concluding portion of Dr Bennett's argument, and the entire amount of proof that phthisis is a disease *primarily* of the assimilation, amount to a satisfactory pathological basis for the treatment by cod-liver oil, or a satisfactory pathology for tubercular disease, is a question which we shall leave in the hands of the reader, merely indicating that we take side with the French critic (see the note), not only as to the question of acidity in regard to which we should have liked to have Dr Bennett's experiments in detail, with all their circumstances, and some approach to an indication of their numbers), but with regard also to many other points in the following paragraph.

> "The peculiarity of phthisis, however, is, that an excess of acidity exists in the alimentary canal,* whereby the albuminous constituents

---

* "A French critic, when noticing this statement, asked upon what proof it was founded. The proof, I thought, consisted in the frequent sour taste and acid eructations in phthisical cases. But since this question has been asked, I have seized every opportunity of testing the amount of acidity in the alimen-

of the food are rendered easily soluble, whilst the alkaline secretions of the saliva and of the pancreatic juice are more than neutralized, and rendered incapable either of transforming the carbonaceous constituents of vegetable food into oil, or of so preparing fatty matters introduced into the system, as will render them easily assimilable. Hence an increased amount of albumen enters the blood, and has been found to exist there by all chemical analysts, while fat is largely supplied by the absorption of the adipose tissues of the body, causing the emaciation which characterizes the disease. In the meanwhile, the lungs become especially liable to local congestions, leading to exudation of an albuminous kind, which is tubercle. This, in its turn, being deficient in the necessary proportion of fatty matter, elementary molecules are not formed so as to constitute nuclei capable of further development into cells,—they therefore remain abortive, and constitute tubercle corpuscles. Thus a local disease is added to the constitutional disorder, and that compound affection is induced which we call phthisis pulmonalis—consisting of symptoms attributable partly to the alimentary canal, and partly to the pulmonary organs.—(P. 28.)

Since finishing the perusal of Dr Bennett's volume, we have rested for twenty-four hours, imbibed to the best of our power both oily and albuminous elements; and by a sound sleep, have got rid of the whirring of the logical machinery in which we were at work. The result has been the following conclusion. On separating the questionable from the unquestionable portions of Dr Bennett's theory, and eliminating all its hypothetical or volatile elements, we obtain a fixed residuum as a solid basis for pathological principles and sound treatment. Tubercular phthisis is, according to this net result of our analytic labours, a disease wherein the patient commonly becomes much emaciated. This fact proves a want of assimilation of the proper elements of healthy nutrition. Oil is one of the elements which disappears (from the external tissues at least) in an early stage of phthisical disease. It is, therefore, of some importance to fatten the patient; while, at the same time, the formation of muscle and fibre should not be lost sight of. The oils generally, and cod-liver oil in particular, have been shewn to have an important power of fattening both man and beast. Let their administration, therefore, be tried in phthisis, under various forms, before active curative efforts have become impossible. Under careful management, tubercular disease can be often arrested, and more or less com-

---

tary canal of persons dying from phthisis, and comparing it with what was observed in the bodies of those who died of other diseases. In this way, I have several times demonstrated, in the Pathological Theatre of the Infirmary, that there existed a marked difference in the amount of intestinal acidity, between the phthisical body and that affected with other lesions."

pletely suspended in its course. When a salutary change takes place, one of the first indications of it is usually the increased reception of nourishment, not only into the stomach, but into the tissues. A little later, the good effects of cod-liver oil, or other fattening substances, can be secured by the administration of them in small quantities, gradually increased as amendment proceeds. In these circumstances, whatever be the amount of faith reposed by the individual practitioner in this remedy, he will do well to attend to three cautions—1*st*, not to let his patient threaten to get *too fat* (as in one case before alluded to), but to stop short of producing a veritable *prize* man. 2*d*, not to give oils in any form where they are evidently not assimilated, or when they impair the appetite for more important alimentary matters. 3*d*, not to neglect *the tonic regimen*, in its widest sense, which should always accompany, if at all possible, the administration of internal remedies.

On all these points, we hereby declare our faith to be in accordance with that of Dr Bennett, and here we take leave of him for the present, thanking him once more for a work replete with interesting observations and ingenious reasoning. Although we have ventured to differ widely from many of his views, we acknowledge with all respect the advantage which the profession derives from the circulation of free thought upon a subject of such vast importance.

PART III.

PROGRESS OF MEDICAL SCIENCE.

I. SURGERY.

Luxation downwards and backwards of the three Internal Metatarsal Bones, a form of Dislocation of the Foot not previously described.

Instances have occasionally been recorded of dislocation of the metatarsus *upwards* upon the tarsus, but so rarely has the accident been observed, that it has attracted little of the attention of surgical writers.

An instance of dislocation of the metatarsus in the opposite direction, *downwards*, has never, as far as we are aware, been previously observed.

The case to which we allude is described by Mr Tufnell in the *Dublin Quarterly Journal of Medical Science* for February 1854. The case occurred in the practice of Mr Dolmage, surgeon of the 7th Dragoon Guards, and the accident is thus described by Mr Tufnell :—

"A trooper was returning off duty to Portobello Barracks, Dublin, on the 30th of November 1851, and was walking his horse cautiously, the road being very slippery from frost. Whilst turning a corner bordering on the canal, the animal suddenly slipped, and fell with his whole weight upon the soldier's right leg and foot, crushing it against the ground. The horse rose instantly, the man remaining in the saddle, but suffering such agony that, unconscious of what he was doing, he reined the animal back into the canal. Here a violent struggle ensued, the horse eventually disengaging himself from his rider, who, assistance being at hand, was dragged out and taken to his regimental hospital close by. He was seen by Mr Dolmage within a very few minutes of the accident having occurred, and before any considerable degree of swelling had taken place.

" The foot was found to be much shortened, curved inwards and bent, the tarsus presenting a hard bony projection, overhanging the metatarsus, whilst deep under the plantar structures a second bony mass could be felt lying obliquely across the sole of the foot.

"Reduction was at once attempted by placing the patient on his back, fixing the pelvis, flexing the leg upon the thigh, and extension then made by pulleys attached to the extremities of the foot and to the toes, and persevered in for a considerable time, during which every possible movement of the metatarsus upon the tarsus calculated to assist reduction was resorted to, and leverage also made upon the dislocated extremity of the metatarsal bone of the great toe, where projecting in the sole, by means of a ruler being applied to it, and drawn upwards and forwards, whilst the clasped hand of a powerful assistant placed upon the instep held that part downwards or backwards. As great a degree of force as it was considered justifiable to employ was expended in the effort at reduction, and continued for one hour, but not the slightest alteration in the position of the bones could be effected. Considerable effusion and ecchymosis followed, the latter extending up almost to the knee. Leeches, fomentation, &c,, were prescribed, and the ordinary treatment for violent contusions had recourse to. Under this treatment, swelling subsided; and ten weeks after the accident, all swelling and thickening had disappeared, the outline of the tendons, and every portion of the extremity, being most accurately defined. In its general aspect the foot somewhat resembled a case of *pes equinus*, being considerably shortened and arched upon its inner border, the distal extremity of the metatarsal bone and first phalanx of the great toe being adducted, the last phalanx at the same time pointing somewhat outwards. The instep presented a normal condition from the malleoli to the extremity of the internal cuneiform bone, which projected in a sharp point, raising the integument, which was stretched over it, white and glistening like a tightly bent knuckle; from the outward border of the cuneiform bone ran an evident ridge, making the division between the tarsus and metatarsus, and defining the line for Hey's amputation of the foot.

" The measurement of the injured member, as compared with those of the opposite foot, was the following:—Length of the dislocated extremity from the point of the great toe to the heel, 9½ inches; of the uninjured foot, 10¼ inches. Breadth of the dislocated foot across its widest part, at the base of the great toe, 4¼ inches, of the uninjured foot, 3½ inches. The extensor tendons of the injured foot stood out in strong relief, raising the toes; the tendons of the sound foot could be but indistinctly seen."

Six months after the accident the foot had "become more inverted, and the projection in the sole was less evident, having been rounded and partly removed by absorption. The patient walked freely with a stick, bearing his weight on the outer border of the foot, as in a case of *talipes varus*, but he could not make any effort at progression, or even move, when the foot was placed flat upon the ground, from a burning pain in the sole, and which he described as resembling the feeling that might be imagined to result from attempting to walk in a very tight boot with a marble under the sole of the foot."

In speculating on the mode in which the accident was produced, Mr Tufnell remarks, that " there were two occurrences immediately succeeding one another, in either of which the accident might have taken place. First, the fall upon the road, and second, the struggle in the water, the foot being entangled in the stirrup. I consider, however, that the excruciating pain experienced from the former places it beyond doubt that it was then that the injury occurred. I conceive that the horse falling on his side, the man's foot was crushed against the ground inverted, with the heel fixed and incapable of receding. In this position, the three inner bones of the metatarsal range would form the segment of an arch, unsupported, upon which the stirrup-iron, coming to act in a flattened direction (pressed horizontally by the superimposed weight of the body of the animal), drove these three bones downwards and backwards amongst the plantar structures beneath the tarsus, whilst the two outer bones, from resting on the ground, escaped detachment from their articulation with the cuboid bone, and in this way the luxation was produced."

The form of the extremities of the bones composing the articulation of the tarsus with the metatarsus must evidently produce great resistance to attempts at reduction of the dislocation. In the recorded cases of dislocation of the metatarsus upwards, reduction has usually been effected without very great difficulty; but the difference in the form of the bones on their upper and under surfaces is thus well described by Mr Tufnell as contrasting the facility of reduction in the one case, with the difficulty experienced in the case under consideration. " In the dislocation upon the dorsum of the tarsus the bones rest upon a smooth and oval surface, with nothing to hinder traction forwards, excepting muscular retraction. In that of dislocation downwards beneath the tarsus, on the contrary, the proximate ends of the metatarsal bones become locked in amongst the inequalities of the tarsal; and that of the great toe, in particular, becomes immoveably fixed behind the tuberosity of the internal cuneiform bone, wedged in the cavity which exists between it and the cuboid bone, and, held there by the *peroneus longus* tendon, it defies all efforts at reduction."

Mr Tufnell suggests the possible advantage, in such circumstances, of subcutaneous section of the tendon of this muscle, but does not hold out much prospect of this measure proving beneficial, as the locking of the metatarsal bones in the cavity of the tarsal arch appears to be the chief obstacle to reduction.

The employment of chloroform (that most powerful auxiliary in the reduction of dislocations) would probably assist to some extent the efforts to replace the bones, by causing relaxation of the muscles, the tendons of which seem to oppose reduction.

The following are among the contributions to surgery—derived from the late campaign in Burmah. We extract them from the first number of a new half-yearly journal, *The Indian Annals of Medical Science.*

Case of Lodgement of a Musket-ball in the left Ventricle of the Heart.

A private in H.M.'s 80th Regiment was wounded at the storming of the Great Pagoda. The ball entered a little above the anterior fold of the left axilla, taking an oblique direction to the cavity of the chest, from which, on his admission to the Shore Field Hospital, and for several days after, blood issued. Dyspnœa and emphysema had followed the receipt of the injury. The left side was dull on percussion, and the respiratory murmur slight, and only under the clavicle. The dyspnœa recurred with increasing severity from time to time, and was accompanied with hemoptysis. The action of the heart was weak, but natural; its systole and diastole regular and equal. Hectic set in, the patient became emaciated, and died about ten weeks after the infliction of the wound.

On dissection, " the passage of the ball through the pectoral muscle could not be observed; but its course appeared to have been between the third and fourth ribs, passing downwards to the left side, forming a cavity, which extended from the first to the seventh rib, from the spine to the cartilages of the ribs anteriorly; this contained a pint of pus. Left lung impervious to air throughout; a small portion of the cloth of the jacket was lying loose at the orifice of a canal situated about the middle of the lung in its convex aspect, which canal, passing forwards and inwards, stopped short close to the union of the pulmonary veins, where all further traces of the passage of the ball were lost. On raising the heart, however, a hard and firmly impacted substance was felt at the apex—this, on examination, proved to be the ball, in the left ventricle at its most inferior part, crossed and recrossed by the *cordæ tendineæ* and *columnæ carneæ*, which firmly secured it in its position. The heart was perfect in every respect; and the only conclusion to which we could arrive is that the ball must have perforated the pulmonary vein, and thus passed into the left auricle, and ultimately into the ventricle."

A case very similar to the above is detailed in the seventh volume of the " Transactions of the Provincial Medical and Surgical Association." In this case the foreign body was a piece of wood, and was found after death (which occurred five weeks after the receipt of the injury), loose in the cavity of the right ventricle. The piece of wood appears to have penetrated the walls of the heart, but, as in the case we have detailed, no trace of its passage into the viscus could be found on careful examination.

We think the entrance of the ball through the pulmonary vein and left auricle less probable than its having penetrated the muscular walls of the heart. In any view the case is very remarkable.

Traumatic Aneurism of the left Axillary Artery; Ligature of Subclavian.

W. R., from the steamer *Moozuffer*, was admitted into the Field Depot Hospital, Amherst, Jan. 20, 1853, under Assistant-Surgeon White (the same gentleman who reports the preceding case), with a gunshot wound through the left shoulder. The ball appeared to have entered immediately above the anterior fold of the axilla, and passing through the scapula, to have lodged in the infra-spinatus muscle, close under the spinous process, from whence it was extracted at Rangoon. He stated that the shot (musket) completely knocked him over, and that the hemorrhage was very considerable, but was readily checked by pressure in the armpit, which gave him great pain; also that several pieces of his jacket were taken out of the wound.

The wounds healed after the discharge of one or two exfoliations, but the patient continued to complain of severe pain along the course of the nerves in the arm, and a swelling appeared in the axilla about six weeks after the receipt of the injury; and a month later, when the swelling had acquired the size of a duck's egg, feeble pulsation was perceived in it. These symptoms increasing, and the pulsation having become very distinct, the subclavian artery was tied on the 24th of April. Some sloughing of the fingers followed, but otherwise the patient progressed favourably. The ligature separated on the fifteenth day, and the patient made a good recovery.

Some doubt may be entertained as to the propriety of the practice which was here followed. A traumatic aneurism with a green cicatrix on its surface will not always be found to be so amenable to the treatment by ligature on the Hunterian principle as it proved to be in this case. According to the principles which have been so strenuously inculcated by Mr Guthrie, the aneurism should have been treated by the "old operation," free incision of the tumour, and the application of ligatures above and below the wounded point in the artery. The successful issue of the case, however, may be urged as the best proof of the propriety of the practice which was followed; and we freely allow the difficulty of adhering strictly to rule in cases of the kind, as well as the temptation to resort to the less formidable, and, in its immediate consequences, the less dangerous proceeding of applying a single ligature on the cardiac side of the aneurism.

Amputation at the Hip-joint.

A Burman, aged 30, was attacked by Dacoits, February 15, 1853, and received a gunshot wound of the left thigh. He was admitted on the following day into the Jail Hospital, under the care of Dr Fayrer, assistant-surgeon, Field Hospital, Rangoon. The slugs, with which the musket had been charged, had entered a little below the trochanter major, and had shattered the shaft and neck of the bone, and had given rise to copious hemorrhage.

Amputation was immediately performed, and as the head and neck of the bone were much comminuted, disarticulation was effected. Chloroform was employed, but the shock of the operation appears to have nearly proved fatal. The patient, however, rallied in the course of a few hours, and progressed favourably till the sixteenth day after the operation, when symptoms of tetanus appeared, which led to a fatal termination a month after the performance of the operation, the wound being healed except at one small point.

Excision of the Knee-joint.

The question of excision of the knee-joint, as a substitute for amputation of the thigh in many cases of disease of the knee, was again brought forward by Mr Mackenzie at a late meeting of the Medico-Chirurgical Society of Edinburgh. The opinion that "the operation, till its recent revival, had been banished from surgical practice on insufficient grounds" seems to be gaining ground. Mr Mackenzie exhibited to the Society a man (between forty and fifty years of age), on whom he had a year previously performed the operation. The limb was now straight, anchylosed, perfectly sound, and not more than three-fourths of an inch shorter than its fellow. The patient walked stoutly and rapidly, and could lean his entire weight on the limb.

Mr Mackenzie sought to confirm his favourable opinion of the operation by the results which had been obtained in the hands of several distinguished surgeons who had recently performed it, and from whom he had received letters on the subject, extracts from which were read to the Society.

The results of the operation, since its revival by Mr Fergusson in 1850, are as follow:—The operation has been performed in *eighteen* cases, of which *fourteen* have recovered, and *four* have died. In *eight* of the successful cases the limb is used freely in progression, and easily bears the weight of the body; in *four*, the result promises to be equally satisfactory, but the operation has been too recently performed to allow of the limb being made use of; in *one*, the result as regards the usefulness of the limb is less satisfactory, the support of a splint being required; in *one*, the patient died suddenly from an apoplectic seizure three months after the operation, when the parts were healed, but before the limb was used in walking.

The operation is strongly opposed by Mr Syme, to whom surgery is so much indebted, as regards the operation of excision of the elbow-joint; but the facts adduced in its favour are very strong, and we confidently expect that, ere long, it will be generally recognised as one of the established improvements of conservative surgery. The value of the operation will not, for obvious reasons, bear comparison with that of excision of the elbow-joint; but it is not, on this account, the less to be regarded as an advance in surgery, if it is proved to be the means of saving life and preserving a serviceable limb.

An account of some of the cases in which the operation has been recently performed, and of the opinions of those who have practised it, is given in the *Association Medical Journal* for March 10, 1854.

II. MILITARY SURGERY.

Sir George Ballingall, the very able and zealous Professor of Military Surgery in the University of Edinburgh, has for many years strongly advocated the establishment of a course of lectures similar to those delivered by himself at Edinburgh, in the Medical Schools of London and Dublin. That he has not selfishly sought to retain a monopoly of teaching this important branch of an army surgeon's education, clearly appears from the following short extract from his "Outlines" published fourteen years ago.

"In London, under the eye of the Director General, and with the aid of the valuable museum which he has established at Chatham, such a course would be conducted with peculiar advantage. In Dublin, from the extent of the garrison, and the number of troops employed in that part of the United Kingdom, facilities of the most important kind would be given for such a course; and in Edinburgh, where the medical students are numerous and closely concentrated, experience has already proved that there is sufficient field and sufficient encouragement for a course of military surgery."

It will be seen from the following extract from the speech of the Secretary-at-War, the Honourable Sidney Herbert, on proposing the army estimates this year, that this subject has at length attracted the attention of the authorities at head quarters.

"The honourable and gallant member for Westminster had very truly said that there was not at present any sufficient means of education for the specialties of the medical service of the army. (Hear.) In consequence of this deficiency he (Mr Herbert) had had made inquiries at Paris and at Vienna as to the practice which obtained there with regard to the special military education of those who were candidates for the post of army surgeon. At Paris they had gone to an extreme which he believed had been found highly inconvenient. They had taken the medical student from the beginning, and instructed him exclusively in military surgery, forgetting that, in nine cases out of ten, military surgery was just the same as ordinary surgery. (Hear.) At Vienna and in other countries they had, on the contrary, very much neglected the special education of army surgeons, and that was the case with us. Edinburgh was the only place where it was possible to get a special education for army surgeons; and it was proposed, therefore, to institute a course of lectures upon military surgery, for which means would be found both in Dublin and London. There was no doubt that there was no army which demanded this more than our own. Last year there was a sudden terrible outbreak of yellow fever at Barbadoes, a colony which had hitherto been considered one comparatively healthy, and the freest from that terrible scourge of all the West India Islands, with which the army surgeons there were not at all able to deal; and it was most necessary, therefore, that before young men became army surgeons they should have the means of making themselves acquainted with the nature of the different diseases incidental to the climates where our soldiers are sent, as well as the diseases to which soldiers, from their habits and mode of living, might be peculiarly liable. (Hear, hear.) It was proposed, therefore, to extend to London and Dublin the advantages of the establishment which already existed at Edinburgh, and it was hoped that arrangements would be made immediately for a course of lectures which would not only be most useful for those who were candidates for the profession, but to which a great number of those who were already army surgeons would gladly have recourse, for the purpose of adding to the stock which they already possessed, and to which they very often would be able to contribute by their own experience."—*Times*, Friday, Feb. 25, 1854.

Mr Herbert's observations must give general satisfaction in more than one respect, for they completely set at rest a suspicion, not wholly at one time, as it would appear, without foundation, that there was a design to supersede the just claims of Dublin and Edinburgh, by establishing one great school of Military Medicine and Surgery in London. Centralization may be carried too far in matters of education as well as in other departments of the public service. Such a monopoly of medical education for the service of the army, established in London, would have been unjust in the extreme, and would have defeated the purpose in view, by taking away the very soul of improvement in education—the principle of emulation among rival schools.

III. MIDWIFERY.

We observe, in the fourth volume of the transactions of the Pathological Society of London, 1853, several important contributions to obstetric science, of which we may notice the following:—

Fibrous Tumours of the Uterus.

Two cases of *uterine fibrous tumours* are carefully reported by *Dr Bristowe*, whose investigations into their minute structure go far to establish the view entertained by Vogel and others, regarding the muscular nature of their elements. In both cases the patients died shortly after delivery, and, consequently, when the elementary tissue of the uterus possessed a characteristic appearance, similar to other involuntary muscular fibre. The tumours were found, on microscopic examination, to consist almost entirely of bundles of fibres, which resembled those of the uterine parietes so much, that it was impossible to say of any specimen, whether it was taken from the normal uterine structure or from the adventitious structure constituting the tumour.

The observations of *Dr Bristowe* while important, as valuable contributions to the natural history of such growths, derive additional interest from their practical bearing, inasmuch as they give us a reasonable explanation why it is that such tumours should enlarge during the progress of utero-gestation. It still remains to be determined by further observations, whether or no such be the origin of the various kinds of uterine fibrous tumours.

Fatty Degeneration of the Uterine Walls.

Dr Bristowe also relates a *case of fatty degeneration of the muscular fibres of the uterus* after delivery, confirming the opinion held by several eminent physiologists regarding the nature of the process, whereby the organ resumes its former size and structure,—viz., by the deposition of fatty granules within the substance of the muscular fibres, at the same time that the nuclei become absorbed, and the fibres resume the appearance they presented before impregnation.

Fatty Degeneration of Placenta.

An interesting case of true *fatty degeneration of the placenta*, which is so often confounded with the pale anæmic state of the organ, is given by *Dr Handfield Jones*. The placental villi were crowded with fatty and calcareous granules, and their vascular loops were observed in many instances to be obliterated. *Dr Jones* is of opinion, that the diseased condition of the placenta commenced in the maternal portion, and that the fœtal portion was only secondarily affected.

Adventitious Structure in Ovarian Cyst—Effects of Temperature on Contents.

Several other cases of ovarian and uterine tumours are recorded, in one of which the cyst was found to contain a quantity of hair, imbedded in a fatty matter, which when cold was of a solid consistence, but on being heated to the temperature of the body, became quite fluid, and which would, therefore, in all probability, have given the feeling of fluctuation to the tumour during life.

Inversion of the Uterus during Labour.

At a recent meeting of the Dublin Obstetric Society, an interesting case of *inversio uteri* was communicated by Dr George Johnson, under whose notice the case occurred some time ago, in the Dublin Lying-in Hospital. The patient, a thin delicate-looking woman, aged nineteen, gave birth to a healthy first child, being about six hours in labour. The gentleman in attendance on the case, after tying the cord, exerted pressure for some time over the uterus to maintain its contraction. This pressure was kept up for a quarter of an hour, when finding a tendency to "draining" the pressure was increased. While thus grasping the fundus firmly, he felt it suddenly recede from his hand, and immediately discovered its expulsion from the vagina by inversion, the placenta being still attached. On the arrival of Dr Johnson he found the woman in a state of collapse, complaining of considerable pain. The placenta, which was adherent to the fundus of the inverted uterus, was, without delay or difficulty, detached. No hemorrhage occurred, either before or subsequent to the separation of the placenta from the inverted organ, which was reduced first by replacing the cervical portion within the vagina, and re-inverting it as far as the body, which was easily effected. The fundus, however, required several minutes' pressure upwards by the flexed fingers of the right hand before it was restored to its position, counter-pressure being at the same time made with the left hand over the pubis.

Stimulants had been previously administered to relieve the patient's sense of great exhaustion, but the feeling of immediate dissolution did not leave her until the uterus had been restored to its natural position. Ergot of rye and the horizontal posture were prescribed, and the patient made a perfect recovery.—*Dublin Quarterly Journal of Medical Science*, February 1854.

Retroversion of the Uterus—Pregnancy—Death.

Mr Isaac B. Brown reports to the Royal Medical and Chirurgical Society, London, a case of supposed *irreducible* retroversion of the uterus in a young woman, aged 20, who had suffered from prolapsus uteri, produced by lifting a heavy weight. She subsequently became pregnant, and was first forced to seek medical relief for the violent straining she suffered during micturition. When subsequently admitted into St Mary's Hospital, the anus was found to be open and the rectum protruding. Distension of the perineum also existed, and the labia were separated so as to allow an oviform body to be seen. A large tumour was discovered, on examination, behind the posterior wall of the vagina, anterior to the rectum. The tumour filled the whole pelvic cavity. The os uteri was pressed forwards on the neck of the bladder, the posterior lip of the os being inferior to the anterior, which was displaced upwards and forwards. The fœtal movements, indicating an advanced state of pregnancy, were distinctly felt. From the urgency of the symptoms which rapidly followed her admission into the hospital, surgical interference was deemed imprudent. Vomiting of dark grumous matter and death ensued three days after admission.

On *post-mortem* examination, peritonitis was found to exist, the bladder was much dilated, flattened, and adherent to the anterior abdominal wall, its mucous membrane appearing disorganized. On examining the uterus, it was found to occupy the cavity of the pelvis, in which it was moulded in a retroverted condition, with its fundus pressing against the posterior wall of the vagina in the hollow of the sacrum. The os uteri was found in firm contact with the neck of the bladder, high up behind the arch of the pubis. On opening the uterus a five-months' fœtus was found, presenting by the breech.—(*Association Journal*, March 3, 1854.)

Instrumental Labour. Different Results of,

Dr F. H. Ramsbotham has published the particulars of two forceps cases that recently occurred in his practice, and which are interesting as showing the different effects of instrumental interference in laborious labour. In the one case, the labour (a first one) had continued for six days, and the membranes had been ruptured 75 hours before delivery was accomplished by means of the forceps; this patient made a quick and perfect recovery. In the other instance referred to by Dr Ramsbotham, the mother had previously given birth to nine children; the liquor amnii had only been evacuated five hours when the forceps were applied to terminate the labour. In this case a fatal result ensued. In both, extraction of the head was effected with less than the ordinary degree of difficulty.

These cases, though interesting as illustrating the necessity and importance of attending to the *effects of the labour on the patient*, and not merely to the *time she has been in labour*, are to be considered quite exceptional in their character and results.—(*Medical Times and Gazette*, March 4, 1854.)

IV. PRACTICE OF MEDICINE.

Medical Facts connected with Emigrant Ships.

In a very interesting communication to the *Glasgow Medical Journal*, Mr. James Fraser, Government Medical Inspector of Emigrants from the Clyde, touches on some highly important topics. One of the duties imposed by the Passenger Act on the inspector is to prevent any person from embarking with symptoms inferring the suspicion of an approaching contagious disease. This regulation, though inflicting occasional hardship in the case of the suspicion turning out to be unfounded, appears to have been attended with the happiest effects on the health of the ships. During the prevalence of the last epidemic typhus, before the Act came into operation, the ravages of that disease on board such ships appears to have been nearly incredible. During the year 1847 and the first half of 1848 typhus attacked every crowded ship between the Clyde and America, the disease breaking out sometimes before land was lost sight of, sometimes not till the American shore was in sight, and at every intermediate point in the 3000 miles intervening between Britain and the new world. Ships carrying a small number of passengers in the same voyage escaped; and ships from the Clyde with a smaller number of better fed passengers for the Cape and Australia, at the same period, were wholly unvisited by this terrible scourge. Mr Fraser suggests that these facts go far to prove that it is occasionally in such circumstances a disease *de novo*. We think there is no sufficient ground to infer that the disease ever sprung up spontaneously in these crowded ships, even in the case where it only appeared at the end of the voyage. Had there been no epidemic on shore at home, and had the disease in general made its attack towards the end of the voyage, the case would have been strong for Mr Fraser's supposition. But even were such the state of the case, several points require consideration before the primary influence of contagion is dispensed with. In the interval between two epidemics of typhus, does that form of fever disappear entirely from our great towns? Most probably not—as we think, certainly not. For a number of years past Edinburgh has never been more free from fever than at present. The number of cases of fever reported for the week ending March 18, in the Royal Infirmary, was no more than five. Nevertheless among these cases there are unequivocal instances of typhus, though of a mild character. Had one or two of these parties, while the disease was still latent, embarked in a crowded emigrant ship, three or four weeks ago, the disease might have escaped attention, as passing for ephemeral fever or febricula till it began to attack numbers in the middle or towards the end of the voyage. The exact latent period of typhus is still unascertained. If the primary effect of contagion be acknowledged in the origin of this disease, it is unphilosophical to assume another essential cause so long as any of the circumstances possibly influencing the operation of the ordinary cause in a particular case remain undiscovered.

The mortality in former times among children and young people in these ships, from smallpox and other exanthematous diseases, sometimes amounted to the frightful extent of 35 per cent. The examination to which the Act now subjects passenger-ships before sailing, and the introduction of steam-vessels for the ordinary service of emigration, will probably put an end to such disastrous consequences for the future.

We have room for only one further fact from Mr Fraser's paper. It appears that such has been the popular persuasion of the suitableness of the climate of Australia for the cure of phthisis, that more than one-half of the whole mortality on the passage from the Clyde to Melbourne and Sidney, within the last three years, has arisen from phthisis. Mr Fraser is desirous to call the attention of his medical brethren to this fact, with a view to their dissuading parties far advanced in that disease from undertaking such a voyage under hopeless circumstances.

Affection of the Heart, Thyroid Gland, and Eyeballs. By Drs Romberg and Henoch of Berlin.

The peculiar disease in which the symptoms of a disturbed circulation and action of the heart are associated with a remarkable enlargement of the thyroid gland, and with a most characteristic prominence of the eyes, has become, of late years, familiar to the profession through the writings of British authors, especially of Graves, Marsh, M'Donell, Begbie, Stokes, White Cooper, Walton, &c., &c. In Germany, too, the disease appears to be well known, and many examples of it have been observed by Pauli, Brueck, Basedow, and lastly by the authors whose interesting paper we have now before us—Romberg and Henoch. Though differing in regard to the etiology of the disease as a whole, and disagreeing to a certain extent in the account given of the rise and occasion of its individual symptoms; still, in the descriptions of all the writers now named, there exists so remarkable a uniformity, as to satisfy us of the identity of the disease which each has observed.

We shall, in the first place, make our readers acquainted with some of the cases in an abridged form, and the remarks of Romberg and Henoch, and then add a few observations of our own, which the perusal of the former have called forth.

Case 1. A. S., aged fourteen,[*] who had never menstruated, was treated, in the clinical ward, for anæmia, and cured by a preparation of iron. In October 1849 she again became a patient, her former disease having returned. At that time the extraordinary paleness of her skin revealed her anæmic condition. The right lobe of the thyroid was swollen, and the jugular vessels were seen pulsating. The anæmic sound was clearly audible in the neck. There was the evidence of an enlargement of the heart, and its first sound was at the base accompanied by a bellows murmur. The patient suffered from dyspnœa, increased by motion, and from great weariness. The bowels were irregular. On the 12th November she was ordered to take iron, which, with a short interruption, she continued to do till January 1850. At that date a decided improvement in her whole system was visible.

Case 2. A girl of eighteen, who had first menstruated a year previously, began to complain of violent palpitation and uneasiness in the region of the heart, brought on chiefly by exertion, especially in ascending stairs. At the same time a swelling had appeared in the front of the neck, and at times she expectorated blood. The diagnosis, after examination of the heart, was that something more than functional disorder existed, and that valvular disease was present. In this patient, as in the former one, the enlargement of the thyroid and the affection of the heart existed, but the eyes were not implicated. She differed from the former case in not presenting an anæmic appearance.

Case 3. Mrs R., aged forty-seven, subject for many years to numerous hysterical complaints, was admitted into the Clinic May 18, 1849. She complained especially of violent palpitation of the heart, and consequent agitation. During the attacks of palpitation, she experienced a feeling of tightness in the throat, and a glimmering before the eyes. The thyroid was evidently enlarged, particularly in its right lobe, and in it she experienced a sense of pulsation and of pain during the occurrence of the palpitation. Then, also, the eyes became unusually large, and appeared starting from their sockets in such a manner as to expose her to the laughter of bystanders. The catamenia were regular, but scanty. The pulsations of the heart were increased to 100 in the minute, but otherwise there was no change detected. She was hysterical, suffered from weariness and from irritability of temper, with inclination to weep. Although the symptoms had existed for nearly two years, they had considerably increased during the previous four months. In this patient the manifestation of anæmia was very clear. A mixture of digitalis, with phosphoric acid and valerian, were the remedial means employed, and amendment was so speedy, that in July of the same year she was able to take a situation as lady's maid, and went to Dobberan, where she experienced great benefit from the use of the sea baths. In March 1850 she was seen entirely freed from her former ailments.

Case 4. C. L., aged twenty, first menstruated at the age of thirteen, and suffered thereafter from complete amenorrhœa for a whole year. During this period she had a trifling swelling on the front of the neck. Menstruation again returned, but very irregularly, and the girl, from being blooming and robust, became pale and weak; the thyroidal swelling increased; palpitation of the heart succeeded, and then followed a strange largeness of the eyes. Considerable benefit was obtained in this case from the steady use of iron.

Case 5. Mrs B., aged twenty-five, presented herself at the Clinic, on July 7, 1848. Always healthy, and having regularly menstruated. She had for three weeks been occasionally exposed to a draught, when washing, with her neck uncovered. She had felt pain in the front of the neck, and had latterly noticed a slight swelling in the situation of the thyroid. On examination at the Clinic, the same remarkable prominence of the eyes as noticed in the other cases was observed, still the sight was not affected. Violent action of the heart existed; and in the thyroidal tumour, now greatly increased, and very large, as also in the head, the pulsation was inordinate. The pulse was 144. Three cups of blood were taken, and in the following week leeches were applied to the enlarged thyroid; from these measures an evident, though temporary, amelioration followed. Afterwards (early in 1850), from the use of digitalis, and due in great measure to the regular return of the menses, which had been much interrupted, restoration to sound health occurred.

Case 6. A. B., a young girl, aged seventeen, had suffered from violent palpitations for two years; had also an attack of typhus fever, and had been neglected, owing to living in a country village. Was admitted into the Clinic, November 1847, when her appearance was most striking. The eyes were protruded; the sight, however, unaffected. The thyroid was greatly enlarged, and appeared throbbing; a loud systolic murmur was heard, and peculiar thrill felt over it. In this case the occurrence of the palpitation was evidently followed by increase in the size of both thyroid and eyes. Pulse equal and regular—116. Bloodletting was adopted three times in the treatment of this case, and after a little time a leech was applied every fourth day to the enlarged thyroid; as in the former case, a temporary benefit resulted. The girl appears to have ultimately fallen a victim to tubercular disease in the chest, surviving the time of her first illness for nearly three years.

Taking the cases which have occurred in their own experience, and those which have been elsewhere recorded, there are in all twenty-seven which form the subject of the following interesting remarks by Drs Romberg and Henoch. They acknowledge twenty-seven to be too small a number of observations to warrant any very decided opinions being drawn from them, but at the same time believe it sufficiently large to afford the groundwork for much useful study. By far the larger number of the patients were females, only four of the twenty-seven were males. All with one exception were young, the most common age being between twenty and thirty. In the larger number of the cases there existed the combination of the three symptoms of palpitation of the heart,—enlargement of the thyroid, and prominence of the eyes; while in six of the twenty-seven cases one or other of the three was absent. In the two cases first treated by Dr Romberg in the Clinic, the prominence of the eyes was not observable. Undoubtedly of these phenomena the palpitation of the heart is the one best understood, and corresponds most readily with the view taken of the whole disease. Almost always the cardiac symptoms are those first discovered and first complained of; then, after a longer or shorter period, the swelling in the neck commences, and the prominence of the eyes follows. Only a few of the recorded cases lead to the supposition that the three diseased appearances arose at about the same time, certainly in the fifth case treated in the Clinic, the enlarged thyroid was the first symptom noticed. It is well to inquire wherein the original affection of the heart consists. That in some instances there exists organic disease is proved as well by examination during life as by *post-mortem* examination (Basedow and Marsh); but again in others it is equally certain that the cardiac symptoms depend merely on an increased irritability of the organ (Cooper, Begbie, Lubarsch). Cases explicable on both these grounds, and on these only, have been treated by the Clinic. In regard to the thyroidal swelling, it is interesting to note its increase and subsidence after the violence of the cardiac palpitation—this fact is expressly stated by Sir Henry Marsh, and mentioned by Begbie as existing in his third case, and also noticeable at times in the fourth; this of course points to an intimate relation between the two symptoms. Marsh and Heusinger describe the condition of the thyroid as a true hypertrophy, and it has been noticed by Graves, that after the lapse of years the consistence of the gland has been much increased.

As regards the remarkable prominence of the eyes, this symptom comes on gradually, and so far as vision is concerned it is not of much importance, seeing that only in one case (Lubarsch) was it at all seriously impaired; but though sight is not much affected, the prominence of the eyes produces a singular disfigurement, causing the sufferer to be not unfrequently avoided in company. Acknowledging the great difficulty of determining upon what peculiar condition the prominence of the eyes depends, our authors set aside the view of the increase of the aqueous humour causing a true enlargement of the eye (Begbie—the theory also adopted by Dr Stokes), also that which attempts a solution by reference to an hypertrophy of the post-ocular cellular tissue (Basedow). They look upon the idea of the prominence being due to a want of tone in the ocular muscles, and an accompanying congestion in the posterior parts of the eye (Cooper, Dalrymple) as more likely; but they appear to think still more favourably of the view of Heusinger, who found in two cases an extraordinary accumulation of fat in the cellular tissue behind the eyes, and regards it as the probable cause of the exophthalmos.

Again, regarding the disease as a whole, our authors proceed to remark, that certainly the larger number of the individuals so affected exhibited evident symptoms of anæmia, such as a remarkable paleness of the skin, the peculiar sound audible in the bloodvessels of the neck—headaches often very violent—giddiness, especially when in the upright posture—humming sound in the ears—attacks of fainting—small frequent pulse, &c. Irregularity of the catamenia also is commonly present, while fluor albus, and sometimes complete amenorrhœa, have been found. Symptoms of an hysterical nature further distinguished not a few of the cases, the globus hystericus, neuralgic pains in different parts, coldness of the extremities, and strange wanderings of the mind. Basedow describes a remarkable calmness and a great desire for pleasure as characteristic features of the mental condition. In some of the cases it is clear that if the disease was not originated, at all events it was furthered by the occurrence of a severe hemorrhage or flux, which reduced the system (Begbie); also a depressed state of both body and mind seemed connected with its first occurrence (Graves). But though anæmia was present in a large number of the cases, there are others whose commencement could not be traced to it, and anæmia cannot therefore be regarded as an essential requisite towards the explanation of the complex phenomena. The irregularity in the uterine system, too, cannot be regarded as altogether explanatory of the disease; for, independently of males being subject to it, these uterine derangements, though marked, were of very varying nature. Heusinger directs attention to the condition of the spleen, which he found after death much increased in volume and manifestly diseased.—*Klinische Wahrnehmungen und Beobachtungen.*

Remarks.—This peculiar disease is certainly not uncommon. To the twenty-seven cases which formed the groundwork of Drs Romberg and Henoch's observations a large number seen by various medical men could now be added. So far as our own observation of its whole phenomena goes, we agree in the opinion first suggested by Dr Begbie, of their origin and dependence being due to an altered or impoverished state of the blood,—in short, to anæmia. No doubt the degree of the anæmia in such cases will greatly vary, but we do not know of any case in which it could certainly be said not to exist; and even in those cases in which the anæmic appearance of the patient is least marked, it must not be forgotten that the irritable and excited heart, the peculiar sighing sound heard in the cervical vessels, and many of the other symptoms which accompany these, are just as truly evidences of anæmia, as the paleness of the face and general surface. In regard to the exact cause upon which the enlarged thyroid and protruding eyes depend we shall not here enter, as we still consider this part of the subject to be obscure. We observe that Mr Mackenzie of Glasgow is disposed to attribute the exophthalmos to a varicosity of the ophthalmic veins, and Mr Walton to effusion into the ocular sheath, while Dr Stokes takes the same view as Dr Begbie, and regards the eyes as affected by a true hydrophthalmia.

We beg to refer our readers to an interesting chapter on this subject, in the recently published work of Dr Stokes on diseases of the heart. Curiously, as it appears to us, Dr Stokes never mentions the term anæmia, yet evidently describes the anæmic condition, both in giving his interesting cases, and his remarks upon them. It is of great interest to note the curability of the disease, as exemplified in some of the cases treated by Dr Romberg. Indeed, just as in the instances given by Dr Begbie, cure seems in some cases (chiefly due to iron) to be equally speedy and complete. That in other instances the reverse is the case is too true, and nothing is more certain than that this peculiar disease, commencing as it undoubtedly does in functional derangement of system, is not unapt to end in structural change and irremediable organic lesion. In this way it is that the existence of organic disease of the heart or other important viscera is to be explained.

Successful Treatment of Acute Rheumatism by the Acetate of Potass.

By Dr Golding Bird.

In the last number of the Journal, we alluded to Dr Bird's interesting remarks on renal depurants; and here we have merely to present our readers with the same author's experience in regard to the treatment of acute rheumatism by *one* of that class of remedies, namely, the acetate of potass. On this point Dr Bird uses very strong language, affirming, that he has never seen the disease in question yield with so much facility to any other remedy. In the several cases in hospital practice under his own care, Dr Bird has seen the cure to be more rapid, and the immediate relief to the patient more marked, by the use of acetate of potass in quantities of half an ounce administered, largely diluted, in divided doses in twenty-four hours, than by any other treatment. In three days he has frequently found the exquisite pain of the joints nearly absent, the patient comparatively comfortable, and able to bear with greater ease the helpless state in which the still swollen joints placed him. In no case have any bad consequences succeeded the employment of the remedy, while complete recovery has been more expeditious, and the ill effects of colchicum and mercury have been avoided. The most interesting and no less important fact has been noted by Dr Bird in his experiments with this salt, namely, that the pain of the disease remarkably declines so soon as the urine becomes alkaline and rises in specific gravity; and he further records his impression, that the tendency to affections of the heart is very much lessened after the alkalinity of the urine has been established.

Dr Bird's experience of the efficacy of the acetate of potass in the treatment of acute rheumatism has been derived from a large number of cases.

The only adjuvants he has employed have been a mercurial laxative, if constipation existed, and a full dose of Dover's powder on the first day or two of treatment, if the pains be severe; the joints being wrapped in sheets of wadding, a plan the author has followed since he experienced in his own person the comfort it affords.—(*Urinary Deposits*, 4th edition.)

V. PHYSIOLOGY.

On the respective Offices of the Minute Biliary Ducts and the Cell of the Parenchyma of the Liver.

Dr C. Handfield Jones has communicated to the Royal Society of London a paper on the liver, supplemental to a former communication presented to the Society in 1847.

The chief purpose of the paper is to point out what he conceives to be the separate offices of the hepatic cells and the minute biliary ducts. Our space will not permit us to give more than his chief conclusions, along with a few of the more general grounds on which these rest.

His chief conclusion is that the bile—that is bile in its complete and perfect state—is not formed in the cells of the parenchyma of the liver. He regards these cells as having a separate office in the animal economy, connected with the preparation of material for the function of respiration, His next great conclusion is that the bile is formed in the minute bile-ducts; that these minute bile-ducts have no communication with the cells of the parenchyma, but that when they are traced to their origin within the mass of the liver their extremities are found to be closed. Hence, that even if the bile were produced within the cells of the parenchyma, there is no channel by which it could make its way into the minute ducts.

He regards then the cells of the parenchyma as representing a ductless gland, like the suprarenal capsule, allowing its product to return to the blood from which it has just been elaborated, while he infers that product to be chiefly sugar, which, returning into the circulating fluid, probably undergoes farther changes before, in the process of respiration, it passed into carbonic acid. He further considers the certainty of the constant production of sugar in the cells of the hepatic parenchyma as a strong *prima facie* evidence that the bile-secreting function belongs to another apparatus associated with it, namely that of the excretory biliary ducts.

"The hepatic cells," he says, "differ in several particulars from the cells of other glands; they are more perfectly formed, of more permanent aspect; they are not disposed as a lining to tubes of homogeneous membrane, but in series which tend more or less to plexiformity, and are apparently distant from any free surface open to the exterior. I consider them, therefore, to form a *parenchyma*, and not an epithelium. Their peculiarly intimate relation to the blood capillaries seems to indicate, as I suggested in my former paper, that they serve as repositories for certain matters absorbed by the blood from the chyme as it passes over the intestinal surface; these matters are, however, doubtless altered by the recipient cells and converted into sugar, perhaps also into oil or biliary matter." By biliary matter, Dr Jones here understands, it would seem, merely the colouring matter of bile. The proof that sugar is made in the liver, most insisted on by Dr Jones, is that it is not found in the blood entering the liver by the vena portæ, while it exists in very large quantity in the blood passing out by the hepatic vein.

Dr Jones insists on the small amount of ramification presented by the minute bile-ducts, and on the little relation they hold to the walls of the parenchyma; he says also that these minute tubes are not, like tubes of mere conveyance, free in their cavity, but filled with epithelium, while the larger ducts are interiorly quite smooth.[1]

---

[1] Philosophical Transactions for 1853, vol. 143.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL.

1st JULY 1854.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Sketch of the Life and Character of Dr Abercrombie; read before the Harveian Society of Edinburgh, at the Annual Festival, 12th April* 1854. By Douglas Maclagan, M.D., F.R.S.E., &c.

It is part of my duty this evening to deliver the address which the laws of the Harveian Society require from the occupant of the chair at the Annual Festival. I am glad, on my own account, to observe that our Secretaries, in convening this meeting, have so far departed from the strict letter of the law, as to call it a Harveian Discourse instead of a Harveian Oration,—for the term oration implies oratory, and that is a gift to which I have no pretension. The law leaves the kind of subject to the choice of the writer; but the practice of my predecessors in this chair has generally been to submit to the Society a biographical notice of some distinguished member of the medical profession. I have not felt myself entitled to depart from this time-honoured custom, and I have, therefore, to solicit your attention, for a short while, to a physician whose eminence entitled him to an earlier commemoration at a Harveian festival, and to a writer more capable than I am of doing justice to his memory. I refer to Dr Abercrombie. With your attention, I crave your lenient criticism, both because biography is a kind of writing to which I am totally unaccustomed, and for which I am certain that I do not possess the requisite qualifications, and because, as regards the special subject of my present remarks, I am conscious that I am addressing many who are better qualified, in every respect, to be my informants than my auditors. I might perhaps have acted wisely in avoiding biography altogether, or in selecting for my theme some departed worthy less known to my audience; but I have felt very strongly that a Harveian notice of Dr Abercrombie has been already too long delayed, and I have, therefore, with all my sense of incompetency for the duty, resolved to undertake it, and to throw myself unreservedly on your kind indulgence.

I have not proposed to myself to enter into minute details of the daily life of Dr Abercrombie. I do not myself possess, nor have I sought from others, the means of doing so. No doubt, the biography of an eminent man, when treated in this way, may be made highly interesting and instructive. It reveals to us glimpses of character which we never could obtain from the contemplation of the man in public life. It is, if I may borrow a modern phrase, the histology of his moral organization. We dwell with pleasure upon the little incidents of his domestic and social life, and contrast them with the largeness of his public doings, just as we eye with admiration the delicate striæ on the fibril, and contrast it with the bulk, and power, and muscular energy of the limb. But this microscopic anatomy of an eminent character requires for its demonstration the hand of one, whose opportunities of close contemplation have often brought the minutiæ of its structure under his eye. To such a qualification, in relation to Abercrombie, I cannot have the remotest pretension. I knew him as a practical master of my art, as an instructor in my hour of study, as a monitor in my moment of serious reflection. It is in these points of view that I now propose to regard him; and the following abstract of his personal history is offered merely as the necessary introduction to a review of his character as a physician, an author, and a man.

John Abercrombie was born on 11th October 1781 at Aberdeen, where his father, the Rev. George Abercrombie, was minister of the East Parish Church. The early part of his education was received, first, at the Grammar School of Aberdeen, and, subsequently, at Marischal College and University, where he studied for four years, and took the degree of A.M.

Having embraced medicine as his profession, he repaired to Edinburgh to pursue his studies, and was placed as a boarder in the family of the Rev. Mr Black, at that time clergyman of Lady Yester's Church. Under this roof he continued to reside during the whole period of his academic career. For Mr Black he maintained an uninterrupted affection; and to him, along with Professors Gregory and Hope of Edinburgh and French of Aberdeen, with the last of whom he had served his apprenticeship, Abercrombie dedicated his thesis.

He took his degree of M.D. at Edinburgh in June 1803, the title of his thesis being "De Fatuitate Alpina." This dissertation is fairly entitled to the appellation of a carefully written essay, but we fail to trace in it much promise of those qualities as a medical author, by which Abercrombie was distinguished in his riper years. The subject was one upon which he could offer no personal observations, for he never had visited a cretin district, and he does not attempt anything original in the way of speculation regarding Cretinism. The chief thing which can be supposed to have conferred any particular merit upon it, is its giving an account of the observations of Foderè, whose treatise Du Goitre et du Crètinisme was published in 1800, and was probably at this time not very familiar to English readers.

After his graduation, Dr Abercrombie went for a short while to London, where he studied at St George's Hospital, and returned to Edinburgh in 1804, on the 12th November of which year he was admitted a Fellow of the Royal College of Surgeons of Edinburgh. At this time, the college had adopted the course of demanding from each candidate for the fellowship a probationary essay on some surgical subject, and Dr Abercrombie was the first who was called upon to comply with this practice, which has been abandoned only within the last two years. The subject which he selected for his probationary essay, was Paralysis of the Lower Limbs from diseased spine. It is no disparagement of the author to say that it does not contain anything that was original or novel, for in truth the literature of the subject even at the present day stands very much where Abercrombie found it; and I believe that no better account of it is to be found than that which exists in the well-known treatise, published in 1778, by the celebrated surgeon of St Bartholomew's, from whom one form of the disorder has received the name of Pott's Disease of the Spine.

Dr Abercrombie having become a Fellow of the Royal College of Surgeons, took a house in Nicolson Street, and established himself as a general practitioner. From the very commencement of his career, he manifested that devotedness to the practical duties of his profession and alertness for business, which characterized him during his whole life. His methodical arrangement of his time, and his punctuality in keeping engagements, were equally the characteristic of his earlier and later years; and these qualities, in combination with the practical sagacity which he manifested, led him soon to receive a considerable amount of public favour. His share of remunerative practice, however, was not enough to engage his whole time, and to a mind so bent as his was on acquiring practical knowledge, attendance on the sick poor offered an attractive resource for occupying his spare hours. Accordingly, in 1805, when it was thought desirable by the Managers of the Royal Public Dispensary to appoint a staff of surgical officers to that institution, we find the name of Dr Abercrombie given in along with others as being willing to give gratuitous advice to the poor; and he was accordingly elected one of the surgeons of the Dispensary on 31st May 1805. This was his only appointment to a public medical institution. He continued to hold this office for at least eleven years; for in February 1816, when the appearance of a still existing rival institution excited much alarm, and no small ill feeling, among some of the officials of the Royal Public Dispensary, and when it was determined to establish a New Town Branch as a check to the young rival, Dr Abercrombie was one of the two senior surgeons who were selected to do duty in the new locality.

But, during this time, Dr Abercrombie's professional services to the poor of Edinburgh were not limited to his official duties at the Dispensary. His character as a practitioner, and his status as a Fellow of the Royal College of Surgeons, had begun to attract towards him a large body of apprentices. According to the practice of that day, this was to the Fellows of the College of Surgeons a matter of some importance. The more advanced apprentices rendered them assistance in the discharge of their professional duties; the junior dispensed the medicines, which, by a custom then universal, now most happily extinct, each general practitioner furnished from his own surgery; and lastly, the apprentice fees were a source of no small direct emolument. In one year Dr Abercrombie entered not less than five new apprentices, which at the most moderate calculation must have yielded him £300. But Abercrombie was not contented to look upon his apprentices as a mere source of advantage to himself. His endeavour was conscientiously to fulfil for them the formal engagement of their indentures—to instruct them in the art and mystery of their profession. To this end he made his gratuitous attendance on the poor importantly subservient. He divided the town into five districts, assigned to each of these a certain number of his more advanced pupils, and thus not only brought medical aid home to many a poor patient, and at the same time made his pupils practically acquainted with the treatment of disease, but he thereby gained for himself a large increase of experience, and amassed much of that practical knowledge, which he afterwards so successfully communicated to the profession, in his consultations and writings.

In addition to this, besides the examination of his apprentices at stated periods, he permitted them to hold a sort of junior medico-chirurgical society in his own house, where subjects of professional interest were commented on and discussed. To his apprentices, Abercrombie seems really to have placed himself *in loco parentis.* I believe that I do not overstep the limits of propriety when I say, that few masters did for their pupils as much, none could well do more, than he, and that few could boast of having been the educator of a larger body of men who afterwards distinguished themselves as medical practitioners.

During eleven years, from September 1810 to September 1821, Dr Abercrombie acted as one of the examining board of the Royal College of Surgeons. Of those who were associated with him at his first election, one only (Mr Wood) remains, but several survive who acted along with him in the latter periods of this term ; and from them I gather that his manner towards the candidates was courteous and considerate ; his examinations were practical and searching, sometimes, it is said, rather minute, but always reasonable and fair.

In 1821, an event occurred which marks an important era in the life of Dr Abercrombie. The death of Dr Gregory left vacant the chair of Practice of Physic in the University of Edinburgh, which, with brilliant success, he had filled for thirty years. It was not likely that there would be any lack of aspirants to the honour of being Gregory's successor, and, accordingly, a number of distinguished men presented themselves to the patrons as candidates for the chair. The magistrates and council had for their choice, among others, the versatile genius and burning enthusiasm of John Thomson ; the practical sagacity and philosophic habits of observation of John Abercrombie ; the extensive erudition and indefatigable industry of Dr Duncan jun. ; and the prestige of his uncle's great name, backing the talents and acquirements of Dr Alison, who then was steadily entering on that career of public usefulness, which has long since gained for him an honour and esteem, independent of all that he derives from his literary and scientific ancestry. But on none of these did the election fall. The municipal rulers had otherwise determined. *Dis aliter visum.* It accords not with our purpose to trace the history of this election further than as it bore upon the prospects and career of Abercrombie. If to him it did not bring the professorial status and the endowments of a productive chair, it still was the tide which taken at the flood leads on to fortune. He had made accurate soundings of public opinion, and felt the direction in which it bore him; and the course which he determined to pursue was bold but not imprudent. He was then in extensive employment as a general family practitioner, but he resolved to abandon this, and restrict himself to consulting practice. With this view he joined the Royal College of Physicians, taking his seat there on 4th August 1823.

From that time to the close of his life, Abercrombie's career was one of steady professional prosperity. His bodily frame robust, his mind ever vigorous and clear, his habits methodical, and his punctuality unimpeachable, his time at consultation never lost upon what was merely speculative, but economically expended upon what was clinically important, enabled him easily to satisfy the large demands made upon him for his opinion and advice, by a public who confided in, and a profession who looked up to him. He shared, of course, largely in the substantial rewards of such labours, but I am led to believe that public rumour, ever prone to exaggeration in such instances, has estimated his professional income above the reality.

Nor were honours denied to him. In July 1828, on the death of the venerable founder of our society, Dr Duncan sen., Abercrombie was appointed first physician in Scotland to the King. The Royal Society of Edinburgh, of which he had become a fellow in 1831, elected him one of its vice-presidents in 1835. In the same year, also, the University of Oxford conferred upon him, in company with Dr Prichard of Bristol, the degree of M.D., by diploma—an honour which had not been bestowed upon any one, with the single exception of Dr Jenner, for nearly fifty years previously. This was not a mere honorary degree, conferring only the privilege of appending M.D. Oxon. to his name; being a degree by diploma, it gave him "the immediate right to a vote in the University, with all the other rights which the fullest performance of academic exercises and residence could bestow."

In 1835, also, among many other honours, he was elected Lord Rector of Marischal College and University, Aberdeen;

and at his installation on 5th November of that year, he delivered an inaugural address, which is eminently characteristic of the practical tendency of his mind. After a few prefatory sentences, in which little is said of Marischal College, and still less of himself, seeing before him only an assemblage of young men, entering upon a fresh season of academic instruction, he seizes the opportunity to give them some suggestions as to the higher objects to be kept in view in the acquisition of knowledge, and the method of successfully attaining them. The rectorial address, as originally delivered, was printed by desire of the Senatus Academicus, and was so much prized for its solid utility, that the author was led to revise and enlarge the practical part, and to republish it a year afterwards, under the title of " Culture and Discipline of the Mind, addressed to the Young." It is a modest little duodecimo, which ought to receive the attentive and repeated study of every academic youth, and one not unworthy of occasional perusal by those of maturer years; for it treats in succinct but clear, in earnest but agreeable language, of the power which man possesses of voluntarily controlling, directing, and regulating his habits of attention and trains of thought; it points out, that sound intellectual culture does not consist in the mere acquirement of knowledge, however extensive that knowledge may be, for this may be an exercise of the memory alone; that there is a culture of the higher powers of the mind, the due regulation of the various mental faculties themselves, of greater difficulty but of higher importance, without which knowledge is vain; it indicates the value of a regulated condition of the mind as bearing upon the acquisition of knowledge, the formation of opinions, and the culture of those moral emotions of the heart which are the highest object to every responsible being; and it points out the means of obtaining this in a series of suggestions so plain and simple, that those of much tenderer years than the audience to which they were originally addressed, may easily comprehend and profit by them.

I have been led to digress a little from the more immediate purpose of briefly tracing the life of Dr Abercrombie, by the direct connection of this little book with one of the honours so justly paid to him. Among these honours we do not find included one which, in the case of a person of Abercrombie's professional eminence, might almost with certainty have been looked for. He never filled the chair of either of the Royal Colleges of which he was so distinguished a Fellow. Assuredly this arose from no disinclination on the part of the Fellows of either College to confer their presidential honours upon him. I believe that I am correct in stating that, among the Fellows of the Royal College of Physicians, movements were made on more than one occasion towards having him proposed for the presidentship, but that he declined being put in nomination, pleading, latterly, an illness which he had in 1841 as a reason for his not accepting the office. As regards the Royal College of Surgeons, his never having occupied the chair admits of easy explanation. At the time that he became a Fellow of the sister College in 1823, there were yet men senior to him, and worthy of the honour, who had not passed the chair; but even if it had, to use a common phrase, come to his turn, there can be little doubt, I think, that he would have declined the honour. For it is to be borne in mind that at this time, and for many subsequent years, the President of the College of Surgeons was, strictly speaking, not the head of a professional body, but the Deacon of one of the Incorporated Trades. We can readily fancy Abercrombie the President of an Academy of Surgery, but to conceive him being a member of the Town-Council, mingling in debates about burgh reform, city elections, and municipal patronage, perhaps with the prospect of being Deacon-Convener and Custodier of the Blue Blanket, transcends our power of imagination. Whatever we may think of the results of the changes which have since that time been made in our municipal constitution, we, as professional men, may at least be thankful for this, that the Surgical College can now choose its President for his professional and personal qualities, without having to consider whether he would prove a fit guardian of our civic interests, or whether he would vote for the Tory or the Whig candidate at the next election for the city.

In truth, there was one part of Abercrombie's mental constitution which rendered all such offices distasteful to him, and exerted a powerful influence on many other actions of his life. Caution, which has become proverbial as a characteristic of his countrymen, was in him not merely largely developed, but excessive. That he declined anything which might lead him into debate or strife, from a sincere desire to fulfil the injunction, "if it be possible, as much as lieth in you, live peaceably with all men," I do most firmly believe. But in running away from polemics, he not unfrequently fell into an error at the other extreme. Eminence in social position is a kind of property which has its duties as well as its rights, and Abercrombie did not always bear this sufficiently in mind. On more than one subject of interest to the public or the profession, where his citizens had a right to expect from him a decided opinion, his trumpet gave either an uncertain or tardy sound, or was altogether silent. Had he more justly appreciated what he knew to be his own social position, (for though utterly devoid of arrogance, he was not unconscious of the eminent place which he held), he would not have been so timid as to encountering difference of opinion in others. His sensitiveness on this point sometimes tended to engender the very difficulty which he sought to shun. We are not, I conceive, though we may be his admirers, necessitated to become his blind panegyrists, and in a man where there was so much to reverence and honour, we need not shrink from noticing what we conceive to have been the little weak point in his character.

The rest of Abercrombie's life may be told in a few words. He continued to exercise the duties of his profession with his usual success and vigour, interrupted only by an illness in 1841, which partook of the character of cerebral congestion, threatening paralysis, and for which he had himself very largely depleted. It is the recorded opinion of his professional friends that he had treated himself too energetically, illustrating the truth of what has become proverbial, that no man is a competent judge in his own case ; but from this he recovered completely, and was soon enabled to resume his manifold labours.

In the midst of all his usefulness, in the full vigour of his powers both of mind and body, the inscrutable decree of God suddenly summoned him from among us. His carriage was waiting for him at his own door on the morning of 14th November 1844 ; he was unusually long of coming out of his private room ; it was entered, and Abercrombie was found extended on the floor all but lifeless. His death, as every one who hears me is aware, was caused by the laceration of some fibres of the surface of the heart, involving the rupture of one of the coronary veins, and causing fatal hæmorrhage into the pericardium.

It is now nine years since Abercrombie died, yet it seems but yesterday that his business-like carriage and well-known countenance were to be seen in every corner of our city. To this meeting I would not attempt, even if I had the descriptive power, to recal his personal appearance. His features are well preserved for the contemplation of future generations in the excellent bust by Mr Steele, placed by the College of Surgeons in their Museum, to commemorate their esteemed fellow-member. The ample intellectual forehead, the grave but not stern lines of his countenance, are there truthfully rendered. But there is a wanting that which monumental bust can never give. We miss the brilliant and expressive eye, which in him, as in many other men of intellectual stamp, gives to the countenance that force and character, which to the painter more than compensates for the lack of classical form and regularity of feature. It is excellently rendered, however, in a portrait in the possession of his family, painted by Sir John Watson Gordon, when Abercrombie was in full health and vigour, of which I shall only say, that a recent inspection makes me the more regret, that it has never yet been put into the hands of the engraver. I am sure that a large number of my professional brethren would with me rejoice, if they could learn that an artist worthy of the duty were permitted to employ his burin on so excellent a subject.

Dr Abercrombie was interred in St Cuthbert's Churchyard, where a monument bearing a profile portrait in relief, also an excellent likeness, by Mr Ritchie, marks the resting-place of his mortal remains. The day of his funeral was one of solemn but emphatic testimony to the worth of the departed, and not easily forgotten by those who took part in the mournful ceremony. The self-invited presence of the two Colleges of which he was a Fellow, of the body of clergy of the Free Church to whose communion he belonged, and of a crowd of his fellow-citizens bound to him by none of these ties, united to testify to the general feeling that a Master in Israel had fallen.

We have traced the leading events of his life, let us now shortly consider his character as a practitioner, an author, and a man.

Abercrombie as a practitioner.—We have seen in our survey of Dr Abercrombie's life, that he was favoured with an immense practice; that he thus had extensive opportunities of becoming acquainted with disease in all its forms, and shared largely in the endowments of his profession. But it is not merely on these grounds that we claim for him the right to be recognised as an eminent physician. The history of what is daily seen in the world too plainly tells us that a medical man's right to professional eminence and esteem is not to be founded upon the number of his patients, or the amount of his contributions to the income-tax. Sometimes a courtly manner and polished exterior, sometimes a pretensious display of learning—tinsel, not gold, oftener yet a still baser metal—the quack's brazen assumption of a skill denied to others, or of a system superior to all existing methods, will lead a fallible public to crowd to the consulting-room, and fill the coffers. But the profession itself is a better judge as to the qualities which really entitle any man to be held eminent. It shuts its eyes to the glitter on the surface, and looks deeper for the sterling ore. The army looks beneath the show of the uniform for the qualities which make the general, before it willingly follows him to the field. The profession looks beyond the exterior display, before it places any man in the position of its acknowledged leader. Skilful he must be, that we may avail ourselves of his aid in our difficulties; resources he must have, that we may draw upon him when our stores are exhausted; but with skill in medicine as an art, he must combine knowledge of medicine as a science; and to these requisites he must add integrity of character, honesty of heart and life, and courteous demeanour towards us. For we do not forget that to our consulting brother we commit the interests not only of our patients but of ourselves. When we carry our difficulties to him as a court of appeal, we must have perfect confidence that he will do justice to the practitioner as well as to the patient. We may have failed of success from error in judgment, it is for his superior experience to instruct and guide, and, as far as in him lies, to counteract the result of our mistakes. We may fail of success in spite of our best endeavours, because our difficulties are insurmountable; it is the duty and privilege of his position to shield us from unmerited imputation of ignorance or unskilfulness. We may be following the path which will lead to a successful issue in good and reasonable time, when the impatience of the sufferer or the anxiety of friends may demand further counsel; and here we have a right to receive from him a due acknowledgment of our capabilities, and full credit for what we have done and are doing. We appeal to his matured experience when our own discernment is at fault, we pay deference to his decision when it is adverse to our own opinion, but we do not forget that we belong to the profession, and have a right to the treatment of gentlemen, and we brook no haughty bearing or discourteous demeanour towards us; and, therefore, it is that in him with whom we would consult, we require skill, resource, and learning, combined with integrity, honesty, and courtesy. In Abercrombie these qualities existed in happy combination, and for him they secured a high place in our confidence and esteem while he lived, and they preserve for his memory our sincere reverence, now that he is departed. I would, however, be much misunderstood if it were inferred from what I have said, that I regard Abercrombie as an unwonted or exceptional instance. I make for him no such demand. He himself would have been the last man to have wished such a tribute to be paid to him. He possessed these qualities in an eminent degree, but he had no monopoly of them. Thankful am I to think that we can always find among our ranks men possessing the full title to be promoted to our leaderships, and proud we may honestly be, that, to the credit of our profession, which is and ever ought to be dear to us, we place at our head only those on whose medical and moral worth we can alike rely.

In manner, especially towards strangers, and consequently towards a large proportion of those who consulted him, Abercrombie appeared reserved; many persons called him cold; he certainly did not make his way to public favour by dint of blandishments. But though cold in manner, he was never unkind in action. It was not in his nature to make a display of sympathy with the sick; it was as far from his nature to feel any indifference towards them. Setting a high value on his time, on which there were many demands from others, and for which he had constant occupation of his own, he was brief and business-like in his visits. He listened attentively to all that he felt necessary to guide him in his duties, but he could feel impatience, and manifest it too, when his precious moments were being wasted by the frivolous or garrulous. But with all this he avoided being harsh; and we are unable to relate of him, for the edification of gossips, any anecdotes of what are styled smart sayings, but which, when called by their right names, are often little else than instances of rudeness.

Of Abercrombie as a practitioner, one of the most prominent characteristics was, his quick appreciation of what were the essential points in the case before him. He laid his hand on these with readiness and firmness, and he never let them go. He formed his opinion of what was needed, and acted upon it promptly and decidedly. His remedies were plain and purpose-like. He had his favourite formulæ, like every other physician, but he was no mere routine practitioner. He had no aversion to trying a recently introduced medicine or preparation, but he never was found rushing into a transient enthusiasm for some new-fangled remedy. He was no friend of polypharmacy in his prescriptions. He eschewed the heroic when it was uncalled for, but he charged boldly and vigorously when the enemy was strong or had attacked an important position. His whole clinical proceedings were essentially practical. This indeed was eminently his characteristic in everything. To borrow the words of another, which I adopt in preference to my own, "His mind was as completely practical as it is possible to imagine. In all his researches and inquiries, while one object was truth and accuracy in results, another was the practical application. With him the object was as speedily as possible to determine what was to be done, what could be done, and how it was to be accomplished."

Abercrombie as an Author.—In what has already been said of the leading events of Dr Abercrombie's life, no mention has been made of his professional and other writings. I have preferred giving to them a separate consideration, that I may thus be enabled briefly to place before you a catalogue of his contributions to medical science and philosophy, in a connected chronological series. Believing that you who knew him as a practitioner will fully recognise his right to be held as eminent, independently of his published works, I have not hitherto cited them in support of his title, nor in illustration of his professional character. But it is by his writings he is known and esteemed elsewhere, and everything that he wrote is eminently characteristic of him. His works evince unwearied diligence, for he wrote much and well in the midst of incessant occupation in practice; they shew a truly inductive turn of mind, for he carefully collected facts, ascertained them to be such, and argued from them with deliberation and shrewdness; they bespeak orderly and methodical habits, for his note and case-books, the originals, we may say, of his treatises, shew that he spared no pains to make his records correct; they evince a firm but not unwarrantable confidence in his own careful observations and large experience, but never to the exclusion of the authority of others, for no man more copiously quotes, or more amply does justice to, those who preceded him in any branch of inquiry. In everything they eminently display his leading characteristic—the practical. Whatever is useful is narrated, whatever is unnecessary is quietly set aside; what is speculative is steered clear of; what is held to be established is clearly asserted; what is held to be erroneous is temperately controverted; what is uninvestigated is indicated as worthy of research. To everything some practical purpose is attached, and how this purpose is to be served is never omitted. It was observed of him by M. Serres, that in a remarkable manner he seemed to tread in the foosteps of Morgagni. It was a correct diagnosis of the French pathologist, and Abercrombie has himself left a post mortem evidence of its truth; for among his manuscripts there exist two little volumes, without date, but evidently written at an early period of his life, which consist entirely of extracts from the works of the great Professor of Padua, whose succinct and practical reports Abercrombie seems thus early to have adopted as the model of his own. Indeed, whether Abercrombie's subsequent contributions to medical science had been intrinsically valuable or not, he would still have been entitled to the high merit, of having been among the first to inculcate and exemplify, that accurate necroscopic examination was the only true foundation of a rational pathology. Previous to his time, morbid anatomy could hardly be said to have had any real existence in Scotland as a branch of medical science.[1]

Dr Abercrombie was in no haste to appear as an author; he had studied, observed, and reflected much before he ventured into print. He did not publish anything till he had been twelve years established in practice.

Let me shortly call to your recollection his contributions to medical science.

1. In 1816, in the 12th volume of the Edinburgh Medical and Surgical Journal, he narrated a case of acute Cynanche Laryngea successfully treated by large bleedings;[2] and in the same volume, under the head of Medical Intelligence, he

---

[1] His zeal for pathological dissections was so conspicuous as to be a subject of general remark. It did not escape the quick eye of Dr Gregory.—"Do you know," said Gregory on one occasion, "who is to be the future consulting physician of Edinburgh? I'll tell you. It's that little dark-eyed fellow who is always looking so closely at my post mortems."

[2] It is impossible to peruse the practical writings of the physicians of the end of the last, and early part of this century, without being struck with the confidence which they placed in sheer blood-letting, and the extent to which they used the lancet, compared with what obtains in the present day. The laity, to whom the fact is notorious, often ascribe this to the caprice of doctors, and insist upon it as a proof that there is nearly as much of fashion in medicine as in millinery. It is easy to see many circumstances which account for this in a more creditable manner. Our improved methods of diagnosis, especially in thoracic diseases, enable us now to detect and check maladies in their earlier stages, when they are vincible by milder measures, such as local bleeding and sedative remedies, or to discriminate affections which do not require energetic treatment from those which cannot be too actively combatted, because they involve important textures, and tend to produce serious after consequences. The stethoscope has saved many a patient from being bled for pleurisy, and has enabled his physician to relieve him by a dose of Dover's powder and a sinapism, because it has shewn that his acute sidestitch involved no more important texture than the intercostal muscles. What we have called our milder measures, our local bleeding and sedatives, are more numerous, more potent, and more available than they were forty years ago. How often do we substitute, especially in large towns where the services of a dexterous operator can be had, the detraction of a few ounces of blood locally for a bleeding from the arm; and how often do our more recent sedatives, such as hydrocyanic acid, digitalis, aconite, and chloroform, enable us to subdue cough, hæmorrhage, pain, or spasm, while our forefathers would have deemed the sedative effect of bleeding essential to the relief of the patient. All this, however, only partially accounts for the marked diminution which has taken place in the use of the lancet. We cannot maintain that our predecessors were wrong in using it so freely, for it was abundantly successful in their hands. We are not going to stultify ourselves by admitting that we are, from mere caprice

communicates, in a few lines, the history of a case where, by the tobacco enema, he relieved a Retention of Urine, from a calculus impacted in the urethra.

2. In 1818, in the January number of the 14th volume of the Journal, appeared his Observations on the Diseases of the Spinal Marrow, the first of those valuable pathological writings on which his reputation as an author mainly rests. He contributes, from his own experience, only one interesting case of acute inflammation and suppuration of the spinal cord. The rest is the result of careful and extensive reading; and the whole paper is valuable, as being one of the first attempts to systematise spinal diseases.

3. In the April number of the same volume, he published a series of cases of Acute Anasarca, shewing that it is often connected with an inflammatory state of the lungs, and insisting on the value of full bleeding in its treatment. Attention is here directed to the subject of the coagulability of the urine, which had been pointed out by Blackall in 1813, which it was reserved for Dr Bright to connect with certain organic changes in the kidney, and the value of which, as a pathognomonic sign, has yet, I believe, to receive its rigid determination. Dr Abercrombie attempted, in some of his cases, approximately to determine the quantity of albumen present, and pointed out its diminution as the cases of anasarca improved.

4. In the July number of this same volume of the Edinburgh Medical and Surgical Journal, he published the first part of

---

or fashion, neglecting what was their most popular remedy. We can properly reconcile the difference between their mode of treatment and ours, only upon the supposition that diseases anatomically similar, do not in our day present the same constitutional phenomena which used to mark them, and that our practice is different chiefly because the symptoms which characterise disease are different also. That this is true, daily observation seems to prove; for even within the experience of those of us whose duration of professional service has been comparatively limited, there is a marked increase in the number of cases of inflammatory disease, where there is little of sthenic in the constitutional symptoms, or rather where, on the contrary, there is a tendency to sinking. It appears to be the fact, that in many instances we are called upon to administer wine and stimulants, where, in the days of the preceding generation, there would have been a call for the lancet. We can observe a variation in the type of disease, as we observe cyclical variations in the seasons of the year. We remark the absence of old-fashioned pneumonias as we do that of old-fashioned hard winters. It is no unreasonable supposition, that many of the present generation may live to see a return to the old types both of disease and weather; and that they may see the lancet again as much and as successfully employed as it was in the days of Cullen, Gregory, and Abercrombie. Instead, therefore, of admitting that these variations in practice are proofs of caprice or fashion of which we ought to be ashamed, we shall more properly interpret them as evincing a rationality in our therapeutics of which we ought to be proud, and as evidence that the legitimate practice of medicine is not founded upon empirical principle, or no principle at all, but that it rests upon a strictly scientific foundation of observation and induction.

his well-known researches on the Pathology of the Brain,—his longest and ablest paper hitherto. This first part is restricted to chronic inflammation of the brain and its membranes, and contains the history and post mortem appearances of twenty fatal cases, and the account of eight cases of recovery from similar symptoms, with copious pathological and practical remarks upon them.

5. In the November number of the same year, his researches on Cerebral Pathology were continued. This second part embraces the subject of Apoplexy. It was here that he first accurately described that interesting class of cases, which may prove fatal without leaving in the brain any morbid appearances to account for death, and which have with all subsequent writers retained the name of Simple Apoplexy, by which he distinguished them. Perhaps the most valuable part of this celebrated memoir, however, is his distinction of apoplectic cases, as regards the course of symptoms and the prognosis to be founded on it, into the three sets of—those which are immediately and primarily apoplectic — those which begin with sudden and violent headache, and pass into coma gradually—and those which are distinguished by palsy and loss of speech without coma. We may perhaps not unreasonably take exception to the terms (the apoplectic and the comatose) by which he designates the two first classes of cases respectively, for both are apoplexy and both are attended by coma; but we cannot hesitate to acknowledge that the distinction which he here drew, and which he has lucidly explained and copiously illustrated by examples, is a most important contribution to practical medicine, especially in the matter of prognosis. To Dr Abercrombie, more than to any other author, we owe the observation, that the primarily apoplectic cases, though always uncertain in their issue, still leave reasonable ground for hope; whilst the others, though apparently less formidable at the beginning, are always more unpromising, and generally indicative of extensive and irremediable cerebral hæmorrhage.

6. The memoir on Apoplexy was resumed and completed in the January Number of 1819 (Ed. Med. and Surg. Jour., xv.), by a chapter treating of paralysis, and giving a short outline of the general treatment of apoplexy. In one section of this chapter, he calls attention to the fact of paralysis in many instances depending upon an inflammatory, not an apoplectic, state of the brain. Here he had evidently noticed, though he failed fully to recognise, that well known pathological condition, which the labours of Rostan and Lallemand, within a year from this date, and of many other writers

since their time, have made familiar to us as ramollissement of the brain. Dr Abercrombie had, in fact, described cases of this kind in his former memoir on chronic inflammation of the brain, but he had confounded the state of softening with suppuration. In another section he points out that "there is a singular modification of paralysis which seems to be connected with the state of the circulation in the part;" and he most faithfully describes one very characteristic example from his own practice of that interesting class of cases, which, within the last few months, has been the subject of much discussion among us in reference to their probable connection with cardiac vegetations. Even in these two instances where we find him failing to give to the pathological phenomena their true significance, we see how carefully he noted, and how systematically he arranged, what had come under his own notice. He was himself perfectly aware that his observations on what he calls inflammatory paralysis were incomplete, and he specially proposed it as an interesting subject for investigation. It is rarely the good fortune of any man to be able to exhaust a pathological inquiry, especially one so extensive as apoplexy and paralysis; and next to his who contributes something new to our stores of knowledge, is the merit of him who indicates something definite for the labours of others to supply.

7. In the No. for April 1819 (Ed. Med. and Surg. Jour., xv.), we have a short but instructive paper from his pen, under the somewhat vague title of "Cases of Dyspnœa in Children, connected with a singular affection of the Throat." It is in fact the description of three cases of post-pharyngeal suppuration, producing symptoms resembling those of croup, and effectually relieved by opening the abscess.[1] It is, I presume, the vagueness of the title of this paper which has led to its having been singularly overlooked by subsequent writers. In the 17th volume of the Dublin Journal, 1840, Dr Fleming of that city details two interesting cases of this kind, and quotes all the authorities which he had been able to collect on the subject. In the Gazette Medico-Chirurgicale, for May 1846, there is a paper on the same topic by a Belgian author, M. Bessems, which is translated in the August No. of the Edinburgh Monthly Journal for that same year. In the Monthly Journal for October 1847, Dr Peacock narrates and delineates a fatal case of the same

---

[1] I may remark in passing, that this is the only instance in which I find him mentioning that he himself performed a surgical operation. He frequently, however, had occasion to operate, in the earlier part of his career as a general practitioner.

nature, and these two last papers are cited by Professor Miller in his Practice of Surgery, in illustration of the fact, that post-pharyngeal abscess occasionally produces symptoms which may be mistaken for those of croup; yet, by not one of all these writers is the name of Abercrombie mentioned in connection with this subject. I have to confess that when I went to peruse this paper I expected something totally different. I thought to find there Abercrombie's account of Laryngismus stridulus. Perhaps there may be others who have equally failed to give to this paper its proper share of notice, and who will not regret my having directed their attention to it, both as one which has not been duly referred to, and as one which well illustrates the practical sagacity of its author.

8. In the No. for October 1819 (Ed. Med. and Surg. Jour., xv.), the series of papers on cerebral pathology was concluded by a chapter on organic diseases of the brain. This portion of his contributions to cerebral pathology does not present, nor in truth does our knowledge of the organic lesions of the brain even at the present day, admit of, those important generalizations which are met with in the previous chapters. Abercrombie treats the subject in the only way in which it could be practically useful—in reference to diagnosis; and he arranges the cases, perhaps with more than necessary subdivisions, according to the kind of symptoms which characterized them. The chief value of this chapter consists in its utility for future reference, containing, as it does, records, either quoted or original, of not less than forty cases of cerebral organic disease.

In this same No. of the Edinburgh Medical and Surgical Journal, October 1819, appeared a criticism on the celebrated lectures by Mr Lawrence, on Physiology, Zoology, and the Natural History of Man, denouncing certain opinions which, to the reviewer, appeared to be unphilosophical in their nature, and dangerous in their tendency. It is no secret that this review proceeded from the pen of Dr Abercrombie. Deeply impressed with a conviction that these doctrines, backed by the authority of so fine an intellect as that of the author, were hostile to the interests of religion and sound philosophy, he seems to have thrown himself into his critical work with unusual ardour and spirit. The general style of the review is very different from that of most of his writings. There is a playfulness and humour in some parts of it, which, making all allowance for the freedom conferred by the reviewer's "we," would hardly have been expected from the ordinary turn of mind of Abercrombie. But his own more special characteristics are there in full development—earnestness of purpose, steady defence of what he held to be true, unsparing denunciation of what he felt to be false, and all tempered by that charity which disposed him, as far as possible, to think no evil. Let the peroration of the review substantiate these remarks. Referring to a quotation from Dugald Stewart, he says—"These considerations from this high authority we respectfully submit to the attention of Mr Lawrence. We believe him to be a man of talents and acquirements; but in the work now before us he has wandered into speculations for which his intellectual habits are entirely unqualified, and has 'reasoned downwards.' We fondly hope that he was not himself aware of the abyss into which his speculations were leading him. We think too highly of his understanding to believe that he is really convinced by his own reasoning; we hope too well of his heart to imagine that he seriously intended to lead his pupils into a system, dark as the valley of the shadow of death, and pestilential as the vapours of Acheron."

9. Having accomplished a systematic series of papers on the diseases of the encephalon, he now set himself to the execution of a similar task in reference to the abdominal organs. Accordingly, in the January No. of the Edinburgh Medical and Surgical Journal, for 1820, vol. xvi., he published the first of his well known essays on the pathology of the Intestinal Canal; the subject of this first section being the diseases connected with derangement of the peristaltic motions. In this valuable collection of cases and practical remarks, he gave special consideration to the subject of Ileus; drew attention to the important fact that it may prove fatal without the supervention of inflammation, great distention being all that can be found after death; he enforced the doctrine that the obstruction in such cases was owing to a paralysed state of the distended portion of intestine, not to spasm of the part below; and he applied his pathology practically, by pointing out the cautions to be observed in the use of strong cathartics, and the probability that bleeding, large opiates, and tobacco—the last of which was his most approved remedy—produced their good effects not by relaxing spasm below, but by moderating the downward forcing action of the healthy intestine above. There are few of Abercrombie's papers more succinct, clear, and practical than this.

In the immediately succeeding numbers of the Journal for April and July, 1820, he continued his papers on Abdominal Pathology, discussing the subject of the Inflammatory Affections of the various textures of the alimentary tube. The records of cases are, as usual, copious and instructive, but there is little so novel or original in these two papers as to call for recapitulation here.

10. In the January number of the Journal for 1821, his busy pen again brings him before the profession as an author. He has done much for the head and abdomen; the chest now claims his attention. In this number he published a paper of considerable length on the Pathology of Consumptive Diseases, which was continued and concluded in the January number for 1822, vol. xvii. To us, at the present day, this memoir has only the comparatively limited value of historical interest. It produces a somewhat curious impression on our minds to read a paper, by a physician of our own times, on Phthisis and its diagnosis from other pulmonary affections, without finding in the whole course of it the words stethoscope or auscultation. Let us bear in mind, however, that as regards auscultation by percussion, though introduced to the world by Auenbrugger in 1761, and for a second time forced upon the attention of the profession in France, by Corvisart, in 1808, it attracted comparatively little notice, till the analogous brilliant discovery of stethoscopic auscultation by Laennec in 1816 demonstrated its importance; moreover, that Laennec's treatise De l'Auscultation Mediate was not published till 1819, and Dr Forbes' translation of it not till 1821, after this memoir by Abercrombie was written; and thus, that at the date of this paper the new resource of the physician, now his main guide in the diagnosis of thoracic disease, could not be familiar to Abercrombie. It seems to me more than doubtful if he had at that time heard of it; for had that been the case, I can hardly think that in this paper he would have totally omitted to notice it. In regard to the purely pathological part, we can easily see now, that Abercrombie's ideas on phthisis were very crude, compared with those which now obtain among us; but let us also remember that the foundation of our knowledge of tubercle essentially rests on the invaluable "Recherches" of Louis, which were not published till 1825.

In reference to the great practical question of the curability of phthisis, and the means of obtaining a cure, he, in common with most physicians of his time, expressed a very cautious opinion. He seems to admit the curability of phthisis rather as a supposition than a reality, although he quotes one case from his own observation, which undoubtedly was an example of arrested tubercular disease of the lower part of one lung. It is to be borne in mind, however, in reference to this, that he hardly calls a case one of consumption unless it had passed into the state of "ulceration of the lungs."

In reference to the then and since much disputed question of the tonic or the antiphlogistic treatment, he is equally reserved. "Consumption, says one, is a disease of debility; says another, it is a disease of excitement. The sound pathologist knows that it is a morbid degeneration of a vital organ; and that except he can combat it there, in its proper seat, the excitement and the debility are equally beyond his control. In this morbid condition there are many varieties, and many of them are obviously beyond the reach of any human aid. There are others in which the tonic treatment is proper and expedient, and in which, supporting the strength of the patient by every possible means, is giving him the only little chance that remains to him of throwing off the disease. But in others certainly the antiphlogistic treatment is required; and by the judicious employment of it at the proper period, some considerable prospect is held out to us of arresting the disease at its first appearance. If these be trifled with, or injudiciously treated at the commencement, the period for active practice is soon gone by, and the error is fatal." Our modern views as to the constitutional origin of tuberculosis will hardly allow us to assent to the doctrine that phthisis is to be combated "there, in its own proper seat."

11. In the No. of the Journal for April 1821, we have from him a short paper on Ischuria Renalis, in which he narrates four fatal and one successful case from his own observation. The well-known circumstance of the fatal termination of such cases by coma, of course, comes in for a share of comment. To Abercrombie and the pathologists of his day, this coma appeared to be connected with cerebral effusion, but the connection of the supposed effusion with the renal disease was quite enigmatical. "The most singular circumstance," says he, "in the pathology of the disease, is its tending to terminate by coma and effusion on the brain. The minute connection here will probably ever elude our researches; but, in a practical point of view, it is of great importance, and presents a most interesting subject of investigation."

These two papers by Abercrombie on subjects so remotely diverse as Consumption and Suppression of Urine, occurring, as they happen to do, in chronological juxtaposition, unite in suggesting to us two subjects of remark. First, we may derive from them a reasonable satisfaction as to the recent progress of practical and scientific medicine. When we see the first physician of his day, darkly groping for a diagnosis, between morbid conditions, which the large proportion of practitioners can now distinguish with comparative confidence;

when we find him looking upon the coma of uremia, now familiar to every hospital clerk, as a dark subject, perhaps destined ever to remain a mystery—we have, I think, a sufficient answer to those who would ask us what progress we are making in our art and science. Secondly, let us temper our pardonable self-complacency with a due appreciation and regard for the labours of those who have preceded us. When we hug ourselves on our advanced knowledge, and say that in many things we are wiser than the last generation, let us remember that for much of this we are their debtors. When we travel along our professional journey with comparative ease, smoothness, and rapidity, let us not forget or undervalue those who, with patient observation, first accurately surveyed the line for us—who cleared it of the accumulated rubbish of centuries of error—who reared solid embankments of truth over quagmires of loose hypothesis—who laboriously tunnelled their way, with less perfect tools than we possess, through many a hard stratum of error, and laid down for us the rails by which we now reach our terminus, *tuto cito et jucunde.* Whilst we flatter ourselves that we are daily making extension of our line, and working it better than our forefathers, let us remember that the Morgagnis, Baillies, and Abercrombies, were great engineers in their day.

12. Dr Abercrombie's next contribution to medical literature appeared as the first paper in the first volume of the Transactions of the Edinburgh Medico-Chirurgical Society, which had been instituted in 1821. Dr Abercrombie was one of its vice-presidents from 1822 to 1829, in which year he was elected president, and occupied the chair during four consecutive sessions. This paper, read on 3d December 1821, is on the Pathology of the Heart, and forms a systematically grouped collection of upwards of thirty cases, including interesting examples of pericarditic and endocarditic inflammation, of valvular obstruction and incompetency, of cyanosis, and, in one instance, of the lesion which proved fatal to himself—a paper which, though deficient according to the present standard, in some points of pathology, and in the whole subject of physical diagnosis, is yet a mine of valuable facts in relation to the general signs and complications of cardiac diseases.

13. In the latter part of the same volume of the Transactions, we have a short paper from him in connection with Dr Alison's well-known memoir on the Pathology of Scrofulous Diseases. Here we find Abercrombie detailing chemical experiments which he had made upon tubercular matter. We do not profess to say that we gain much from it, nor can

we boast of being very much further advanced than he was as to the chemistry of tubercle; but it is interesting to observe this man, of truly practical habits, endeavouring to turn to good account the collateral science of animal chemistry, now, in many respects, a valuable help to the pathologist, and yet destined to be of still greater service to him.

The above, so far as I have been able to gather, are Dr Abercrombie's chief contributions to periodical or serial medical literature. We have brought him down, as an author, to the year 1823, by which time his extended reputation and consequent unceasing employment as a consulting physician, left him but little time for writing papers. Though he ceased to contribute to the journals, however, he did not discontinue his labours as a medical author. In 1828 he published, in one volume, the papers from the Edinburgh Medical and Surgical Journal on Diseases of the Brain and Spinal Cord, much improved in many respects, especially as regards due recognition of the phenomena of softening of the brain, which he compared to gangrene in other textures, and holds to arise, in one form, from inflammation, in another, from obstructed circulation in the brain. In the same year, under the title of Diseases of the Abdominal Viscera, he republished his pathological papers on the Intestinal Canal, with additional chapters on the liver and other abdominal organs, which he had contributed to the Edinburgh Medical and Surgical Journal in 1824. In the collecting of his papers on cerebral and abdominal pathology into distinct volumes, he had been anticipated by others; a strong proof of the value with which these papers had been regarded by the profession at the time of their publication. In 1821 the separate papers on diseases of the brain were collected and translated into German by Dr De Blois, under the directions of Professor Nasse of Bonn, who appended to the volume some observations of his own upon tumours of the brain. In 1822 Dr Wolff of Bonn collected into one volume, and translated into German, the papers from the sixteenth volume of the Edinburgh Medical and Surgical Journal on the Diseases of the Intestinal Canal. The books, as completed and published separately by Abercrombie himself, were subsequently translated into French and German. They were thus, both at home and abroad, at once recognised as standard works on Pathology; they found a place in every medical library, and a new edition of each was called for and published two years afterwards.

From this time Abercrombie's contributions to strictly medical literature were few and comparatively unimportant. In 1832 he published a small treatise entitled Suggestions on the Malignant Cholera, which is generally held not to have added to his reputation as a medical author. In 1834 he communicated to the Medico-Chirurgical Society two instances of cerebral disease, which are included in a record of cases published by the Society, in the Edinburgh Medical and Surgical Journal of that date.

In 1843 he read to the Medico-Chirurgical Society, a short paper on stammering, an abstract of which is given in the Monthly Journal of Medical Science for March 1843. The paper itself was never published in extenso, Dr Abercrombie having found, after he had committed his observations to writing, that he had been in great part anticipated by Dr M'Cormack of Belfast, who had written a small volume on this subject in 1828.

According to Dr Abercrombie, stammering does not consist in any defect of the organs of speech properly so called, but is rather connected with a deficiency in the management of the voice, especially with a habit of attempting to speak when the stammerer has no voice, that is, when the chest has been emptied of air or nearly so. The principles on which a cure may be accomplished are thus laid down by him, it being premised that to obtain success everything depends upon the perseverance of the patient himself: "I. To direct the attention of the individual to the three distinct parts of which the function of speech consists: 1, A full and continuous current of *air* proceeding outwards from the lungs; 2, The formation of this into *inarticulate sound or voice* by the action of the larynx; 3, The formation of this into *articulate sound* or *speech* chiefly by certain movements of the lips and tongue. He soon perceives that he has no difficulty in performing any of these actions when they are thus made separate objects of attention, and in this manner he is led to understand, that his affection does not depend upon any defect in any of the organs of speech, or a difficulty of performing any of the processes of which the function consists, but in a certain want of harmony among these processes which has grown into a habit. II. The second and principal part of the treatment is, to exercise the individual in the habit of never attempting to speak without having a full and strong current of voice. . . . . When he has thus been made to understand the principles upon which the removal of the affection is to be conducted, the further treatment consists in a course of exercises calculated to give him a full command of his voice, and so to correct the habit which he has acquired of speaking, or attempting to speak, without sufficient voice."

Some interest attaches to this paper from the circumstances which led to its being written. Dr Abercrombie had seen, in consultation with Dr Simpson, a patient who was a stammerer, and to whom Abercrombie, practical in all matters, had kindly volunteered some instructions as to how she might get rid of her impediment. Dr Abercrombie told Dr Simpson that he had been led to pay particular attention to this subject, from his having himself been affected with stammering in his earlier years. In fact it was this impediment in speech, which had led him to abandon a cherished intention of devoting himself to the ministry, and to adopt medicine as his profession. It was at Dr Simpson's request that he was induced to throw his observations on the subject into the form of a communication to the Medico-Chirurgical Society; and it is evident that the unnamed "individual on whom his first observations were made" was himself. He certainly had been successful in overcoming his impediment, for though not a fluent speaker, he exhibited in his after life no appearance of stammering, either in public or colloquial speaking.

Although Abercrombie, however, subsequently to 1828, may be said almost to have ceased to contribute to medical literature, he was not entirely engrossed with the practice of his profession; his pen was far from being unemployed. He appeared as a philosophical author in 1830, when he gave to the world his work on the Intellectual Powers and the Investigation of Truth. There, if any where, we might have expected that Abercrombie would have indulged a little in speculative discussion; but we find him the same resolutely practical man as ever. In the strictly metaphysical part of the work there is no pretension to originality; it is a simple, but on that account not less valuable, exposition of what is known, as to the manner in which that part of us which thinks and wills, remembers and reasons, carries on its operations. The greater bulk of the work is purely practical, and though not directly addressed to any class of the community, it specially commends itself to the members of his own profession, by the valuable chapters on the application of the rules of philosophic investigation to medical science, under the heads of the acquisition and reception of facts, the arranging combining and separating facts, the tracing the relation of cause and effect, and the deducing of general facts or general principles. To us also it has a professional interest, from its contributing from his own observation, many interesting examples, bearing upon the curious but difficult subject, of the influence exerted on the mind, by disorders of the bodily organ through which it manifests its operations. Perhaps I cannot better characterize this work than by quoting the opinion of an able critic, who certainly had not been over laudatory to Dr Abercrombie's strictly professional writings, but who thus expresses his appreciation of this, Abercrombie's first and greatest contribution to Philosophy: "It is true that there is little originality attempted in these inquiries, and that much of the matter which they contain we have long been made acquainted with by writers of another school; but metaphysics is the last field which any man, ambitious of originality, would enter in search of fruit, and our profession stands much less in need of brilliant discoveries on the essence or exercise of the mental powers, than of a general and practical acquaintance with mind as it is already known. This more useful information is contained in these inquiries; and the strongest argument by which we can urge them upon the attention of our readers will be found in the voluminous review with which we have considered it our duty to introduce them to their notice. To conclude, in the phraseology of their subject, they exhibit much clearness of perception, much accuracy of association, much good taste and strong judgment, and, though last certainly not least, in these days, a moral system so religously disciplined, as can confer upon a head well informed, all the advantages of a heart well regulated. The style is occasionally loose, and the language is not perhaps sufficiently precise for metaphysical disquisition; but, when the pursuits and avocations of the writer are considered, it is less remarkable that such slight deficiencies are found, than that they should so rarely occur."—Medico-Chirurgical Review, April 1831.

In 1833, Abercrombie published his smaller work on the Philosophy of the Moral Feelings. This little volume may be regarded as forming the connecting link between the larger and more metaphysical work just noticed, and the series of tracts on subjects of practical religion, which he also wrote. It points out the distinction, but, at the same time, the harmony and co-operation, which exists between the intellectual powers and the moral feelings. It gives a detailed analysis of man as a moral being, in which the existence and operation of conscience as a distinct and regulating principle of the mind is insisted on; it criticises the different theories of morals which have been propounded by various philosophers, takes exception to them all, as not duly recognising the authority of conscience; maintains the perfect consistency with the strict course of philosophic inquiry, of an appeal to revelation; and concludes with what virtually is a practical application of the subject—a disquisition on the moral relation of man to the Deity.

Of his tracts on religious subjects this is not the place to attempt any analysis. They are five in number, and along with his rectorial address have been published in a single volume. They make no pretension to originality—nor, we may say, are they distinguished by any great variety. Their merit lies in their solid practical character. They are his Essays on the Intellectual Powers and Moral Feelings, viewed in the light, and applied and enforced with the authority of Scripture. The orthodoxy of Abercrombie's faith is clear enough from these writings—but it is not the direct object of these tracts to argue for what is doctrinal. He does not assume the special function of the divine—they are practical religious works, not theological.

Such are Abercrombie's published contributions to medicine, philosophy, and religion. They present a goodly array, although not requiring us to term him a voluminous writer, for his papers, though often long, are not numerous. Their value is to be estimated not by their bulk, but by a consideration of the care in observing, the fidelity in recording, the judgment in systematically arranging, and the tact in practically applying, which they evince throughout. Abercrombie's style, as regards composition, may be said to be characterized by correctness rather than elegance. It commends itself to us by its aspect of practical usefulness; it cannot be said to attract us by its brilliancy. Whilst many philosophical, and not a few scientific and medical writers, remind us of the rapid dashing engine of the rail, Abercrombie rather suggests the same great mechanical power when stationed at the mine; doing the work unremittingly and quietly with a few long and powerful strokes; dipping into dark places where much valuable matter was stored, but where he had to labour hard to reach it; pumping out much that obstructed progress and prevented others from working it successfully, and ever and anon bringing up to the surface a goodly load of solid, substantial, useful material.

We have thus endeavoured to delineate the principal features in Dr Abercrombie's character as a physician and author, but the portrait, ill-drawn I know in many respects, would be utterly an unfinished sketch, and unfit for presentation before you, if it did not also exhibit, however imperfectly, the general complexion of his character as a man.

To do this, it is unnecessary for me to paint in strong colours, or endeavour to put a high glaze upon the picture. The character of such a man is exhibited most truthfully, in the plain unvarnished tale of his daily life and conversation. We have claimed for him no exemption from the failings of our nature. To do so for any man were worse than useless; but remembering that he was a man, we may pronounce him good. He made the profession of religion in his writings, decidedly but most unostentatiously. He made the practice of it in his life. It was interwoven with his nature. It was no sanctimonious garb assumed to carry him with due decorum through his worldly avocations,—it was not a cloak put on to hide, from the remembrance of himself or others, the follies of a misspent youth. The pious instruction of the paternal manse had been to him as the good seed sown in a good soil. It grew with his youth; it ripened with his maturity, and brought forth its fruit an hundred fold. Early in life we find him enrolled among the office-bearers of the church. In the year 1807, when he was only twenty-six years of age, he was ordained an elder of Lady Yester's, a parish with which he was connected, not merely by the accident of his first residence in Edinburgh being within its bounds, but by his having been an inmate of the family, and a warmly attached friend, of its clergyman Mr Black. It was after his death, and during the incumbency of Dr Fleming, that Abercrombie became one of its elders. To him the eldership was not a mere matter of form, or an empty ecclesiastical title. Whilst he held the rank he discharged the duties. He visited in the parish much and usefully. He made himself acquainted with the moral and social condition of its inhabitants, and the observations which he there made, he afterwards embodied in a short pamphlet, on the Moral Condition of the Lower Classes in Edinburgh. In conjunction with another former member of that kirk-session, who yet survives (Mr Andrew Balfour), he collected a mass of parochial statistics, which received from Dr Chalmers a high commendation for its utility; and when his increased professional engagements prevented him from visiting the parishioners, he evinced the continued interest which he took in their spiritual welfare, by his tracts on the harmony of Christian faith and Christian character, and other subjects, which he wrote and printed for gratuitous distribution among them, and which he prefaced by a short and earnest address to the industrious classes.

But it was not merely thus that Abercrombie made himself an instrument for good to others. Whilst by his writings he diffused the highest and most useful of precepts, by his consistent deportment he set before others the no less valuable instruction of his example. The activity of his benevolence, as manifested by acts of charity and liberality, neither few nor small,—the integrity of his life,—the gravity of his demeanour,—the very reserve of his manner, combined to give weight and influence to his name, and left upon the minds of those who came in contact with him, the impression of his being a man richly endowed with the virtues and graces of the Christian character.

To these, doubtless, we might add those parts of his religion, which were manifested only in the domestic circle, and those which were unknown save to Him who seeth in secret. But the pieties of the family altar and of the private closet need not to be dragged into light to establish the character of Abercrombie. Leaving them untouched, as being among the things with which, as a stranger, I cannot intermeddle, I find in the retrospect of what Abercrombie wrote and said and did before his fellowmen, the sufficient proof that he merited what he received at their hands, that good name which is better than precious ointment.

Such was Abercrombie as a physician, an author, and a man. Active and alert in his professional vocation, zealous for the diffusion of useful knowledge by his writings, religious in profession, and pious in life, he fulfilled the apostolic injunction, and was not slothful in business, fervent in spirit, serving the Lord.

28 HERIOT ROW, EDINBURGH,
12*th April* 1854.

ART. II.—*Tropical Fever and Dysentery; being the result of an experiment by order of Government, in the General Hospital, Calcutta, and Garrison Hospital, Fort-William, in the years* 1849–50, *with a Sketch of the Theory resulting.* By E. HARE, Surgeon, 1st Bengal Fusiliers.

(Continued from p. 58.)

II. DYSENTERY.

Before my arrival in Calcutta, I had seen only dysentery in the dry climates up country, the Punjaub, Cabul, and the hills; but in the marshes of Bengal I was warned before hand, by many of my friends, that I should have a very different disease to contend with. I fortunately commenced my experiment in the cold months; and my up-country treatment, by large injections during that season, answered my expectations, and gave me time to study the more dangerous form, which appears in the rains, called hemorrhagic dysentery, and which is ably described by Dr Raleigh, who was a long time attached to the General Hospital, in his book on the treatment of dysentery. Notices of it are also found in the older writers on tropical dysentery, who describe its symptoms as having a putrid tendency, and prescribed bark, as Raleigh says, in the diaries of the General Hospital, supposing it to have antiseptic properties, and with it wine and brandy. The symptoms are, pure venous blood passed in large quantity, pouring out like water by stool, and coldness of the whole body. The post mortem appearances are, intense venous congestion of, and serous effusion between, the coats of the mesentery, sloughing of the mucous membrane of the colon in large, ash-coloured patches, with a general blueness (it is often described as a purple colour) of the rest of its mucous surface.

These evidently recal to our minds the abdominal congestions which are so invariably the accompaniments of malarious fever, and which Dr Twining so often calls our attention to in his published cases, viz., fulness and heat of the abdomen, with cold extremities. The blue colour of the mucous membrane is evidently from intense venous congestion, and the hemorrhage arises from these vessels bursting on the diseased and sloughing surface of the bowel.

Dr Macpherson describes two cases in the General Hospital register :—

E. Rogerson, Sept. 1, 1849, dysentery; died 15th, of the putrid and passive form of the disease.

Dr M. remarks on the post mortem :—" Viscera healthy, except the colon. There was a general oozing of blood from the mucous surface throughout to the anus. A large quantity of blood was found in the rectum; there was general ulceration of the intestine. This (he writes) was a case of simple dysentery, which was yielding to treatment when fever supervened, after which the hemorrhagic tendency developed itself."

John Crosby, died 20th Sept. 1848; hemorrhagic dysentery. He passed pure blood before death. The post mortem describes " The whole of the large and small intestines dark and congested; the veins of the mesentery and mesocolon full of dark blood; the inner coats of the whole of the large and small intestines more or less purple, with much abrasion, scarcely amounting to ulceration. The portions most diseased were the transverse colon and rectum."

I could quote, had I space, a volume of such cases as these, but must content myself with one more, shewing how the less putrid forms of dysentery may also be inseparably mixed up with fever in their symptoms. It is Dr James Johnson's own case, p. 186. I will give it in his own words, abridging principally the dysenteric symptoms; but the case is well worth reading in the original.

The cause—Snipe-shooting in the half-inundated paddy-fields, a few miles below Kedgree, in the Sunderbunds. *The* *symptoms*—Rigors and chills, alternated with flushes of heat. This was the first night. During the next day I was better, and concluded that all was well. In the evening, however, griping commenced, and fever came on. My skin now became hot, dry, and parched, and by eleven o'clock at night I could scarcely leave the commode. I was often delirious. I was now *passing blood fast*, and the fever ran high. The next day passed rather easier than the preceding night, but I had considerable fever. As my night closed in, the exacerbation was great, and I was again delirious. On the third day my fever ran higher, with hot, dry skin; and as night approached my apprehension of the usual exacerbation brought on extreme mental agitation.

This last case was a young, robust constitution, which could react against the congestive effect of the malaria. The first were cases which could not; answering to the algid fever described by Dr Hashel, and which is nothing more than congestion of the whole venous system, without power of reaction. In both it appears that the abdominal congestion relieved itself by bleeding from the bowel. It is true that, as shewn by Dr Mackintosh, bleeding in the cold stage of fever —that is, during the congestion—will often stop the paroxysm. So bleeding from the bowel, or severe purging, will also mask in very many cases the febrile symptoms, by destroying the reaction; yet, as in Dr Johnson's, the cases are so numerous where the purging intermits, with frequent notices in the diaries of this kind—passed a very bad night, constantly at stool; has been better to-day, bowels more quiet, to be followed again by another increase; and the patients constantly remark, that they were better in the morning, but took on a change during the afternoon. Dr Wilson also says of the dysentery in China (p. 53), that it almost invariably alternated with fever.

Dr Holland's first two proofs of identity, therefore—viz., first, by their intermissions, second, by their succession to or alternation with intermitting fever—are thus clearly ascertained; and the third test, viz., *cure* by the *same* specific medicine, was also used by me for hemorrhagic dysentery, with a general result in both hospitals, and 347[1] cases, of

---

[1] The particulars of the result of my experiment for diarrhœa and dysentery was, in the *General Hospital*, 98 admissions and 7 deaths, or 1 death in 14 cases; the average there for the last twenty years having been 1 death in every 5¾ cases. I had, therefore, nearly 1-3d only of the average mortality. Again, in the *Queen's regiment*, treated by Dr Hare, 249 admissions and 10 deaths, or 1 death in 25 cases. Treated by the surgeon, 179 admissions and 21 deaths, or 1 death in every 8½ cases. My mortality, therefore, in the regimental hospital, was 1-3d that of the surgeon's during the same year.

1-3d the average mortality, and 1-3d also the surgeon's mortality, whose cases were treated at the same time and place. When I say hemorrhagic and malarious dysentery, I mean cases where, as Dr Johnson's, "I was passing blood fast;" that is, where pure blood is passed in quantity, being hemorrhage, in fact, from the bowel, or where remissions of febrile symptoms and of purging can be traced, and especially if the dysentery have followed an attack of fever, which is very common. In other cases where bloody mucus is passed mixed with streaks of blood, more arterial than venous, or dark, bilious stools, tinged with a trace of blood, or, again, yellow, frothy stools, with mucous and no blood, I do not say that *all* these are malarious dysentery, nor did I treat them in my experiment with quinine.

I found it very difficult to cinchonize cases of malarious dysentery, and have frequently given 6 and 7 scruples daily to some patients where I saw that death must result except strong means were used. I pushed the quinine with them to an excess never perhaps before attempted. I have produced with it not only deafness and dizziness, so that the patient could not stand, but blindness also, lasting on one occasion three days; yet these symptoms were almost the certain prelude to recovery. The bleeding from the bowel ceased on their appearance, often a severe accession of fever came on, and the patient was in a short time convalescent. The effect of cinchonism on the eye was much like amaurosis, with expanded pupil insensible to light. The deafness disappeared last, and sometimes continued partially for a week. Longer than this I have not seen any symptoms of cinchonism continue.

My researches in the records of the General Hospital, elicited another remarkable fact. I noticed that dysentery had been treated in the earlier days of the hospital by scruple doses of calomel and mercurial frictions, whereas, latterly, since 1846 onwards, calomel had been avoided as a poison, and sugar of lead and opium given; and on examining the returns, it appeared, that the mortality of the first four years,[1] 1830–33, had been only 1 death in 6, whereas, in the last four years, 1846–49, it had been 1 death in 3. Dr Martin's tables, p. 61, also shew, that for 12 years, from 1826 at the General Hospital, the ratio of mortality for dysentery was only 1 death in 6. About the year 1833–34, Dr Twining's treatment by blue pill and ipecacuanha was introduced, and mercury in this milder form was given freely, almost without a change in the practice, till 1846, when sugar of lead replaced it. The mortality during this period continued steadily, 1 death in 4; but from 1846 to 1850, it rose immediately to 1 death in 3.

---

[1] See the Tables i. and ii., at the end, for the General and Regimental Hospitals.

The records of the General Hospital afford, without doubt, the most accurate statistics of dysentery in the world. For this disease is the commonest of all there, and the treatment of every case is recorded, so that the effects of remedies on large numbers can be most minutely ascertained, from its weekly, monthly, and annual returns, deposited in the office of the Medical Board.

But to assure myself more thoroughly of this strange fact, I obtained also the returns of the European regiments stationed in Calcutta for the same 20 years.[1]

Mercury has never been so entirely relinquished by regimental surgeons as in the General Hospital. Blue pill has, however, supplanted the former scruple doses of calomel and inunction; and, accordingly, the mortality for the first four years 1830–33, was only 1 death in 9; whereas now, from 1846–49, it has been 1 death in 6½. Also, in periods of ten years, from 1830–39, it was 1 death in 11¼ only, and from 1840–49, 1 death in 9. All statistics, therefore, of the most infallible kind, show a large increase of mortality, by what is called our modern mild system of treating dysentery in Bengal; and where mercury has been entirely omitted, even a a double mortality to what it was when salivation was early and eagerly sought for.

But salivation was before proved to be a specific for malarious fever; here, therefore, we again attain evidence of fever and dysentery being cured, as Dr Holland says, by the *same* specific remedy, and it has long been my wish to try a third specific remedy for ague, viz., arsenic, in *all* the forms of malarious fever and dysentery. Since I left Calcutta, I have been on active service in Burmah, with no regular hospital, and none of the necessary means for experiments. I have, however, already tried it in a sufficient number of cases to convince myself that the result will be as I anticipate. If, then, by single tests, viz., bark and colchicum, many diseases have already been clearly ascertained to be caused by simple poisons, and arranged under their single causes; how much more having three tests, which cube the value of the proof, can we demonstrate and positively separate *all* malarious diseases, both acute and chronic, under the head of one blood poison, thus greatly simplifying our nosology, and by the various observed effects of this *one*, guide our reasoning in search of the analogous effects of *other* blood poisons, such as cholera, &c.

---

[1] See Table at the end.

Moreover, to know certainly what the effects of an unknown poison are, is much ground gained towards its discovery, or at least, which is a great advance, this knowledge enables us to decide positively what it is *not*, viz. (as has been held), sulphuretted hydrogen, &c., because inspired in any quantity sulphuretted hydrogen produces none of the symptoms of acute malarious fever, nor can they be cured by quinine.

Another very strong proof of the antidotal nature of the effects of bark and salivation, is the well-known fact, that the taking bark every morning during malarious seasons, will give immunity from the disease. This fact has been noticed very often by officers in India, who have thus been in the constant habit of venturing with impunity into unhealthy jungles, in search of tigers and other sport. Lind also says, p. 186 and 257, that the governor himself, and a few others, on the coast of Senegal in Africa, drank every morning a cup of bark and orange-peel. The bark was scarce, and few could do this ; but those who drank it were the *only* survivors of the party, and they all continued in good health. I could multiply such examples, had I space, and moreover the proofs as I noticed before, of the safety which salivation also gives, in otherwise deadly situations. My own experience, too, suggests the same effect for arsenic, which, in the continuance of its effects, and securing patients from relapse, is undoubtedly far superior even to quinine.

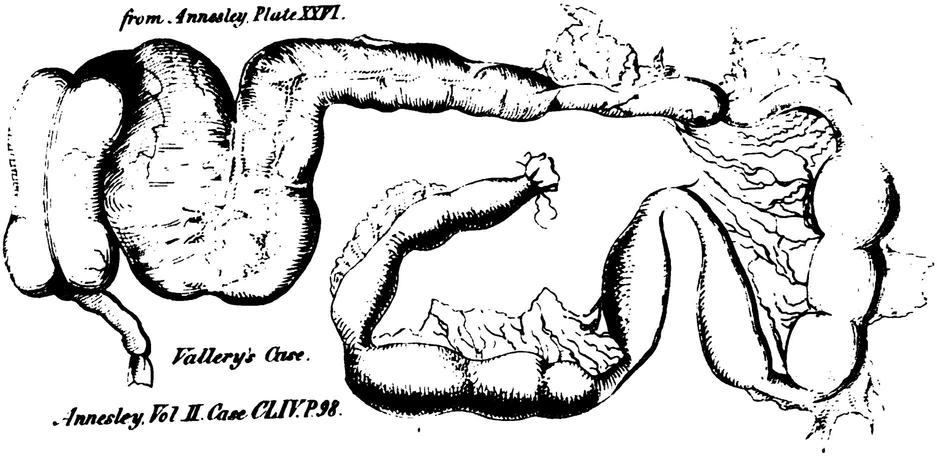
But to return from this digression, I must add, that I used in addition to quinine for malarious hemorrhagic dysentery, large and very frequent injections of cold water, containing sugar of lead or alum, for the purpose of checking the extensive sloughing, and hemorrhage from the mucous coat of the colon, till the specific remedy quinine could take effect by relieving the abdominal congestion. My treatment for common inflammatory dysentery I had already published before my experiment in 1847, and in the *Edinburgh Medical Journal* for 1849.

Dr Annesley describes this commoner form of dysentery thus, p. 152 :—" The view we have given of disorders which depend on accumulations of morbid secretions, and local matters, in the cavity of the larger bowels, shews, in a very remarkable manner, one of the earliest pathological states of dysentery.

" Collections of excrementitious matter tend very evidently to irritate and inflame the mucous surface on which they lodge, and cause ulceration, and even sphacelation, in a very

FIG: 3.

from Annesley, Plate XXVI.



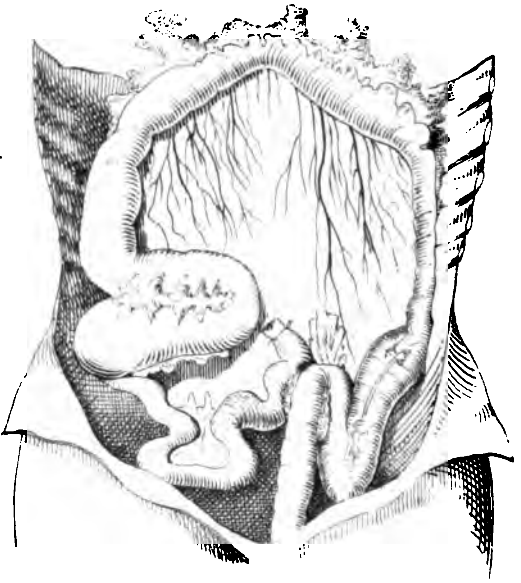
Vallery's Case.

Annesley, Vol II. Case CLIV. P. 98.

FIG: 4.

Annesley Pl. XXX

Cavanagh's Case, Vol. I. P. 492.



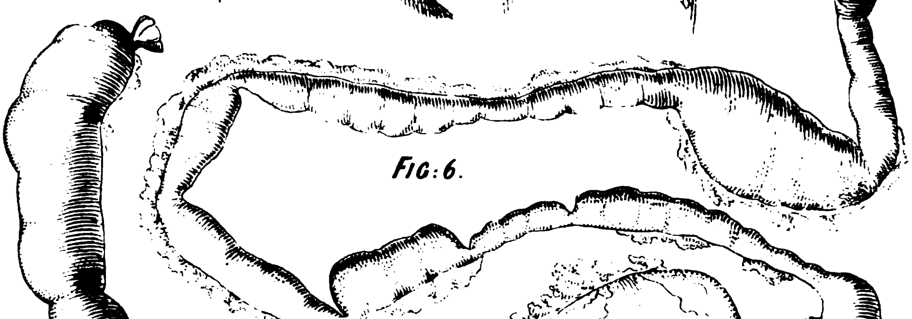


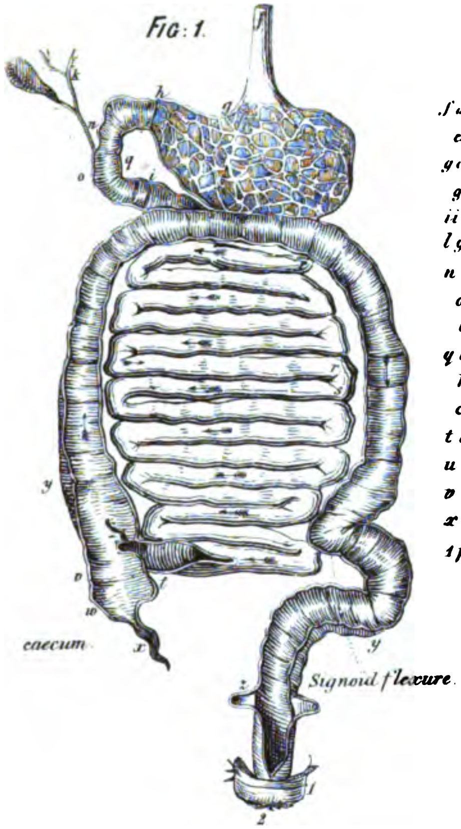
FIG: 6.

FIG: 5.

Connelly's Case
Vol. I P 261.

Diagram from Elliotson's Physiology. NATURAL STRUCTURE.

FIG: 1.



REFERENCES.

f oesophagus. the upper part having been cut away to shorten the drawing.
g cardia. h pylorus, the space between g and h. being the cavity of the Stomach.
ii duodenum. k hepatic duct.
l gall bladder. m cystic duct.
n ductus communis choledochus formed of the two o the opening of the choledochus into the duodenum. p pancreatic duct
q its opening into the duodenum which here is distinct from that of the choledochus. r jejunum. s ilium.
t termination of ilium in caecum.
u superior fold of the valve of colon.
v inferior of ditto. w caecum
x vermiform process. yy colon. z rectum
1 part of levatores ani. 2 anus.

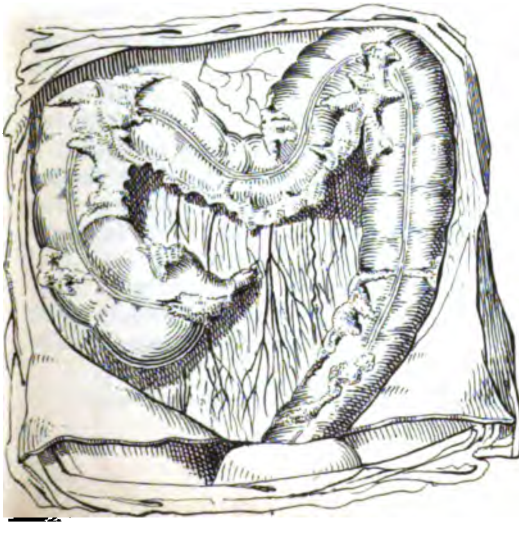


FIG: 2.

From Annesley. Plate XXIV.

Vol. 2nd P. 101. DISEASED STRUCTURE

WYLLEY'S CASE.

short period, if neglected or injudiciously treated. In a great many cases dysentery is preceded by costive bowels, often of long duration. In a few instances, when the evacuations are copious, the diarrhœa subsides, and the patient escapes, at least for that time, a true dysenteric attack. This result seems to arise from the irritation produced in the bowels by the fæcal accumulations having subsided in consequence of the irritating matters having been removed. Frequently the dysenteric symptoms are present from the first, the stools being scanty, streaked with blood, and abdominal pain and tenesmus. In cases of this nature, the increased action of the muscular coats of the bowel—add also the swollen state of the mucous membrane, especially about the sigmoid flexure and rectum—prevents the passage of the fæcal collections through the canal, and, in many cases, occasions a complete obstruction, little passing away but the perfectly fluid secretions. In cases of this description, if the disease be not early subdued by very decided treatment, sloughing of the mucous coat takes place, followed by involuntary motions; when the fæcal accumulations at last come away, such parts of them at least as have been dissolved, being washed off by the watery secretions, poured from the irritated vessels of the inflamed surface."

This is the result of Dr Annesley's large experience, and in most expressive language he shows the fatal effects of accumulations of fæces and acrid secretions in dysentery, and that the danger of this disease arises solely from their retention. Dr Gregory also says,[1]—"But the employment of purgatives constitutes the most important part of the cure of dysentery. They must be steadily persisted in, till fæcal evacuations have been procured, and the sensation of load in the bowels completely removed, which leads to the effort of straining; and then, and not till then, may the practitioner desist from the free use of cathartics."

Dr Raleigh remarks,[2] that purgatives are most requisite, but urges the use of those of an unirritating nature.

But there is the difficulty. We can perceive the strong objection to giving large doses of irritating purgatives, in a raw and ulcerating state of the bowel, and yet, from the dreadful results of delay, and the obstinacy of the constipation, see what fearful doses are constantly required. The 10 to 20 grain doses of calomel, and 3 to 4 ounces of black dose, given by Annesley night and morning, would excoriate most of our healthy bowels ; how must it then injure and hasten to ulceration the acutely inflamed intestine, and yet they must be given; for unless the irritating secretions are expelled, death must result. For read what Bampfield says, p. 118, the most practical writer we have :—

---

[1] Practice of Physic, p. 550.

[2] On Dysentery, p. 75.

"Let it be remembered, that *however great* the quantity given, purgatives must be continued, till their purgative effects be produced, and in all cases this should be accomplished in 12, or at furthest in 24 hours, (my plan does it in 10 minutes, and with a little soothing, warm water), for the obstinacy of the constipation generally bears its proportion to the severity and danger of the disease" (that is, the more inflamed the intestine, the more necessity to irritate it with repeated excoriating purgatives). "It may surprise," he continues, "many of my readers to learn that a purgative result is as difficult of attainment, in some cases of tropical dysentery, as in painter's colic. By perseverance, and yet disappointment in the use of cathartics for many hours, the patience of the sufferer may be almost exhausted, and the resolution of the practitioner to persist may waver; but purgatives must *never* be abandoned, he must pertinaciously persevere in their use, till the intestine be emptied. I should not insist so strongly on the necessity of resolute activity, had I not observed the fatal effects of an indecisive and temporizing practice. I used to prescribe 8 to 12 grains of calomel twice or thrice a-day, till it salivated and purged; to which, however, from its uncertain operation on the bowels, I soon added 30 grains of jalap, or an ounce of Epsom salts. These, with mercurial frictions, generally purged; but one day I met with a case in which they did not purge till the third day, and the patient died on the fifth, thus announcing the danger of this indecisive practice" (viz., 36 grains of calomel and 30 of jalap daily).

"In 1806," he says, "three more men fell a sacrifice to this indecisive treatment; as its purgative effect did not ensue till the third or fourth day, and ptyalism could not be excited. Since that period I have given cathartics in the following manner"—and then he tells us what he gave in his improved practice. These are two of his cases, p. 122.

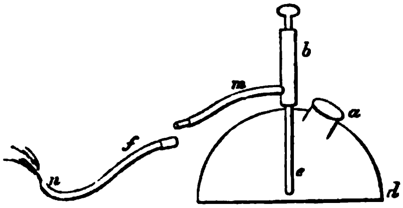
Angus Graham took 110 grains of calomel, 180 grains of jalap, 10 ounces of Epsom salts with infusion of senna, and 8 grains of ipecac.—all this in 36 hours—when he passed only one fæcal stool.

Robert Allen took in 36 hours, 90 grains of calomel, 132 of jalap, 120 of aloes, 140 of extract colocynth, an emetic, and an ounce of castor oil; and the next day, 20 grains of colocynth and jalap, 10 of calomel, and 1 ounce of castor oil.

He rested not here, however, but says, "Small pieces of fetid fæces, not bigger than peas, got enveloped in the mucus of the intestines, and are retained, notwithstanding the fæcal evacuations are frequent. An assiduous renewal, therefore, of cathartics is necessary till the intestine becomes quite easy."

Dr Annesley's book on Dysentery is full of similar cases. Bampfield, moreover, was a man of great experience, and says, p. 54:—"I have treated dysentery in many parts of Europe, America, Africa, and West Indies, China, and the Pacific Islands, and have not been able to discover any essential difference between dysentery in these countries and in the East Indies;" and, indeed, Annesley's description of dysentery, who practised only in Hindostan, is virtually the same as Bampfield's; see his passage quoted above. Dr James Johnson also, who saw dysentery in the East Indies, remarks, pp. 375, 360:—"That no doubt purgatives will cure if they act, but the difficulty is to make them act, till ulceration has begun, and it is too late."

The treatment for this form of dysentery, which I pursued in my experiment in Calcutta, was this. The moment a patient was admitted, his bowels were thoroughly washed out by a powerful syringe, with a flexible tube passed above



a. Funnel.

b. A good sized pump, with a tube *e*, reaching nearly to the bottom of the reservoir, to prevent air being pumped into the bowel, *d* a reservoir capable of holding at least a gallon.

f. A flexible tube opening at the top *n*, and not at the sides, and with a socket fitting over, not into *m*, the other tube from the pump.

The tube *f* ought to be smaller in size than is commonly used, and no bulging portion at its end *n*. It ought not to have its holes at the sides, because the bowel is in contact with these, and the water comes back again often by the anus. It ought to squirt with the full force of the syringe from its point, directly up the bowel. In passing it, open the sphincter with two fingers, and let the tube in passing rub against the fingers, and not the edge of the bowel. Let it also be warmed and well oiled. I have passed it in this way in many hundred cases, and in some even where the rectum was ulcerated, with very slight pain, certainly far less than that of the dysentery which it immediately relieves.

the sigmoid; warm water without limit in quantity was then slowly injected by a powerful pump, till the patient complained of the distension, and the abdomen was visibly enlarged. This passed away by stool, completely emptying the bowel, and always gave the greatest relief, putting a stop at once to the straining constantly at stool, without passing anything but a little acrid fluid, or bloody mucus. These painful symptoms were at once removed by the large enema. If the patient was plethoric, or had much febrile heat of skin, or pain on pressure to the bowel, he was bled once fully, and took a good dose of opium. The man then soon falls asleep, generally with copious perspiration from the warm water and opium. When he awakes another large injection is given to wash out any remains of irritating secretions, and leeches, if necessary, with fomentations were applied to any part of the colon which was painful on pressure, and a third injection given before night, with another dose of opium. The patient is generally by this time convalescent, but the injections are carefully continued, whenever the slightest griping or uneasiness recurs during the next day or two. The same treatment by injections is also equally necessary when the colon contains not solid but principally liquid excretions. For these, whether bilious or secretions from the inflamed mucous membrane itself, are always in all forms of dysentery intensely acrid.

The whole object, then, of the treatment is to wash these carefully out, to constantly renew the ablutions and fomentations with warm water enemata, and to calm with opiates or bleeding the constitutional excitement; to place, in short, the inflamed bowel in a state of perfect rest, and keep it so, by never allowing its acrid contents, whether fluid or solid, again to remain in it. If, however, there be the constitutional symptoms of malarious fever, and much hemorrhage from the bowel, I then gave as above quinine to saturation, and for the present delayed the bleeding as much as possible, by injections of cold water, containing sugar of lead or alum, which, at the same time, cleanse the colon of all fæces or irritating fluid secretions. Now, it will appear wonderful that such simple and obvious treatment should have escaped notice, for so common and so fatal a disease, during so many centuries, and neither has it failed to be observed by many, I may say by all, who have treated dysentery. But till O'Beirne discovered that a flexible tube can be passed into the bowel, without danger or pain, beyond the sigmoid flexure, injections, however much desired, could not be given in the more severe cases; for the rectum is so sensitive, and so largely supplied with nerves, that when inflamed, it immediately contracts on the injection, and expels it, if more than 3 or 4 ounces be attempted; whereas, to wash out the colon thoroughly, requires 6 or 7 pints. Bampfield did not use injections, because he says, p. 126, "that they were rejected without fæces, immediately they reached the rectum and lower part of the sigmoid flexure."

Annesley, p. 279, evidently likes injections, but never used more than 12 ounces, because "the slightest distension of the rectum and sigmoid flexure, expelled them again without fæces, only a little fluid matter; but even this (he says) is an object worth attaining, and should not be neglected."

By passing an elastic tube beyond the sigmoid, I have found, in more than 300 cases of the severest form of dysentery, not the slightest difficulty in washing out the colon from cæcum to anus. It is, in fact, the most cleanly and best way possible for administering an enema. The flexible tube, *separated* from the syringe, is introduced under the bed-clothes without exposure. At a movement of the hand the assistant brings the syringe, fixed on a small stool, places it by the bed-side, attaches the flexible tube, and moves the piston up and down with a steady, slow motion. I trained my assistant to perform the whole process without a word uttered. The finger was raised and he ceased pumping, it was depressed and he renewed it. A sign was made with the hand, he took away the whole apparatus from the bed-side, and the elastic tube was then removed from the bowel. The most delicate lady could not object to such an operation. The relief too is instantaneous, and the bedding is not constantly wetted, as by the common short enema pipe, from the water running backwards from the anus. Since the publication of my first pamphlet, there has been much unnecessary discussion whether the long tube, as it is called in India, is always necessary. My reply is, that the short rectum tube will often be sufficient; but, in very many cases, and these the most dangerous, it will *not*, as Annesley and all others acknowledge. But the treatment of dysentery by enemata does not depend on a discussion upon long or short tubes; its basis is simply this—to wash out the bowel thoroughly and repeatedly, and let each decide for himself how he can most easily and effectually accomplish this end. My own experience is decidedly in favour of an elastic tube in all cases, as far more effective and less likely to injure the bowel than a metal rectum pipe; moreover, the water does not run back and wet the bedding. Most of my patients have preferred the elastic tube as less inconvenient, and have often asked for and passed it themselves two and three times a-day whenever they felt griping. Another argument has constantly been raised, whether scybala and solid fæcal matters are found in dysentery. In Bengal dysentery, undoubtedly, they very rarely are,—that is, after death; but that some fæces must be constantly in the colon when dysentery first attacks the patient, no one, I think, can deny; and it is at this early stage that the mischief is done. Dr Annesley says (p. 152), "Frequently the dysenteric symptoms are present from the first, the stools being scanty, streaked with blood, and abdominal pain and tenesmus. In cases of this nature, the increased action of the muscular coats of the bowel, and also the swollen state of the mucous membrane, especially about the sigmoid flexure and rectum, prevent the passage of the fæcal collections through their canal, and in many cases occasion a complete obstruction, little passing away but the perfectly fluid secretions. In cases of this description, if the disease be not early subdued by very decided treatment, sloughing of the mucous coat takes place, followed by involuntary motions, when the fæcal accumulations at last come away, such parts of them at least as have been dissolved being washed off by the watery secretions poured from the irritated vessels of the inflamed surface." Again, he says (p. 163), "The reason why scybala and solid fæces are seldom found in East Indian dysentery is, that the serous fluid, copiously secreted from the irritated intestine, washes it, squeezed as it were, through the restricted canal in a liquid form."

Dr James Johnson remarks (pp. 359, 361):—"By bleeding, intestinal strictures are relaxed, and scybala and fæcal accumulations pass off; and if the patient dies at a less advanced period of the disease, or where mortification has not relaxed all stricture, fæcal accumulations are found in the colon above these strictures." He means by strictures, what Annesley says is the canal obstructed from swelling of its mucous membrane from inflammation. This dispute, however, about solid fæcal matter appears to me by no means required; for it is acknowledged by all that the stools passed, and therefore the liquid contents of the colon, are in dysentery *invariably* of a most foul, acrid, and often putrid character. Without doubt, therefore, the solid fæces are, if anything, less acrid and irritating than the liquid; and therefore there is still greater necessity for large injections, to dilute and wash these last thoroughly from the inflamed mucous surface of the bowel, where, if allowed to remain, they must cause ulceration or sloughing sooner even than solid fæces.

The above, it will be observed, are simple deductions from the facts recorded by all our best and most experienced practitioners; but if the morbid anatomy of dysentery be carefully considered, it appears to me that its treatment by enemata must follow as a direct corollary from the appearances there observed. I need not insist here on the unanimous evidence which we have that the colon is the seat of dysentery, and that the small intestine is almost invariably healthy, or if the ileum be inflamed, it is but a small part of it, in immediate contact with the highly inflamed and sloughing cæcum, and that the vascularity shades off (see Annesley's plates) from this point as its focus, rapidly decreasing as it recedes from the valve of the cæcum, but even this is very rare. Of the 160[1] cases of fatal dysentery in the General Hospital, which have occurred for nine years up to 1850, in only one was there more than slight abrasion, "whereas the large intestine was ulcerated in all, chiefly at the cæcum, sigmoid flexure, and rectum." But it is needless to repeat a fact acknowledged by every one. Now the question naturally arises, Why the disease is so completely confined to the space between the valve of the cæcum and rectum?[2]

Sydenham says, "The reason is, that the corroding secretions easily flow off in the small intestines, and therefore cannot cause there excoriation and corrosion, as happens in the large, particularly in the colon, where the morbid secretions being entirely stagnant, corrode without limit the mucous membrane. Therefore, I have found, as I should have supposed, that abstergent clysters and warm water is the best."

Bampfield, too, says, p. 228, "Experience has fully persuaded me that the morbid secretions excoriate and excite the ulcerative process in the villous coat of the intestine."

Sydenham's observation is without doubt the true one. The small intestines loose, and ever in motion, pass on rapidly their contents, but in the fixed colon they are delayed, and, being "*morbid*, corrode without limit the mucous membrane."

In animals, but particularly the horse, where the stomach is very small, the food, and even water, rapidly passes from the stomach, through the small intestines, to be digested in the colon, the secretion from which is acid. In the stomach and small intestines it is reduced merely to a liquid, pulpy form; but when it arrives at the colon the valve closes on it, and being shut up between this valve and the sphincter at the anus, is delayed sometimes for many days, till its fluid and nutritious portions are absorbed, and the remainder at last slowly ejected as dry and hardened fæces no longer containing any matter capable of absorption. The cæcum in the horse is very large, and is called its "water stomach," from the rapidity with which water taken into the stomach passes into it, through the small intestines, to be there detained and absorbed. Any one comparing this with the colon of a man, must plainly see that it is formed by nature to accomplish the same function.[1] There is the valve to shut it up from behind, and the sphincter and sigmoid flexure to close its other orifice, and its contents are slowly passed upwards against gravity, and detained by gravity in its horizontal portions, before it arrives at the descending part and rectum. They are even then prevented from pressing hastily on the rectum, and thus exciting too early evacuation, by the beautifully contrived twistings and serpentine bendings of the sigmoid flexure. This sigmoid flexure and the rectum are far more copiously supplied with nerves than the rest of the colon, which is doubtless the reason why in dysentery it is so irritable, and contracts on the smallest injection, whereas fluids injected beyond the sigmoid pass up almost without sensation. This mechanism, therefore, gives us a satisfactory reason why the colon should alone suffer. For the acrid secretions pass rapidly along all other parts of the bowel, and are detained only there. The whole mucous membrane of the colon thus becomes inflamed and swollen; but the principal obstruction arising from this swelling is necessarily in the bendings of the sigmoid: tormina and tenesmus, with constant rectal straining, nothing passing but a little liquid fæces and acrid fluid, are the early symptoms therefore of dysentery. Frequently the swollen mucous membrane protrudes through the anus, causing intense suffering, and the whole colon is distended and painful on pressure, but particularly at the cæcum, and above the sigmoid. At length the contents of the bowel begin to pass more freely, but not before irreparable injury has been done to the colon. Twining, p. 60, thus describes the symptoms of this stage:—"In three or four days after the attack the stools have a horrid odour, resembling putrid blood. It has been compared to the anatomist's macerating tub. Now, suppose a tube, Fig. 1, closed at the two extremities, *a* and *b*, and partially filled with fluid or semi-fluid matter. This, if moved about in it, would by the action of gravity necessarily accumulate more or less equally at the two extremities[1] of the bend, *a d* or *b c*. Now read what Parke says, p. 14, and compare it with the above:—" Two important anatomical varieties of dysentery are those in which ulcerations are found, chiefly in the cæcum, and those in which ulcerations are principally confined to the rectum. The varieties are also generally the result of *local irritants*, and produce peculiar symptoms. Thus, when the cæcum is the part affected, there is great pain on pressure over the cæcum, and a very perceptible fulness in that region, arising either from arrested fæces or secretions, or inflammation, and œdema of the coats of the cæcum. If the inflammation runs high, ulceration and destruction of the ileo-colic valve ensues. (The cork rots from the burning nature of the contents of the bottle.) When the rectum is principally affected, there is intense tenesmus, the stools are nearly pure dark blood; and if the case be neglected, some portion of the mucous membrane speedily sloughs and protrudes through the anus. (The cork rots at the other end if the acrid fluid run over principally to that side.)

---

[1] See Macpherson's Statistics of Calcutta Dysentery, p. 34.

[2] *De curatione Dysenteriæ*, 1818, p. 11.

---

[1] Dr Watson (Lect. p. 644) cured a patient, after feeding him for five and a half weeks entirely by injections into the colon of beef-tea and egg.

Fig. 1.

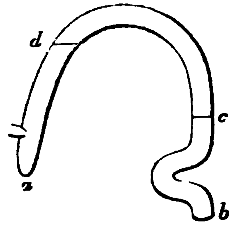
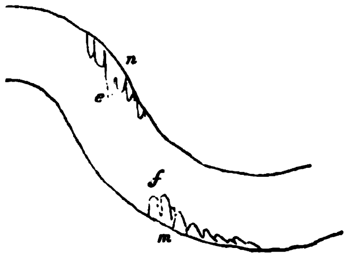


Fig. 2.



Suppose this experiment:[2] Take any part of the living intestine, small or large, of an animal; place it in a syphon shape, as Fig. 1., with the bend uppermost, and obstruct with gentle pressure each end, *a* and *b*; suppose this portion of the intestine partly filled with a strong solution of sulphate of zinc, and move it about in the intestine till part of the liquid remain at *a d*, and part at *b c*, the two ends of the tube, would not these two ends be the first to inflame, ulcerate, and rot,—would not the rest of the intestine more or less escape, except where in moving the fluid the solution of zinc was brought in contact with the more prominent tops, *e*, *f*, Fig. 2, of the folds or wrinkles of the mucous membrane,—and would not the parts in or between them, and therefore concealed and covered by these folds, suffer less ? Dr Raleigh had great experience of dysentery in the General Hospital, Calcutta, and says, p. 29, "The mucous membrane preserves its integrity in the spaces or sulci between the elevated masses, whilst *over* the elevated portions it is extensively abraded by sloughing ulceration." Dr Baly, also, p. 338, "These gangrenous portions were generally situated on the transverse folds or plicæ." The writer in the *British and Foreign Review* remarks, p. 344, "This (effused) lymph has generally occupied the *summits* of the plicæ." The mucous membrane constantly, it is true, sloughs in mass, its whole thickness being destroyed, but still any theory of dysentery to be received must account for these lesions of its more exposed surface only, and also the intensity of the disease, confined to the cæcum and sigmoid; but except Sydenham's, I have not yet seen any which even attempts to do so. I have frequently seen one side of the valve of the cæcum perfectly healthy, and the other towards the colon covered with black sloughs, and, as far as my observation goes, the ileum is never seriously diseased, except the valve has been so much injured that it does not act, but allows the contents of the colon to pass through it. If, then, this theory be true, the treatment by injections follows as a corollary. Wash out the colon, and we place it in the same condition as the small intestine, and save it from disease. I do not speculate here on the primary cause of dysentery. I merely state these its known secondary effects, which can only be accounted for by the delay of acrid matters in the colon, of which the obvious cure is cleansing by repeated, copious, and early injections of warm water.

---

[1] This is precisely what must constantly take place in a patient with dysentery, constantly rising to stool, and in bed, with the head and shoulders slightly raised.

[2] I do not say that this experiment is practicable in a living animal. I only use it roughly to illustrate my meaning.

Chronic Dysentery.—If injections are valuable in acute, still more are they in chronic, dysentery. Annesley describes it thus:—"The inflammation of the acute stage leaves the villous coat of the colon throughout hypertrophised, its papillæ having an erect rough appearance and feel, of a dark red or purplish hue, with here and there livid patches of congestion, particularly at the caput coli and transverse arch, where ulcers are often found. The sigmoid flexure is often very much contracted, its villous coat of unnatural thickness and inflamed, as well as the rectum."

Now, I ask, is this a condition of the intestine likely to be improved by the irritation of constant purgatives? and yet the contractions[1] of the colon, together with the loss of muscular power and peristaltic action, necessarily succeeding the acute stage of the inflammation, render it absolutely necessary that they or some substitute should be given. The secretions, too, continue acrid, the appetite is often ravenous, while the powers of digestion are much weakened. The half-digested food and acrid secretions accumulate and distend the intestine above the contractions of the colon, causing great irritation, and constant purging of fluid matters. Purgatives have therefore at all times been unwillingly resorted to as a necessary evil; every writer inculcating the use of the mildest laxatives in theory, but in practice, in their *cases*, using from necessity calomel, black draughts, compound jalap powder.—(Annesley, pp. 366, 369, 377, &c.) To correct these they used freely opiates and astringents, which, in checking the dangerous symptoms of the purging, produce accumulations. The accumulations require again purgatives, and so on the changes are rung till the patient dies, or escapes only by the powers of a strong constitution, which is moreover too often weakened, if not destroyed, by the previous treatment for the acute stage. That this is no exaggerated picture of the present treatment of chronic dysentery, any one who has been much in a European hospital in India must acknowledge. See what Raleigh says, p. 117, of the wretched uncertainty of his treatment for chronic dysentery. I will illustrate this also by extracts from Bampfield, who describes from nature, with the ample experience which he had in his extensive practice. "This retention of fæces occurs in every variety of chronic dysentery, although the patient have frequent loose stools; and it probably takes place above the diseased portion of the intestine."—(P. 172.) "This state of retention is difficult to detect, for the patient at the time has frequent stools, which are not very scanty, and induce him to describe his complaint as a free and troublesome purging. A retention of fæces always causes gripes, flatulence, increased evacuations of morbid mucus and secretions, general fever and nervous irritation, only to be relieved by a purgative."—(P. 184.) He acknowledges (pp. 197, 199) the necessity of using astringents, and their great success for a few days; but says they are too apt to produce constipation, and its painful and disordered consequences. "The constipating effects appeared to be more disadvantageous to the patient than the astringent powers on the morbidly secreting vessels were beneficial, and I found that if too weak they produced no effect; if strong, they always produced constipation, and with it increased morbid secretions and tormina, and sometimes actual relapse."

---

[1] See a few specimens of chronic dysentery in the plate attached, copied from Annesley, who has drawn numerous other examples.

No one can read Raleigh's cases (pp. 118–148) without being convinced that the reason of the varying effect of the sulphate of copper was owing to the same cause. Annesley bears evidence to the same fact, p. 371: "It should always be kept in recollection, that the *necessary* employment of tonics, astringents, or anodynes, occasions accumulations of fæces, and requires purgatives. While this plan (astringents) is being employed, purgatives will be necessary, and no medicine can be more beneficial than a black dose."—(P. 372.) And yet he says, p. 376, "Active cathartics excite the raw and inflamed surface of the constricted part, and increase the morbid state, which it is our object to remove." Again, p. 378, "In cases of contraction of the colon, the chief object is to preserve the contents of the colon in a fluid state, to prevent accumulations of fæces from forming above the stricture, and the irritation they would occasion." Thus, the disease requires astringents to check it, astringents require purgatives, and purgatives aggravate the disease. See how Bampfield laments this necessity:—"It must be granted (he says, p. 199) that astringents display great powers over the morbidly secreting vessels; but it must also be admitted, that if they induce constipation, the morbid secretions are much increased, and it becomes necessary to abandon them. If the constipating effects of these astringents could be generally obviated by the combination of a defined quantity of any purgative medicine, which would not decrease their astringent effects on the morbidly acting vessels, then it would appear that the desideratum for the cure of this disease would be obtained."

Injections evidently fulfil this desideratum in the most perfect manner, and are infinitely preferable to this half-purgative, half-astringent system proposed by Bampfield, if it were possible, which it is not. By large injections we may wash out daily, with the most soothing applications, the excoriated intestine, removing its acrid secretions, and the fermenting half-digested fæces; fomenting its tender surface the meanwhile, and softly stretching the strictured parts with the gentle expansion of water. And when in the colon, thus daily cleansed and soothed, the inflammation and irritation have calmed, what numerous applications are there which can be applied by the elastic tube to the whole diseased surface of the colon, from the cæcum to the anus, gently to constringe the overstrained vessels, and heal the ulcers. The great value of such medicines as alum, nitrate of silver, sulphate of copper, &c. in ulceration of other mucous membranes, is notorious, and there can be no reason assigned why they should have less effect on the colon. I have already used them largely with perfect safety in the General Hospital, with most decided results; and have collected cases where the colon showed the scars of extensive ulceration up to the cæcum, and which were quite healed by the use of repeated injections of alum, and occasionally nitrate of silver. The long tube, in fact, changes an internal disease into an external, and enables us to apply the same treatment of cleanliness and various lotions to the colon, that we would, with certainty of cure, to ulcers of the mouth.

One other great advantage we also obtain by this treatment, that we can support the patient, emaciated by chronic dysentery, with good food, instead of the milk and low diet now given, and thus, by strengthening the constitution, greatly assist the healing of the ulceration; for we need not with injections fear the accumulation of undigested food in the intestine. Bampfield says, p. 234,—"The healing of an ulceration of the intestine is a process of nature, carried on under such disadvantages (irritation of fæces and constant purgatives), as will, from every probability, require a long time for its completion, even in the most favourable circumstances. It is indeed observed by Galen, that ulcerations of the intestines more readily admit of cure than of other internal organs; because medicines, injected in the form of enemata, become immediately applied to the affected parts; but it must be *evident* (says Bampfield) that this rule can only apply when the ulcer is within reach."

We have now, however, most effectual means of reaching them, and the success of which has been shown in my Calcutta experiment. If, however, we can trace fever, scurvy, bad water, &c., as the primary cause of the chronic dysentery, we must apply our remedies to these also, at the same time that we soothe and palliate the local disease. I must remark, in conclusion, on malarious dysentery, that if the above treatment by injections be not adopted, statistical facts of the most undoubted kind prove the necessity of our returning without delay to the salivating system. For the returns of the largest and longest-established Dysenteric Hospital in the world show that, since mercury has been avoided, the mortality has been double for many years' continuance what it was when salivation was sought for as the first and only object of the treatment. And to complete the remarkable proof of the importance of mercury (if my system by quinine and injections be not received), these statistics clearly show, that as mercury has gradually been disused, so the mortality has correspondingly increased. If statistics then are, as they ought to be, our only guide to rational practice, our path is clear,—we must return to salivation till some more successful method be discovered. But the fact that, in treating 346 cases in Calcutta, I had but 4¾ per cent. deaths, will, I hope, induce a trial of large injections by others, and thus prevent the necessity of resorting to the more injurious remedy, mercury.

Table of Mortality from Dysentery among the European Troops, Fort-William.

Years.	Admissions.	Deaths.	Years.	Admissions.	Deaths.	Years.	Admissions.	Deaths.
1830	238	22	1839	60	4	1847 to Apr.		
1831	232	29	1840	184	15	1st.[1]	27	5
1832	187	27	1841	147	6	1847–48	96	6
1833	158	13	1842	144	6	1848–49	198	18
1834	100	9	1843	81	21	1849 to Dec.		
1835	222	12	1844	60	4	1st.[2]	131	40
1836	121	5	1845	90	8			
1837	71	4	1846	114	11	Total,	2699	267
1838	38	2						

For the *first* 4 years 1830–33, 1 death in 9 admissions.
For the *last* 4 years 1846–49, 1 death in 6½ admissions.

In periods of 10 years—

1830-39, 1 death in 11¼ admissions.
1840–49, 1 death in 9 admissions.

Rangoon, Aug. 29, 1853.

---

[1] From this date the yearly returns were ordered to be from April to April of each year.

[2] December 1st was the date the table was compiled.

ART. III.—*Case of Catalepsy*. By JOHN COLDSTREAM, M.D., Fellow of the Royal College of Physicians of Edinburgh.

A lady, in her twenty-ninth year, unmarried, of sanguine temperament, ruddy complexion, generally healthy, but affected with lateral curvature of the spine in a moderate degree, on account of which she was subjected to a variety of treatment between the eighteenth and twentieth years of her age, had an attack of simple fever, in May 1852. During convalescence, she spoke incoherently, and manifested aberration of intellect, chiefly on the subject of the disposal of her money, which she wished to lavish on others more freely than was consistent with her circumstances. Her medical attendant advised change of scene, and she was brought to Edinburgh, about three weeks after her first seizure. Very soon she ceased to advert to the subjects which she had previously dwelt upon, but continued, for some time, in an excitable state of mind—irresolute, capricious, shrinking from society, and unable for any occupation requiring more than ordinary attention. In the course of the summer, this abnormal condition gradually disappeared; the general health was restored, and, by the end of August, the patient seemed to be as well as she had ever been. During the autumn she resided chiefly in the country and on the West coast, and enjoyed changes of scene and society as usual. In the course of the month of September, however, she began again to manifest a disinclination for intercourse with family friends, and the expression of her countenance indicated mental uneasiness of some kind. Now, as formerly, she averted her eyes from the person who conversed with her. She became unwilling to take her usual exercise out of doors. She sometimes wept, and expressed anxiety regarding her spiritual interests. In this state she returned, in the beginning of October, to Edinburgh, where she remained till the end of February. During these five months, the patient passed through various phases of mental disorder, being generally unwilling to go out of doors, yet yielding to advice so far as to take pretty long walks, and to drive out several times into the country. She also went to public places, but so as to avoid being seen or spoken to by acquaintances. She read, and seemed to enjoy simple books; but she was evidently unfit for the energetic use of her mental faculties,—originally of no mean order, and highly cultivated by education. Her chief occupation at this time was in sewing and knitting. Her mental perturbation was aggravated in paroxysms, some of which coincided with the menstrual period, preceding it, and relieved by the flow. During these exacerbations, the head was hot, the face flushed, and there was a disposition to weep. On two occasions, the patient refused to speak for twenty-four or thirty hours in succession; choosing rather to express herself in writing, which she did freely. Her silence was evidently voluntary. She gave no further explanation of it, than by writing that she "felt very nervous and somewhat giddy." A note, written on one of these occasions, now before me, states that, on a certain day, when at dinner, she felt "a sensation in her eyes, and also in her jaw; that she thought her food did her good, but that she had a degree of giddiness and nervousness." The same note bears also these words, addressed to me: "Since you were here, I have been extremely nervous; was much worse in the evening; did not sleep in the beginning of the night, and was really ill; my limbs got cold. To-day (18th Feb.) I am extremely nervous; I am seriously ill." From the state in which she used these strong expressions to characterize her sensations, she soon passed into a more tranquil condition, presenting no indications of either mental or bodily suffering. Her symptoms varied from day to day; but, during all this period, there was no recurrence of the mental aberration which manifested itself so decidedly at the commencement of her illness. Nevertheless, she frequently asserted that she regarded herself as being on the verge of insanity, and requested me to have her placed in a lunatic asylum. In consultation with my friend, Dr Smith, it was decided that another trial of change of scene should be made; and, in the beginning of March, the patient left Edinburgh.

For a very few days after her removal to the country, the patient appeared to improve; but, gradually, she became more and more unwilling to go out of doors, and to see strangers. Fits of extreme obstinacy now occurred; she would not go to bed; she would not take food; she would not speak for hours together. On the 13th of March, on being visited for the first time by Dr Naismith, from whose account I quote, "she complained of feeling 'very ill,'" but would give no further account of her sensations. Her manner was peculiar; she seemed depressed, never raised her eyelids, and spoke only in answer to questions. No indication of bodily indisposition existed, excepting coldness of the extremities." For two or three days subsequently to this time, the patient continued to make the same indefinite complaint of feeling very ill. Her manner remained unchanged. She expressed aversion to the presence of strangers; and, though tractable on most points, decidedly refused to leave her bed-room; she took her food well, desiring butcher-meat more than usual. On the morning of the 18th March, having been with difficulty persuaded to get out of bed, she threw herself on the floor, and remained there in her night-dress, in spite of all remonstrances. On being told that Dr Naismith was expected to visit her immediately, she said—"In that case, I will not speak a word;" and from that time forward she was speechless. "She was lifted back into bed," writes Dr Naismith, "and on my visiting her shortly afterwards, I found her in that cataleptic state, which, continuing in greater or less intensity, gave so remarkable a character to the case throughout its subsequent course. The changes that appeared in the symptoms after this were, I would say, rather in degree than in kind. The patient's removal to another residence on the night of the 29th March, had the effect of throwing her, for the time, into a state of what seemed to be absolute want of consciousness, as well as of volition. For a few days previously, she had been employing herself in needle-work; and readily made signs in answer to observations addressed to her. She never did so after the removal just referred to."

By the middle of April, the patient had fairly entered the last phase of her illness, in the course of which the extraordinary phenomena which I am about to describe presented themselves. Her state from this time until the middle of September, when she died, was characterized by almost constant cataleptic quiescence, rigidity, and unwillingness to take food. From the time of the sudden seizure above mentioned, the patient lay almost constantly on the back, giving no indication of hearing, seeing, thinking, or even of feeling, excepting for about an hour each morning, when (until within a few weeks of death), she walked through her room, assisted in washing and dressing herself, and moved her arms and clapped her hands for the sake of exercise, so long as no one but the nurse was present. If, while she was so engaged, any other person entered the room, she immediately became cataleptically motionless and rigid, in whatever attitude she happened to be at the time of the intrusion; and this state was maintained so long as the intruder remained in the room. At other times, in bed, and on the sofa, the muscles of the limbs, and of other parts of the body, both sides alike, were affected with cataleptic rigidity whenever an attempt was made to move them. And yet it was only an attempt to move the limbs or body, so as, in some degree, to stretch the voluntary muscles, that produced any sign of consciousness or sensation, and this was always the cataleptic rigidity or contraction, which was not induced by the most considerable degree of irritation applied to the skin. Indeed, the whole body, including the eyes, the auditory canals, the nasal passages, appeared to be affected with *anæsthesia*. But of this, more hereafter. She never spoke while awake after the 18th March, excepting on two occasions, one of which was during the action of an electro-magnetic apparatus, which forced her to cry out; and the other was within a few hours of death, when she faintly uttered one word. She made no voluntary movements beyond those necessary at her toilet, excepting when the nurse once asked her to address a letter for her, which she did. Although, in general, no words addressed to her in conversation seemed to make any impression upon her, and evoked no kind of response, by whomsoever they were spoken, yet she was on a few occasions seen to weep, and even to sob audibly, when affecting narratives, or passages from books of a spiritual kind, were read in her hearing; and, in one instance, when she was visited by an old domestic, to whom she was much attached; yet music seemed to produce no effect whatever. Notwithstanding her apparent inattention and insensibility, her attendants believed that she was conscious of all that passed around her, and they acted on the belief, cautiously avoiding everything at all likely to wound her feelings. The nurse observed that, at times, while in bed, and apparently unconscious of being watched, she looked about her with an intelligent and observing eye, without, however, moving any other part of her body. She seemed to sleep during the night regularly; occasionally she muttered, and even spoke distinctly, a few words in sleep; but there was not the least attempt at somnambulism.

Perhaps the most important (although not the most singular) of all the symptoms was the resistance offered to all attempts made to supply the patient with food. This feature first presented itself about the beginning of April, but was not permanently established until towards the end of the same month, after which time, every attempt made to administer either food or drink by the mouth induced violent contractions of the muscles both of the lips and of the jaws, rendering it impossible to get anything into the mouth without first forcing a strong wedge between the teeth: the closure of the nostrils was then required to accomplish the swallowing of each mouthful. But, during this troublesome process, repeated many times a-day, there was never the least attempt made by the patient to push away those who were feeding her. Her whole body continued in its ordinary state of quiescence, the muscles of the mouth and jaw alone excepted. Sometimes, however, the face became much flushed.

For some time after the reluctance to take food was manifested, the attendants did not succeed in getting the patient to swallow a quantity sufficient for due nourishment. The consequence was great emaciation, which, at the end of three months, became extreme, although then more food was introduced.

Three months were passed in the country after the establishment of the cataleptic state, with but little change; when it was considered expedient to remove the patient again to Edinburgh. The removal was effected by rail, without difficulty, but without the least expression of consciousness on the part of the patient, until she was told that she had reached Edinburgh, when she first smiled, and then shed tears. She now came again under my care.

As there was but little change in the symptoms from day to day, I propose to arrange my observations on the case as it presented itself after this period, under groups, corresponding to the five prominent features of the case, which were,—1. Cataleptic rigidity; 2. Quiescence; 3. Anæsthesia; 4. Speechlessness; and, 5. Resistance to the introduction of food.

1. *The Cataleptic Symptoms.*—At all times, when awake, the patient seemed to be ready for the manifestation of cataleptic phenomena. During the day, when she generally lay on a sofa, the cataleptic stiffness was induced whenever any one, even the nurse, took hold of any part of her body, so as to subject it to passive motion. Uniformly was it found, at least in the latter weeks of the malady, that a slight degree of motion was not adequate to the induction of the cataleptic state. So that even when the arm (for instance) was extended at a right angle with the body, and fixed in that position by the action of the muscles of the shoulder, the fore-arm could be moved by a gentle touch to the extent of making the hand describe an arc of two inches, or thereabouts, without inducing rigidity; but the moment that the deflexion was carried a little farther, so as to stretch the muscles in some degree, the cataleptic contraction took place. This appeared to me a very interesting phenomenon, and I frequently repeated the experiment, with uniform results.

It was, probably, in consequence of our patient having been in a weak and emaciated state during most of her illness, that the degree of tonicity and rigidity manifested by the muscles, when cataleptically affected, was not so great as is met with in some cases. It was, for instance, easy to overcome the rigidity by ordinary muscular exertion on the part of the attendants, who could place the patient's body and limbs in any position they pleased. But it was, indeed, wonderful to see limbs so very much emaciated as these were latterly bear such weights and strains as they were exposed to. About the end of April, Dr Naismith found that the patient could balance her body on one foot, with the arm of the other side extended horizontally for five minutes before she showed symptoms of fatigue; further, that, while the arm was extended horizontally, a weight of ten pounds suspended at the wrist was borne without the position of the limb being changed, but that a little additional weight pulled it down gradually.[1] On more than one occasion, while the patient lay on her side, with the knees somewhat bent, I elevated both legs together until the knees and ankles were raised about eight inches above the sofa. In this very inconvenient position they remained for a considerable time, and bore a weight spread over the limb, from the knee to the ankle, of about twelve pounds, without giving way.

The strain upon the psoas muscles, caused by placing the body in a half-sitting posture, the legs being stretched on the sofa, seemed to cause a good deal of uneasiness, manifested by flushing of the face; but the position was maintained for some time. In health, such a posture cannot be endured for many minutes.

The eyelids were generally closed; if an attempt were made, with the finger, to raise the upper lid, a contraction of the *orbicularis* muscle was excited, by which the lids were spasmodically drawn together; a firmer application of the finger sufficed to overcome the effect of this contraction, and, without difficulty, the whole front of the eyeball could be brought into view; then the levators became cataleptically affected, and the eyelids were, by their action, kept *open* for a long time. The eye, so exposed, was almost always directed forwards; it had a vacant expression; neither bright light, nor the quick approach of a threatening body produced any change either in the direction of the axis or in the position of the eyelids; the movements of the iris were more sluggish than usual.

It was remarked that the cataleptic rigidity was greater in the afternoon than in the morning.

---

[1] In a similar experiment, reported by Heberden as having been made at St Thomas's Hospital, London, in 1764, the arm of a cataleptic woman "remained extended with a weight of *seven* pounds *in* the hand."

Repetition or protraction of the experiments on the peculiarities presented by the action of the muscles appeared to induce fatigue. When the patient was placed on her feet, and kept so for some minutes, she seemed ready to sink on the floor, if not held up by those about her; and it was latterly observable that, even while the patient lay on the sofa, long maintenance of a limb (the arm for instance) in a position requiring much muscular effort produced paleness of the countenance, and considerable acceleration of pulse.

2. *Quiescence.*—In the course of the six months during which the cataleptic condition existed, the patient exercised her voluntary muscles to an extremely limited extent. It was only for about an hour each morning that she moved about her room, and assisted in washing and dressing herself. This over, she lay on her sofa, or in bed, in absolute stillness. Not a muscle was moved, not a feature changed, excepting when she was subjected to some force from without; or, on very few occasions, to some mental stimulus. Her statue-like immobility was most remarkable. It struck the beholder with a kind of awe, especially when he found that, in no way, he could succeed in obtaining indications of either consciousness or attention. It has been well said that this strange state is "a fearful blending of the conditions of life and death; presenting, so to speak, life in the aspect of death, and death in that of life." It is especially with reference to this feature in the case of his "thunderstruck" patient that the same author remarks:—"I protest I cannot conceive anything more dreadful."[1]

Truly affecting was the sight of a fine young woman, known previously as highly intelligent, amiable, and accomplished, lying from day to day, and month to month, in absolute torpidity, presenting no obvious sign of life but that afforded by the most gentle breathing. After witnessing such a case, one can well understand the foundations in fact, on which, doubtless, are based some of those almost incredible stories of protracted sleeps, which one meets with in certain of the older authors. It was easy, also, to imagine that a still quieter state of the respiration and a feebler pulse might have led bystanders to look upon the patient as actually dead.

About five weeks before death, the morning activity ceased entirely, and the patient moved no more, excepting passively.

3. *Anæsthesia.*—It was imposible to determine positively, whether or not there was entire absence of the excitability of the sensory nerves, for, in the cataleptic state, no re-

---

[1] Diary of a Physician.

sponse (excepting in one particular) could be obtained from the patient. Certainly, no indication of its existence in any part of the body was given, excepting when an attempt was made to put anything into the mouth, and when electro-magnetic currents were passed through the limbs.

The surface of the cornea, the deeper parts of the nose, and of the auditory canal were touched, more or less rudely, without any proof of feeling being elicited. The fingers at the roots of the nails, and the skin in the most sensitive parts of the body, were pressed sharply, but no motion followed. Flies were seen to run over the lips, cheeks, and eyelids, and pins were thrust through the skin of the limbs and soles of the feet, without any shrinking on the part of the patient; and yet, whensoever, and by whomsoever, a spoon was conveyed to or made to approach the mouth, the muscles of the jaws and mouth contracted strongly, and resistance to the threatened intrusion of food was maintained to the end.

Romberg defines *anæsthesia* to be "a diminution or loss of the energy of a sensory nerve, from its excitability or condensing power being reduced or destroyed," (*On Diseases of the Nervous System*, i., p. 192). The same author recognises anæsthesia of the fifth pair of nerves, anæsthesia of the vagus, of the nerves of the eye, of the ear, of the nose, of the tongue, of the sympathetic ganglia and their nerves, of the spinal cord, and of the brain, as so many special modifications of this abnormal condition of the nervous system.

It appeared as if my patient laboured under almost all of these modifications combined. At all events, no manifestations of sensation or excitability followed the making of a loud noise close to the ear, the application of the vapour of strong hartshorn to the nostrils, or the putting of disagreeably-flavoured substances on the tongue; but when a bright light was thrown on the eye, the iris moved, although sluggishly.

The strong spasmodic action of the mouth and jaws, excited by attempts to give food, seemed to be owing to a centric cerebral affection, causing a morbid reluctance or antipathy to the reception of nourishment in any form. The degree of resistance offered seemed in no degree to depend upon the state or qualities of the food presented; solid and fluid, sweet and sour, produced the same effect; so that I had reason to believe that the gustatory nerves were almost as much affected with anæsthesia as were those of the other senses.

What has been stated regarding the effects produced by the touching of the lips with a spoon, or with any article of food, would seem to prove that the oral organs were exempted from the state of anæsthesia which was spread over other parts of the body; and yet it was observable that the lips were sometimes almost covered with flies, creeping even into the commissures and within the mouth, without the least movement on the part of the patient being excited by their presence.

It may be regarded as an interesting fact, that, although there was no muscular motion in the way of drawing back the head, or otherwise wincing under the application of strong hartshorn to the nostril, there was generally produced thereby a considerable flow of tears.

4. *Speechlessness.*—On the two occasions already mentioned, when my patient refused to speak for several hours consecutively, some weeks before she became cataleptic, I was satisfied that she had lost, not the ability, but the will, to speak. I believed her then to be affected with a peculiar form of hysteria, and treated her accordingly. When she resumed the use of her organs of speech, she appeared ashamed of her previous silence, and was unwilling to give any account of what had passed through her mind, excepting to the extent (as she wrote of herself at the time) that she felt very nervous. It seemed as if she were afraid to speak lest she should utter nonsense.

The speechlessness, which occurred synchronously with the first appearance of cataleptic symptoms, was ushered in by an assertion, on the part of the patient, of her determination not to speak to her medical attendant. But not only towards him, but towards all about her, she ever afterwards maintained complete silence, notwithstanding every effort used to induce her to speak. On several occasions, however, during sleep, she uttered words and sentences in an audible voice; and when the electro-magnetic apparatus was used pretty sharply, she suddenly called out "Stop, stop," with a bewildered expression of great alarm, so distressing to witness that her friends requested that the process should be discontinued. It was with a very feeble voice that, when she was very near death, she uttered the words "sofa," and "yes;" but even then she could not be induced to answer questions.

5. *Resistance to the introduction of Food.*—This formed a very prominent feature in the case. It was similar to what is frequently met with in mania and melancholia. It continued throughout the whole course of the malady, after the

accession of catalepsy. Coincident with the apparent insensibility of the nervous system generally, and of the special organs of sense in particular, there was exhibited the anomaly of the muscles of the lips and jaws being excited to violent contractions by the mere touching of the lips with a spoon or with any article of food, and even by the approaching of these towards her mouth. This action, conveying to the bystander the impression of the will of the patient having been roused to determined resistance to every effort to supply the body with necessary food, was continued as long as the feeding process was persevered in. Even after the whole frame had become extremely emaciated, and every indication of great debility existed, the jaws were so firmly closed as to make it difficult to insert a small wedge between the teeth.[1] The effort to accomplish this, preparatory to the administration of food, caused considerable flushing of the cheeks, and sometimes a flow of tears; but never were the arms raised to push away the hand of the attendant, nor was the head drawn back. At first, the patient frequently rejected the food introduced into the mouth, but latterly she swallowed all, although sometimes slowly. A few days before death, she seemed to lose the power of swallowing, but continued to clench the jaws, whenever an attempt to open the mouth was made.

Although food was so absolutely refused throughout the whole course of the catelepsy, digestion appeared to be carried on in a nearly normal manner; at all events, there was no indication of derangement of any of the chylopoietic viscera. The emaciation, which ultimately became extreme, commenced immediately after the patient began to decline nourishment, before the attendants had learned how to administer it in spite of opposition. The atrophy then begun could not be corrected by increased quantities of food, given subsequently. The pulse became more quick and feeble as the atrophy progressed; but, for a time, while a full supply of nutriment was introduced into the system, the pulse fell. The impulse and rhythm of heart were nearly normal; there was no blowing sound; the respiration was generally performed softly and slowly, as in deep sleep, excepting during attempts to give food, when it was more hurried. Until within a few days before death, the urine was abundant, and healthy; it was generally passed during the struggles induced by the feeding process. Defecation continued wonder-fully normal throughout a great part of the disease; during the last weeks, it was accomplished with extreme slowness. The catamenia appeared in the course of a very few days after the first cataleptic seizure, and continued as usual during that period; afterwards, they did not return.

---

[1] Heberden says of his patient, "Her mouth was closed, and I was unable by *any means* to open it."

Soon after the accession of catalepsy, a bed-sore formed just over the coccyx, and proved troublesome for a time; the necessary examination and dressing of this sore evoked no expression, either of pain or of any other distress. It cicatrized perfectly, and remained so during some weeks, but, subsequently, the ulceration returned.

During the latter stages of the malady, the *temperature* of the body generally was low; the extremities were cold when not supplied with heat externally.

Death took place by exhaustion, about six months after the cataleptic state manifested itself; little or no change occurred in the symptoms latterly. The whole frame was reduced to remarkably small bulk and weight; the pulse became a mere flutter, the respiration extremely slow, but still the muscular rigidity was produced as formerly, although more feebly, and the teeth were clenched whenever a spoon was brought near.

I regret to be obliged to add, that I could not obtain an opportunity of making a *post-mortem* examination.

I have not hitherto made any reference to the causes of this severe malady. In truth, they cannot, with certainty, be determined. We had several of the conditions of the nervous system that are usually regarded as predisposing to the disease; the patient was sensitive and impressible; capable of energetic exercise of her intellect, which was highly cultivated; her bodily frame, although healthy, was not robust. Several months before the first accession of nervous disease, she sustained a severe shock on the occasion of the suicide of a young friend, to whom she was much attached, and in whose company she had passed much of her time immediately before the fatal catastrophe. It was remarked by her friends that she seemed to recover from the apparent effects of this shock sooner than could have been expected; and they do not believe that it had any connection with the disorder which afterwards shewed itself. But I am disposed to look to that shock as the origin of the evil. It appears to me certain that painful impressions made on the moral feelings by startling events, such as that referred to, produce a disordered action of the nervous system, which, in many cases, does not manifest itself by external indications for a long time. Other moral causes may have existed in this case, and probably did, but of their nature and depth we know nothing with certainty. At all events, there was no discoverable ailment prior to the fever which ushered in the first stage of the disorder, (excepting a tendency to constipation);—no eruption, no worms, no uterine disturbance.

A general survey of the cases of catalepsy already on record, shews that the prominent predisposing causes of the disease are mental depression, anxiety, intense intellectual exertion, anger, terror, and the sudden reception of painful intelligence.

Treatment.—During the earlier stages of the case, there was, of course, good ground for apprehension that the patient would become either maniacal or melancholic; there were, however, such indications of *hysteria* as led to the hope that most of the symptoms depended upon that affection, and that, ultimately, the nervous system would be restored to its normal state. I felt the more hopeful of this favourable result, because I knew that the patient had originally a strong and well-cultivated intellect, and was possessed of sound moral principles, and a healthful play of the social affections; and, further, because I was flattered from time to time with appearances of considerable improvement under treatment.

I need not detail the treatment pursued prior to the cataleptic seizure; it was simple enough. Subsequently to that crisis, special attention was directed to the head, where both the existing symptoms and the results of investigations in like cases led us to believe that some abnormality of structure in the central organs of the nervous system existed. The scalp was shaven and blistered, sinapisms and frictions were used along the back: enemata of turpentine, asafœtida, and infusion of valerian were administered; while, by the mouth, were given valerianate of zinc with aloes, sulphate of iron with rhubarb, musk, quinine, cod-liver oil, sulphate of zinc, &c.; inunction with olive-oil was also tried, but late in the disease. The cold douche and hot pediluvia were not neglected. It was after the patient's return to Edinburgh that the electro-magnetic apparatus was used. The intensity of the electricity was increased gradually. The wires were placed in contact with various parts of the body. At first, nothing was observed beyond the ordinary muscular contractions, induced by the electrical discharges; but when, after a time, the intensity was augmented, and one wire placed over the epigastrium, while the other was applied to the inner surface of the leg below the knee, strong contractions of the abdominal and femoral muscles took place, the face flushed, the pulse rose much, the head and shoulders were raised from the pillow, and, with an expression of countenance and tone of voice indicative of great uneasiness, the patient called out "Stop, stop," and we did discontinue the operation. It was repeated two or three times within a few days, with the very same results, excepting that the exclamations were less strong. After the operations were over, the patient relapsed into her usual state of quiescence; the cataleptic rigidity was unchanged. Otherwise, it appeared to the patient's friends that she was rather injured by the electrical treatment. And, certainly, it was after this that the morning activity gradually lessened, and the emaciation increased. It was, therefore, deemed advisable not to persevere. It would seem that, in other cases, more or less resembling this one, electricity has been of more service than any other remedial means. Its earlier use, in my patient's case, might have been more useful. Carriage-exercise, and lying on a couch in the open air during a great part of the day, in fine weather, were persevered in for some time, while the patient resided in the country. On one or two occasions, Dr Naismith administered chloroform in full doses; but little change in the patient's state was observed at the time, yet she slept for some hours afterwards. It was observed that, in this sleep, the mouth was more open than usual.

In the treatment I was favoured with the counsel of my friends, Dr Begbie and Professor Simpson.

Remarks—The peculiarities presented by this case, especially the lengthened duration of the cataleptic symptoms, appeared to me to make it worthy of the time and space I have been led to devote to it. And to describe, so as to be understood, cases of such diseases of the nervous system, obviously requires some minuteness of detail. Even Abercrombie, whose taste and capacity for concise accounts of cases were so decided, after dwelling at great length upon the history of a cataleptic patient, which occupies three pages of his book on diseases of the brain, wrote thus:—"I make no apology to practical men for these long details, as the affections are of a very uncommon kind, and as *it is impossible to convey any idea of them by a general description.*" In the progress of psychological investigations, particularly with reference to the physical effects of moral agencies acting through the brain, it seems likely that such cases as the one now before us, when faithfully detailed, will come to be regarded with special interest as leading lights in a channel where great difficulty has hitherto been experienced by all who have attempted to thread it. Full histories of well-observed facts and symptoms will, perhaps, serve the purposes of the medical psychologist better than the most minute accounts of necroscopic appearances; they will be to him the best illustrations of mental pathology.

Certain it is, that, in the cases of this kind already on record, there is a very great variety, both in the kind and in the grouping of the symptoms, a variety which appears to have appalled systematic writers, both nosologists and physiologists, and which indicates the necessity of having a much larger accumulation of facts, before further attempts be made to draw inferences likely to be useful in practice.

One of our most laborious and well-informed physiological authors (Dr Carpenter) distinctly says:—"It is impossible, in the present state of our knowledge, to give any satisfactory account of catalepsy or of trance."—(*Principles*, p. 1103, 4th edit.) It follows that if our knowledge of and power to treat efficiently cases of this kind are to be increased, they must be more accurately observed, and more fully reported upon than heretofore. In this view, I trust the preceding narrative will not be found too long.

In one of the most masterly treatises on the subject which I have met with, the article *Catalepsie* in the *Dictionnaire de Médecine*, by Georget, special reference is made to the intimate relations which exist between hysteria, monomania, and catalepsy, and to the extreme difficulty of determining, in certain cases, in which these several affections exist together, which is the primary disease and which the accessory. Undoubtedly, the case just detailed presents this difficulty. The obstinate refusal of nourishment could only have originated, in such a case, in a monomaniacal perversion of the judgment and will; before the cataleptic symptoms appeared, there was certainly present some degree of hysteria; and throughout, while some of the symptoms indicated the existence of organic disease in the central organs of the nervous system, others gave the impression of the patient's will being much concerned in the production of them; that, in short, she feigned disease, and wished to impose upon us. Reduced to its simplest elements, I believe that this case was one of slight organic disease of the brain, the result of mental shock, and that, had the same occurred in a person of ordinary nervous temperament, it would have produced a common case of monomania. The peculiar sensitiveness and activity of the nervous system in my patient had, I presume, admitted of those singular phenomena, which have engaged our attention in the preceding pages, appearing as supplementary symptoms.

The persistence of the cataleptic state for so long a time without any complete remission, taken along with the predominant quiescence, were different from anything I find recorded in the histories of catalepsy which I have consulted; and yet, in other respects, the symptoms were just such as have been frequently met with in cases of catalepsy. Nosologically, I believe this case must be placed in the same category.[1]

Having referred to the apparent grounds for suspecting that some of the symptoms in this case were feigned, I must add a few words in defence of my own opinion that it was not so. Undoubtedly, catalepsy has been frequently feigned. Cullen asserted that he never saw a case that was not feigned. We all know what extraordinary pains are taken by hysterical females to excite sympathy, and to draw attention to themselves, what self-inflicted miseries and pains they will endure for the sake of securing notoriety. But in all such cases *the motive* becomes sooner or later apparent, and the imposture is discovered.

In my patient, her moral character supplied not the least ground for suspecting selfish motives or desire to deceive. The reality of her malady was proved by her unwillingness to take food, by the peculiar toleration of moderate motion of the limbs, while a greater degree excited rigidity, by the obvious anæsthesia, and by the persistence of the morbid symptoms to the very close of life.

ART. IV.—*On the Comparative Pathology of Post-partum Hemorrhage.* By JOHN BREMNER, Surgeon.

(Continued from vol. lxxx., p. 89.)

In resuming the consideration of post-partum hemorrhage from No. 196 of this Journal, I would direct attention to the human body as developed in all its physical perfection.

To every one possessing only a moderate acquaintanceship with the naturally harmonious and unrivalled mechanism of this noble structure, the reality will be quite apparent, that the existence of perfect health throughout the same can only be maintained so long as its balance or equilibrium-state remains unshaken. However soon, either as the effect of injury inflicted or disease spontaneously engendered, this shall experience an overthrow in any, even although a minute organ or portion, not only does the part so affected presently become the seat of disorganization, but the body in general is speedily more or less imbued with the morbid influence emanating therefrom.

---

[1] Consideration of the phenomena presented by the two conditions of the nervous system, known as trance (or catalepsy) and ecstasy, has led most modern nosologists to treat them as varieties of the same disease. Dr Copland and others have given definitions which, however, are not very satisfactory, inasmuch as they make a high degree of mental excitement and abstracted contemplation, presumed to exist in ecstasy and not in trance, the distinguishing characteristic. But the most that can be said is, that such mental excitement is not manifested *by language* in catalepsy as it is in ecstasy. With regard to the distinction between the hysteric and cataleptic paroxysms, Georget says that both affections are convulsive,—the spasms being clonic in the one, and tonic in the other.

Such being the appointed law in the economy of animated existence, the healthy operation of its functions must of necessity not only suffer a corresponding degree of interruption, but in numberless instances a total suppression ensues, too frequently under circumstances of aggravation, so great as to frustrate all attempts made for their removal.

If, in accordance with the proposition here laid down, the human frame be so liable to disorder from every trifling casualty met with in ordinary life, its susceptibility must be proportionably increased in the case of the impregnated female; and, consequently, a corresponding degree of vigilance rendered requisite on the part of those in attendance, both previous to and until the eventful process of parturition be conducted to its termination. By means of attention bestowed in this manner only can the numerous obstacles which, in the majority of instances, stand in the way of shortened and safe delivery, be effectually removed, and the system of the patient restored, as far as the instrumentality of means can accomplish to the state it enjoyed before the occurrence of impregnation.

It having been already remarked in the preceding part of the inquiry that the class of Mammalia and other animals enjoy well-nigh a total exemption from post-partum hemorrhage compared with women, the obligation then come under now devolves upon me, viz., of investigating the causes which seem chiefly concerned in effecting results so directly opposed to each other, as these are universally found to be. In order the more correctly to comprehend the nature of these, it must appear obvious that no other source exists from whence authentic information could be so fully derived as by instituting an analytical survey of the uteri and secundines in both species. The greatly enlarged size of the blood-vessels belonging to the organ in the human female has been already taken notice of, as well as the *natural* implantation of the placenta, and the advantages resulting therefrom.

With respect to the form and dimensions of this uniting medium—as every one understands—it presents the appearance of a roundish, flattened, spongy, undivided mass, in most instances averaging, in the healthy state, upwards of one inch in thickness, by six in diameter, enveloped in the respective uterine and fœtal tunics. Without entering upon any physiological disquisition on the subject, it may be implicitly assented to, that all other figures would have fallen short of the requisite adaptation to the peculiar exigencies in the case of women except that here described.

In turning from the human to the brute creation, it is to be observed that, with perhaps a casual reference, the only animals which shall be made the subjects of investigation are those with whose habits we possess the most intimate acquaintanceship, and whose value, at the same time, commands to them more than a cursory attention, as well previous to as during the period of parturition, viz., the cow and the mare. A survey of the gravid uterus in each of these towards the completion of gestation will be found to present an appearance in certain points widely different from the preceding. In the cow, instead of the compact and highly vascular substance as above detailed, a number of placentulæ or cotyledons, as they are usually termed, are met with, varying greatly in size, and interspersed throughout the extent of the membranous coverings at certain distances apart. In each of these there exists both a maternal and fœtal portion. A very remarkable form of organization is witnessed in the mare, viz., the entire internal uterine surface being lined over with the external fœtal chorion, into which the umbilical vessels are inserted.

With respect to the state of the sanguiferous system in these animals during the period of gestation, the most decisive evidence is afforded us that the mass of blood becomes increased and determined in every case to the uterus, through vessels also considerably enlarged, in a ratio commensurate with the development of their respective fœtuses, as in women. As corroborative of what has been advanced in the preceding paragraph, I have much pleasure in transcribing the testimony of Mr Barlow, the respected and learned assistant professor of the Edinburgh Veterinary College, in reply to communications addressed to him relative to the various topics connected with this department of the inquiry. First, as respects its superabundance:—"The amount of blood," writes this gentleman, "circulating in the system of the mother increases as gestation advances, this being required on account of fœtal development." Second, In summing up his account of the enlarged state of the bloodvessels, he says, "Indeed I may say shortly, that both arteries and veins connected with the gravid uterus of quadrupeds do materially increase in size as the fœtus increases; and this increase," he adds, "is far most obvious in animals yielding great quantities of milk." Upon a review of the organization of the different placentary masses here briefly submitted to analysis, it must be at once apparent, that upon their separation from the parietes of their respective uteri, the tendency to post-partum hemorrhage is much greater in women than quadruped animals, of which the cow and mare may be ranked amongst the chief in the domesticated state. In the former, on account of the magnitude and entire nature of the organ, separated from bloodvessels much enlarged beyond their usual dimensions, and over which no assured description of control can be exercised, independent of effectual and permanent uterine contraction, discharges of blood in the absence of preventive measures must be more or less liable to occur.

In the latter instance, however, a momentary consideration will suffice fully to demonstrate that in all natural cases unsurmountable barriers present themselves to the effusion of blood in almost any quantity upon parting asunder the connection betwixt the uterus and fœtal secundines. It has been already stated that the cotyledons in the cow are possessed of a maternal and fœtal portion. They generally range from two inches and upwards to half an inch in diameter; averaging in their undivided state three quarters of an inch in thickness, and gradually declining in proportion to size—the largest being placed towards the fundus of the uterus, and the more diminutive in the opposite direction. On account of the admirable arrangement and form of structure thus constantly witnessed, every avenue is blockaded, if we may so speak, against the occurrence of hemorrhage. "In consequence," as Mr B. justly observes, "of the maternal portion remaining behind, there is neither rupture of vessels or exposure of venous openings, as in woman." In the mare any such event must likewise be looked for in vain. The vessels on the internal uterine surface, and those ramified throughout the whole exterior of the attenuated fœtal chorion, appear only of similar dimensions to the capillaries of the conjunctiva when slightly inflamed, and are therefore incapable of giving exit to blood when their extremities are exposed.

Previous to having obtained a more intimate acquaintance with the anatomy of the gravid uterus in the inferior creation, and the experience of Mr B. to the contrary, I had been inclined to imagine that the absence of post-partum discharges of blood depended in a great measure on a more effective state of uterine contraction than was generally met with in the case of woman; and would consequently be found of more frequent occurrence amongst domesticated animals whose muscular system had become relaxed from confinement to the stall before the termination of gestation, than others roaming at greater freedom, or altogether in the wild and migratory state. Laying aside the fact, however, that in the cow at least, a period varying from eight or nine hours to several days is sometimes required for the discharge of the secundines, an inspection of the structure of each of these apart serves fully to dispel all doubt on the subject, and to assure us that no such casualty can take place in the healthy and complete state of the organs and their appendages. A question, however, of no trifling importance here presents itself for our consideration.

As post-partum hemorrhage is known from time to time to occur amongst quadruped animals, occasionally in such profusion as to prove destructive to life—What are the circumstances under which it has been found to have taken place? The experience and testimony of Mr B. on this department of the inquiry seem most decisive; he commences with stating, "I have not any recollection of a case of hemorrhage after delivery, wherein this (the delivery) was effected by the animal herself."

After making allusion to the difference of opinion entertained by different veterinary surgeons on this subject—which he has no hesitation whatever in ascribing to the imperfect manner in which post-mortem examinations are conducted, frequently to their entire neglect—he writes as follows:—"My own practice, which has been very extensive, goes to shew that in *every case* where post-partum hemorrhage in domestic animals has terminated fatally, and wherein careful post-mortem dissections have been made, a visible lesion, rent, or wound, has been found in the uterine surface. In no case do I remember fatal hemorrhage to have occurred without injury of the placenta, notwithstanding that almost every fatal case has been minutely examined into for many years past." In a succeeding part of his communication he again states:—"I have often known death follow, apparently as the result of this cause (placental hemorrhage), but on dissection have always found some rent, or lesion of surface, in which vascular trunks were involved;" and immediately adds, "I have seen cases, but not very numerous, in which hemorrhage took place after delivery, and was overcome by treatment. In these, however, almost entirely without exception, artificial mechanical means, and often unskilful violence, were used in extracting the fœtus." In reply to the query presented to him as to whether hemorrhage might be more commonly met with in very young than more aged animals, his language in substance is as has been already transcribed, and where in conclusion he further states, "However exhausted by parturient efforts, I never knew hemorrhage occur which could be supposed due to that state, and the attendant want of uterine contraction. A few ounces of blood are often lost at healthy parturition, and this comes entirely from the ruptured navel string, which you are aware we do not tie."

As proof of what he has here advanced, Mr B. alludes to the fact that, amongst cows, the womb frequently becomes everted soon after the birth of their calves, and prior to the expulsion of the membranes, &c. "Before the uterus can be returned," adds he, "the membranes require to be detached from the numerous cotyledonous surfaces. In effecting this, a gray or rather milky-looking fluid appears on the uterine and fœtal points of separation; but unless unskilful violence be used, and presuming all to be healthy, I never saw any blood." "In the mare," says he, "I never saw a case of post-partum hemorrhage, unless, as I stated regarding the cow, it depended on actual rending or lesion of surface, not due to separation of membranes." The same intelligent and scientific practitioner favours us with the results of his experience in a case of cæsarean section, in which he operated upon a bitch in the month of August last. "The animal," he states, "was small, but the puppies large, on account of her having connection with a large dog. She had been in labour for two days, and being much exhausted, the pains were entirely ceased. On laying open the uterus, a dead fœtus was found impacted in the os uteri and swollen vagina; four other fœtuses were extracted alive, enclosed in their membranes, which, of course, required to be detached from the surface of the uterus. It immediately collapsed, but did not contract. The mother was preserved alive by means of stimulants for about four hours; the puppies sucked. She remained in a state of collapse, but still there was no contraction nor hemorrhage. On dissection, the womb was flaccid and collapsed; but making allowance for what had been removed, there did not seem to be any contraction. I carefully examined the placental surfaces; there was not any hemorrhage." Several similar cases, he informs us, were attended with the like results. As still farther tending to develop the physiological history of the gravid uterus amongst inferior animals, compared with woman, it has been deemed proper, along with the lengthened extracts already presented to the profession from the pen of this gentleman, to add the following very important paragraph. In reply to the query, Had he ever met with or known of any case where the placenta was extended over the uterine orifice, he thus states :—" I never saw or heard of a case wherein the placenta was attached over the entrance of the womb in quadrupeds. In all the cases of parturition I have witnessed, hemorrhage before delivery has never been observed; and in the many cases of abortion in cows and mares which come under our notice, no discharge of blood ever occurs before the fœtus is expelled."

ART. V.—*On Poisoning with Chloride of Zinc, with Remarks on a lately-published Case thereof.* By THOMAS STRATTON, M.D. Edin.; Royal Navy.

During the last five months, circumstances having prevented my seeing the *Lancet* as published; it was not till last week that I observed in the number for 20th August 1853, an account of a case where, on the 6th November 1852, an adult took a draught of a strong solution of chloride of zinc, in mistake for Murray's Fluid Magnesia. As the narrator recommends a mode of treatment which I consider comparatively inefficient, I take the liberty of making a few remarks on this important subject.

The treatment employed was the administration of eggs and milk—the reader will perceive with but little benefit, although the narrator strangely observes that " the efficacy of prompt treatment is illustrated in a very striking manner." Whereas the patient had almost continuous vomiting for twenty-six hours; the mixture of eggs and chloride of zinc " being thrown off by the most violent action, returning in a solid coagulated mass, one portion of which was so large and so firm as almost to cause suffocation." Various painful symptoms presented themselves. A gradual improvement was made for the first two months; the patient then retrograded, was confined to bed for a fortnight, and then continued to have gnawing pain of the stomach. At the end of *six months*, " a gradual improvement has again taken place, but the natural hue of the skin has not quite returned; the taste and smell are quite impaired; the pain in the stomach is occasionally felt, and the general strength is impaired." The communication is dated April; it would have made the case more complete to have mentioned the patient's state, whether better, worse, or stationary, at a date later than April; the account is not published in the *Lancet* till the 20th of August, so that three months' later details might have been given.

Notwithstanding that the issue of the case is of the above unsatisfactory character, the narrator—after referring to a fatal case where the egg-treatment was used, and, although vomiting occurred, chloride of zinc was found after death in the tissues of the stomach—then proceeds to say:—

"From this consideration, then, fortified by the successful issue of the case, I would strongly urge the necessity not only of a copious but a long-continued supply of albuminous matters. I think it the more important to refer to this point, because the treatment of poisoning by chloride of zinc is still unsettled or misunderstood. In one of Dr Stratton's cases soap-suds were administered; and in the case referred to at the Metropolitan Free Hospital, the physician who had charge of it, in reporting the history, recommends that soap or carbonate of soda, or carbonate of potash, be given in order to throw down carbonate of zinc. This practice is doubtful. The carbonate of zinc, though insoluble, might still be injurious; certainly calomel, carbonate of lead, sulphate of baryta, and other substances, cannot be said to be inert."

Now, I think the reader will agree with me that it is difficult to see how the egg-treatment is recommended by the "consideration" stated above: eggs are given; after death, the poison is found in the stomach; therefore, in future cases, eggs ought to be given!

The narrator observes, "The treatment of cases of poisoning with chloride of zinc is still unsettled or misunderstood." Here I beg to differ from him as far as the publication, five years ago, of a successful case can settle anything. The case of James Curran, treated by me May 4, 1848, and published in the *Edinburgh Medical and Surgical Journal*, Oct. 1848 (also in Dr Hall's *British American Medical Journal*), was one where, on an empty stomach, certainly two hundred grains, perhaps four hundred grains, of solid chloride of zinc were taken in a solution which was mistaken for whisky. As carbonate of soda or carbonate of potash was not at hand, I treated him with a solution of soap. On the fourth day he had no complaint but weakness and impaired appetite. On the eleventh day his appetite, digestion, and strength were the same as before he took the chloride.

The narrator observes that carbonate of zinc "might" be injurious; as to chloride of zinc, no "might" is required.

The narrator remarks, that calomel, &c., although insoluble, cannot be said to be inert. To this I reply, that as a stomach containing eight or ten grains of calomel is in a totally different position from one containing as much corrosive sublimate (supposing no treatment to be employed in the latter case); so, in like degree, is a stomach containing a drachm of carbonate of zinc differently situated, as regards danger, from one containing as much chloride of zinc.

A further objection to the egg-treatment is, that the narrator describes the patient as being "nearly suffocated" by tough masses, so large as to be nearly unvomitable.

As some readers may feel interested in the matter, I beg to enumerate all the cases of poisoning with chloride of zinc which I have seen recorded in the periodicals.

Case First.—Elizabeth Richard, aged 17, a French-Canadian domestic servant, on 4th November 1847, at 61 Craig Street, Montreal, put to her lips a quart bottle and drank about a wine-glassful of a weak solution of chloride of zinc, mistaking it for whisky. I resided in the house, and had given the solution to another inmate for deodorising purposes. I have reason to think that she took twelve grains of chloride of zinc. She instantly felt pain and nausea, and another servant gave her milk to drink; she vomited freely. She felt indisposition and anorexia for three weeks after. She was not seen at the time by any medical man, as shame kept her from speaking of the occurrence till a month after, when I heard of it, and spoke to her about it, as did also Dr Winder. I spoke to her occasionally afterwards about her health, which continued good.

Case Second.—On May 4, 1848, at 163 Craig Street, Montreal, James Curran, aged 54, a robust, healthy man, employed as a messenger or porter, put to his lips a quart bottle containing a dense solution of chloride of zinc, mistaking it for whisky. A Montreal May is very hot, and he was also much heated with work; and it was at noon and before his dinner hour. I resided in the house, and it was in one of my rooms that he found the solution, which was not made by myself but supplied to me, so that I was not quite certain of its strength; however, I found that a drachm by measure of rain-water weighed fifty-seven grains, and a drachm by measure of the solution weighed eighty-eight grains. On using one of Beaumé's *Pese-Acides*, it fell in rain-water to zero, and stood at 55° in the solution. When this occurred I was not in the house, but was calling at a house in Hanover Street. A messenger immediately came for me, and I saw the patient about twenty minutes after he had taken the poison. He afterwards told me he supposed he had swallowed about a wine-glassful. If he swallowed two ounces of the solution, I consider that he took four hundred grains of solid chloride of zinc. As carbonate of soda or carbonate of potash was not at hand, I made him drink freely of a solution of soap. He made a rapid and perfect recovery. The particulars of the case are given in the *Edinburgh Medical and Surgical Journal*, October 1848.

These were the first recorded cases of this form of poisoning, and this was the first time that the proper antidote was clearly pointed out and employed.

Case Third.—In January, 1849, at Kensington, Mrs Collins, aged 40, took a mouthful of a solution of chloride of zinc in mistake for vinegar: carbonate of soda was administered; she recovered. The case is noticed by Dr Ogier Ward in *Medical Times*, 8th Nov. 1851.

Case Fourth.—In the *St James's Chronicle* of Sept. 1, 1849, it is mentioned that a woman, named Mary Robinson, had been committed for trial at the next Bury Assizes for the murder of her child by giving it the solution. The woman was acquitted.

Case Fifth.—On August 16, 1850, at Reddingfield in Suffolk, a child aged 15 months had some of the solution administered to it by its mother, who was poor and unmarried; it was not known whether the poison was given intentionally or in mistake. The lips and mouth were much corroded, which doubtless arose from the probable reluctance of the unfortunate child to swallow the liquid. Mr Miller saw the child an hour and a half after the poison had been given: eggs were administered; the child died in ten hours. The case is related by Mr Lethby in *Medico-Chirurgical Transactions*, vol. xxxiii.—*Edinburgh Medical and Surgical Journal*, January, 1851.

Case Sixth.—A male adult, John W., date not given, took a glass of the solution in mistake for gin: for three hours he received no aid, and was then admitted into one of the London hospitals, which he left in a week, and some time after came under the care of Dr Bushnan, at the Metropolitan Free Hospital, on May 27, 1851. He recovered.—*Medical Times*, 11th Oct. 1851.

Case Seventh.—In March 1852, a seaman in H.M.S. Arrogant at Portsmouth, took some of the solution in mistake for gin. It is said, in the *United Service Gazette*, 27th March

1852, that he was sent to the Naval Hospital at Haslar, where he died the same evening.

Case Eighth.—Is an instance occurring on 6th Nov. 1852, and referred to in this communication. In this case, as the first fæces passed were inodorous, this shews that the chloride had passed lower than the stomach. The gnawing pain of the stomach, which was not relieved by leeches or poultices, would probably, if not very severe, have been alleviated by a pad and belt; if more severe, by sinapisms or blisters. The writer speculates as to whether the constitutional symptoms were from the acid or the zinc: there does not seem to be any reason to accuse the acid. Among the symptoms, it is curious to observe that putrid and fæculent matters appeared inodorous to the patient.

In 1847, I had supplied to me a quantity of a dense solution of chloride of zinc (Sir Wm. Burnett's disinfecting fluid), to be used in the Emigrant Fever-Hospitals in Canada, and during 1847–8 I used or saw employed about 226 pints, making, when diluted, about sixty times this quantity for actual use. Being thus in the daily use of it, I had been but a short time so engaged, when, after having my hands wet with the solution, and then washing them with soap and water in the usual manner, I observed that the soap was decomposed; from this, it naturally occurred to me that in the event of any one swallowing, in mistake or otherwise, a quantity of the solution, that then carbonate of soda, or of potash or soap, would be the proper antidote.

It is worthy of notice that it is said the solution is at times made of an *impure* chloride of zinc; if this be the case, it ought to be borne in mind, in analyzing the constitutional symptoms, as *all* of these may not be due to chloride of zinc, some of them possibly being caused by the said impurities.

It is strange that a bottle of the undiluted solution has been mistaken for one of whisky or gin, as it is so much heavier. As usually met with, each bottle of the solution has pasted on it a paper with very full directions for use; it would be a good plan to add a couple of lines mentioning the antidote in case of accidents. The stomach-pump ought not to be used where this corrosive poison has been taken.

If the patient be seen without loss of time, and if the carbonate of soda treatment be freely used, I consider that the primary or corrosive effect of the poison will be successfully met, the whole of the dangerous chloride will be converted into a carbonate, and in most cases there will not be any of the chloride left in the stomach to be absorbed, and to produce its secondary or constitutional effects. Any young practitioner will perhaps allow me to remind him, that as the solution of chloride of zinc (Sir W. Burnett's disinfecting fluid), is in very extensive use in hospitals, and for domestic purposes, and on ship-board, it is a matter literally of *vital* importance that its antidote should be as generally known as the chloride itself.—(*Edinburgh Medical and Surgical Journal*, July, 1848; January, 1850; April, 1851.) In conclusion, I trust the author of *case eighth* will not attribute the foregoing observations to any want of courtesy on my part.

PRINCE EDWARD ISLAND,
Dec. 16, 1853.

ART. VI.—*On the Facts and Opinions recently recorded in regard to Asiatic Cholera.* By J. WARBURTON BEGBIE, M.D., Fellow of the Royal College of Physicians, lately Physician to the Cholera Hospital, Surgeon Square, Edinburgh.

Cholera, from which Great Britain had remained exempt for a period little more than three years, and which, during the latter half of that short time, had been distinctly recognized as again advancing towards us, made its reappearance, with almost unexampled malignity, at Newcastle-upon-Tyne,[1] on the 30th day of August 1853. Before the close of the following month Scotland was invaded; and with very varying severity, the cities of Edinburgh, Dundee, and Glasgow, with several country towns and districts, have already suffered from its ravages. Hitherto, in this its third, as in both former epidemics, many towns and places have enjoyed a merciful though unaccountable exemption from cholera; and notwithstanding our readiness to allow that the cities and parts of cities, the towns and rural sites, most obnoxious to the disease may frequently, if not generally, be indicated with tolerable accuracy; we have been caused to observe its appearance in places and under circumstances which render its occurrence in one locality equally inexplicable as the immunity which another has enjoyed. At the present time, there is an evident lull in its dreaded progress through Great Britain, a circumstance which our previous knowledge of the disease leads us to regard as the not unlikely precedent of its not distant appearance in a more extended, if not in a more fatal, form.[1]

---

[1] From the 1st September to the 4th November there died in Newcastle 1553 persons, which, in a population of 87,385, gives a percentage of mortality of 1 in 57.—(*Tables shewing the number of Deaths from Cholera in Newcastle in 1854.*)

The approach and anticipated appearance of cholera, scarcely less than its realized presence, have on this, as on former occasions, called forth numerous excellent, and many highly valuable, monographs and memoirs on all the various topics connected with the disease. Instead of subjecting these (even supposing that practicable) to a formal review or analysis, it is the object of the author of this paper, acting on a suggestion received from the editor of the Journal, to glean from them such observations as appear of interest and value, and to endeavour to lay these before the reader in the form of a connected narrative. Foremost, alike in accuracy of observation as in extent of research, among recent emanations from the British press on the subject of cholera, are the reports[2] of Drs Baly and Gull—the former on the cause and mode of diffusion, and the latter on the morbid anatomy and pathology of the disease—the combined reports constituting a work on cholera, upon whose appearance both authors merit our warmest commendation, and of which the learned and distinguished body to whom we are indebted for it, and under whose auspices it is published, have no reason to be ashamed. With like commendation it behoves us to speak of Mr Farr's[1] masterly report on the mortality of cholera in England in 1848–49, submitted to the Registrar-General. Of Dr Milroy's[3] report on the cholera in Jamaica we have also to express a very high opinion. Besides these, there are other reports on cholera to which we shall have frequently occasion to refer, more particularly that of the General Board of Health[5] and its appendices, which, though not published quite so recently, from their standard excellence, demand our consideration. We are specially desirous of directing attention to the above *reports*, because they are all eminently occupied with the presentation of

---

[1] "When epidemics of cholera have begun during the hot season, they have quickly risen to their climax, and have then soon declined and become extinct on the advance of the cold season; whereas, in the instances in which they have commenced at the close of the year, they have, as a rule, not caused any considerable mortality before the following spring or summer."—(*Dr Baly's Report*, p. 34.)

[2] Reports on Epidemic Cholera drawn up at the desire of the Cholera Committee of the Royal College of Physicians, by William Baly, M.D., and William W. Gull, M.D., members of the Committee. 8vo, London, 1854.

[3] Report on the Mortality of Cholera in England, 1848–49. London, 1852.

[4] Report on the Cholera in Jamaica, and on the General Sanitary Condition and Wants of the Island. London, 1853.

[5] Report of the General Board of Health on the Epidemic Cholera of 1848 and 1849. London, 1850. With Appendices A, B, and C, by Dr Sutherland, Mr Grainger (1850), and Dr Wynne. 1852.

facts, and of facts carefully noted and observed. Need we express our own conviction, that not until the accumulation of numerous facts, shall we be in a position to advance any probable or consistent theory as to the cause or mode of propagation of cholera, and certainly not less so as to its most efficient treatment. Of works on the nature and treatment of cholera we may here mention Dr Stevens,[1] which contains an extended account of the so-called saline treatment of the disease. We regret that so many personal and offensive remarks should have been allowed to creep into the book; shorn of these, its interest and value, instead of suffering, would have been increased; while, farther, we cannot help regarding the personalities which are introduced as adding most unnecessarily to the bulk of the volume. Of other works, from whose perusal we have derived pleasure and instruction, we may, in a word, allude to the following, by Mahony,[2] Barwell,[3] French,[4] Cormack,[5] Coghlan,[6] and Hearne,[7] and to the admirable, and now well-known papers and reviews by Drs Alison, Graves, Parkes, W. T. Gairdner, William Robertson, &c. &c. &c.

In the sequel we propose to consider the three following points in their order:—

1st, The diffusion and mode of propagation of cholera,

2d, The morbid anatomy and pathology of the disease.

3d, Its treatment.

1st, *On the diffusion and mode of propagation of cholera.*—This is a subject which may truly be said to have very largely excited the attention of medical men in all countries and at all times since its first appearance. Indeed, without much fear of contradiction we may assert, that if anything like the same amount of diligence and zeal had been directed to the inquiry as to the best means of combating the disease, as has been bestowed on the investigation of its manner of propagation, we should at the present time have our notions of the treatment of cholera fixed on a far more stable basis than even the most sanguine can claim for it. But while fully admitting the truth of this remark, we cannot subscribe to the feeling entertained by some, who appear to regard the whole time and energy which have been spent on the former inquiry as useless, seeing that, apart from any theoretical interest that attaches to the question, the determination of the manner of the propagation of cholera contains in itself much that directly bears on the question of its suitable and most trustworthy treatment. Let the contagious or non-contagious nature of cholera be once fairly determined, and the occasion for, or unnecessary institution of, quarantine, besides other points of scarcely less importance, become likewise settled.

---

[1] Observations on the Nature and the Treatment of the Asiatic Cholera, by William Stevens, M.D., D.C.L., Oxon. 8vo, London, 1853.

[2] A Pathological and Practical Treatise on Epidemic Cholera, by O'B. Mahony. London, 1853.

[3] Asiatic Cholera: its Symptoms, Pathology, and Treatment, by Richard Barwell. London, 1853.

[4] The Nature of Cholera investigated, by John George French, F.R.C.S. London, 1854.

[5] Notes on the Pathology and Treatment of Cholera, by John Rose Cormack, M.D. London, 1854.

[6] Practical Observations on the History, Nature, and Treatment of Cholera Asphyxia, by John Coghlan, M.D. Dublin, 1854.

[7] Thoughts on Cholera, by Edwin Hearne, M.D. London, 1853.

The different notions as to the importance of this inquiry are doubtless easily accounted for, when we consider the amazing diversity of opinion which prevails upon the subject itself. No better subject for medical discussion at present exists than the question of the mode of propagation of cholera; nor can we be surprised, from the very frequency of its discussion, and the little harmony that still prevails, that to very many others the subject has long ago become distasteful. "Amongst eighty-four communications relative to this topic," writes Dr Baly, "there are thirty-two in which the writers either distinctly maintain that the disease is not contagious or infectious, or evidently lean to the adoption of that view, and seven in which the contagious or infectious nature of the disease is asserted in an unqualified manner. On the other hand, in fifteen of the replies received by the committee, the writers advocate, or at least admit, the probability that cholera is propagated in more than one way; in twenty-four, no opinion is expressed, although in several of them the facts communicated are so stated as to shew that the writers would not altogether exclude infection of some kind from the modes of diffusion, yet clearly regard it as only of partial influence; and in six other papers it is simply stated that the disease is communicable under some circumstances, or that instances of infection have been observed by the writers.[1] In a recent discussion in the Medico-Chirurgical Society of Edinburgh,[2] of thirteen who took part, seven were contagionists, though to very various extents; four were decidedly non-contagionists, while two expressed no opinion on the point. The differing notions as to the degree and extent of the contagious power of cholera have no doubt also been the cause of many conflicting statements, as well as of seeming variance of opinion, which upon proper investigation was found not to exist. The strong opinions which have been advanced by some, it will shortly be our duty to denounce as untenable; but others more modified in form have been expressed, which are well entitled to our consideration, indeed force themselves upon our understanding. In entering upon an inquiry as to the contagious or non-contagious property of a disease, there are certain facts we are not unapt to forget, but which we do well to hold in remembrance; for example, the degree of contagious power possessed by one disease being lesser or greater than that possessed by another is readily comprehended—measles, scarlet-fever, and small-pox are certainly more contagious than typhus fever, though they cannot on that account be termed more truly contagious diseases. We must also acknowledge that at one time, and in certain places more than in others, a contagious disease may be more prone to manifest its contagious properties. Other opinions which have been, and are still entertained, we at the very outset do well to divest ourselves of. We cannot comprehend a view which has been advanced by those opposed to the notion of the contagious nature of cholera, as well as used by some who accept its power of contagion to a limited extent, that cholera may be, or really is, contagious in detail, that is, when attention is fixed upon an epidemic or outbreak of the disease which has extended only to a few cases; but in the mass it is not so. This argument is altogether fallacious, and is opposed to all the known laws of contagion.

---

[1] Report on Epidemic Cholera, p. 3.

[2] Monthly Journal, 1854.

We may here allude to another source of error in connection with the question of the propagation of cholera. Many have virtually prejudged it from the formerly universally prevalent idea, that in India all observers had agreed in considering cholera non-contagious. On this point our knowledge is now more accurate, chiefly owing to the extended and careful inquiries of Dr Copland, and to the publication of the observations of Scot, Parkes, &c., who have seen and treated the disease in India. This, however, is not to determine the question in the other way, while so careful and experienced an observer as Bell, and while, as we willingly concede, the majority of Indian practitioners, support the doctrine of non-contagion. Let it, however, be borne in mind, that not a few of these "admit the existence of facts, which would in this country be considered as strong presumptive evidence of contagion, and of opinions which bear to our minds the closest resemblance to a modified admission of it."[1] For the present we propose to inquire, if human intercourse does exert any influence in the spread of cholera. We shall, in the outset, record the following instances, or series of cases,[1] having reference to each of the three epidemics of cholera,[2] from which great Britain has now suffered, premising in the words of a highly gifted and esteemed physician,[3] that if the facts stated in them are, as we believe, carefully examined and truly represented, we consider them conclusive as to the fact of a certain contagious property being attached to the malignant cholera of 1832 and 1849, and we may add 1853. We think it of the greatest importance to impress upon *our* readers this presumption also, that in all the cases which will be cited to prove, and, as we think, to establish the contagious nature of cholera, along with those persons by whom the disease was acquired, there were, in all probability, very great numbers similarly situated in every particular, except the exposure to the particular cause, which, in the case of the former, originated the disease. What we desire to impress will be better borne out by a reference to a fact recorded by Dr Davy, in regard to another disease, whose contagious property, like that of cholera, admits of doubt; with Dr Alison's remarks upon it. A vessel, aboard which an undoubted case of plague had existed, being taken into quarantine at Constantinople, and two of the officers appointed for the purpose being sent aboard to examine the cargo, both these officers were seized within a few days with symptoms of the plague, of which one died in the lazaretto, the disease having been for a considerable time previously, and being thereafter unknown in Constantinople, *i. e.*, in a population of 800,000 souls. Now, adds

---

[1] Review, Monthly Journal, 1849.

---

[1] The question of the sole or principal means by which cholera is disseminated through a country, after its first introduction, is not to be decided by selected instances, says (Dr Baly, at page 156 of his Report), but by the results of an impartial inquiry into the origin of the disease in a large number of places indiscriminately. In regard to this observation of Dr Baly, we would only remark, that in our opinion, while probably the best means of arriving at a correct result, would be by the method he has indicated, still, while the field of accurate observations and detailed experience remains limited, we must be content to refer to selected, by which we mean the most carefully observed, instances.

[2] Properly speaking, we should content ourselves by a reference to what has been recorded in regard to the present and the last epidemic; but as it may serve to strengthen impressions, and add interest to the argument, we shall offer no farther apology for carrying our readers back, it may be on several occasions, to the epidemic of 1832–33.

[3] British and Foreign Medico-Chirurgical Review, January 1854.—Dr Alison, in his argument, has been directing particular attention to one consideration, which he thus states, "In these inquiries into the external causes of diseases, the number of individuals to whom our continued observations or experiments may easily be made to extend, is often very much greater than ever it can be in therapeutics; and, thereafter, if these observations have been carefully made and accurately recorded, they will very soon acquire, by the mere force of numbers, the full weight of the *instantiæ crucis*."

Dr Alison, when we attend to this fact, we think it must appear sufficiently obvious, that what makes it decisive is not the mere circumstance of the case of plague having occurred aboard that vessel, and succeeded to others previously occurring there, but it is the circumstance of its having occurred there *only* at that time, and for a considerable period before and after; and that when this is duly considered, it must be allowed to approach as nearly to the *instantiæ crucis*, *i. e.*, to an observation made on *many* persons placed in similar circumstances, *in every respect but one*, as can reasonably be desired. For without inquiring into the circumstances of the remaining population of Constantinople, we are quite safe in asserting, that among them there must have been great numbers, in all other respects, equally liable to plague as these officers of police; but we know that there was no other exposed to the influence of this particular cause—intercourse with the sick; the efficacy of which it is our object to ascertain—and finding that no other was affected by the disease, we infer that the efficacy of that particular cause is established by the true method of induction or exclusion. In all such cases, we have a large body of *negative evidence* statistically known, although often not formally stated, to support the *positive* evidence of individual cases following one another in a particular succession. When this negative evidence is clear and extensive, a very few positive facts become decisive as to the operation of an individual cause, because the cases in which all previous conditions, except that one, have been alike, being almost infinitely numerous, the influence of that one cause is soon brought to the *experimentum crucis*. But when such negative evidence does not exist, to furnish a comparison with the positive evidence, a great deal of labour may be expended on the statistics of a question of this kind without any practical result.[1]

But though convinced of the manner here indicated being thoroughly satisfactory, in all points, for the determination of the question, as to whether a disease really does or does not possess a contagious property; and entering upon an inquiry here with a view to the proper elucidation of this question as regards cholera, as one, in our opinion, of very great importance, we are not, even though we shall succeed in establishing the contagious nature of the disease, on that account approached much, if at all, nearer the explanatian of how cholera is first introduced into a country, or how its dissemination in a country it has already reached is effected. These important questions we for the present leave, believing that we shall best enter upon their consideration, when we shall have satisfactorily determined, as we shall now attempt to do by a reference to recorded facts, first of all, whether, under any circumstances, cholera is capable of being communicated from sick or infected persons to the sound. As the best means of inquiring into this subject, we propose, even at the risk of appearing tedious, to place before our readers examples in the way already mentioned.

---

[1] British and Foreign Medico-Chirurgical Review, January, 1854.

A.—Series of cases illustrative of the contagious nature of cholera, and having reference to the first epidemic of the disease in this country, 1832–33.

Importation of cholera into Bathgate, as given by Dr Simpson in his paper on this subject.[1] "In this village, which contains a population of nearly 3000 inhabitants, and is situated in West Lothian, 18 miles west of Edinburgh, on the great road between that city and Glasgow, six cases only of pestilential cholera occurred during the prevalence of the disease in Scotland,—four of them in persons who came to the town from infected districts, and the two others in the female villagers who acted as nurses to these strangers. There were *two* importations of the disease. *First*, a middle-aged female stranger was allowed, secretly, and contrary to the orders of the Local Board of Health, to enter one of the lodging-houses within the outskirts of the village on the evening of the 20th April 1832. This woman had, as it was afterwards ascertained, been for some time a residenter in Glasgow; but she had gone two or three days previously to Edinburgh (where cholera was at that time prevailing), in order to relieve some goods lying in a pawnbroker's shop. After having lodged in Edinburgh for either one or two nights, she set out again for Glasgow on the morning of that day on which she reached Bathgate. By the time she arrived at this latter place, she was *already* complaining of being unwell, having become indisposed upon the road; but her state was not such as to alarm the lodging-house keeper, or to prevent him admitting her. Having manifested, however, during the night well-marked symptoms of cholera, her condition was only, on the following morning, reported by Mr Dixon, surgeon to the Local Board of Health, who immediately adopted all the means necessary to prevent the spread of the disease, and sent a female, who had been eighteen months resident in the village, to attend her as a nurse. The stranger died on the night of the 22d April

---

[1] Edinburgh Medical and Surgical Journal, vol. xlix., 1838.

Next day the village nurse, after being engaged in washing the clothes of the deceased, was seized with all the most marked symptoms of malignant cholera; she recovered, however, after passing through a very severe attack of the disease. *Second.*—No other cases of cholera were seen in the village till the evening of the 27th of April, when the second importation of the disease took place in the persons of three female mendicants, who had the second or third day previously left Edinburgh, having been lodging there in Blackfriar's Wynd, where the disease was then prevailing. On the 25th, they were in the immediate vicinity of, and probably visited Newbridge, a small village at mid-distance between Edinburgh and Bathgate, and in which cholera was perhaps more prevalent and fatal, proportionally to the number of inhabitants, than in any other locality in Scotland. That night (25th) they slept at the farm-house of Niddry Mains, and while there either one or two of them complained of being unwell, so much so that they did not reach the village of Broxburn (which is only about a mile distant from the farm-house, and about two miles westward of Newbridge) till the afternoon or evening of the 26th. Mr Andrew Millar, the son of the farmer at Niddry Mains, was obliged, as I am informed, to come out of church on the following Sunday (the 29th) from sickness, followed by an attack of diarrhœa and some vomiting, which continued for about ten days. His father, who had been for some time in an infirm state of health, became suddenly much indisposed on the evening of the 4th May, with much sickness and sense of sinking; and when seen by Dr Thompson on the following morning, was evidently in the collapsed stage of cholera—and died that afternoon. The three mendicants took up their abode during the night of the 26th in a lodging-house at Broxburn, and endeavoured, as was afterwards confessed, to conceal the disease under which they were labouring. Next morning two of them presented such open and alarming symptoms of cholera, that all of them, however inhumanely, were forced out by the lodging-house keeper, and after lying on the public road for several hours, they were, by the local authorities of the place, forwarded in an open cart to Bathgate, a distance of seven miles. By the time they reached Bathgate they were both in the stage of collapse, and were immediately placed in a room in the village jail—a somewhat isolated building, situated in almost the very centre of the town. The elder stranger sunk in thirty hours after her arrival; and the younger one, her foster child, survived only for about twenty-four hours longer. Three female nurses, all natives and resident in Bathgate, were engaged to attend these cases—a duty in which they were assisted by a niece of the elder stranger, who had accompanied her. One of these town nurses was attacked with cholera on the evening of the 30th, the day on which the younger stranger died; and she sunk under the disease in the course of about eighteen hours. On the same day, also, the third remaining stranger, a girl of twenty, became affected with the disease, and died of the secondary head affection on the fourth day afterwards. Besides the four nurses, there were also necessarily freely exposed to the contagion in Bathgate, the landlord and his wife, to whom the lodging-house belonged; in which the first patient died and where the nurse was confined, as well as the four surgeons of the village (one of whom lived with the patients as a nurse); the clergyman of the parish, who was most assiduous in his attentions to the sick; and a man who frequently visited them and acted as porter to the hospital. None of these or of the other inhabitants became affected with the disease, with the exception of the two nurses, and perhaps, we may add, one of the surgeons who had an attack of tormina and diarrhœa. The villagers, in general, under a salutary dread of the disease, avoided as carefully as possible all intercourse whatever with the infected persons and houses." Such is Dr Simpson's detailed account of the introduction of cholera into, and its continuance in, Bathgate. We might easily add other accounts both from the extended investigations of the same and from other authors, though scarcely a more accurate or complete one; but for the present prefer addressing ourselves to the more recent histories which have been published of 1849, and during the present epidemic.

B.—In the Monthly Journal for June, 1849, in the course of a series of excellent and very valuable reports upon the subject, drawn up by different practitioners in Scotland at the desire of the then editors of the Journal, Dr Fleming and Dr Gairdner—there appeared the following from Dr Craig[1] of Kilwinning, in Ayrshire:—"Previous to Wednesday, the 3d of January last," writes Dr Craig, "I had not seen or heard of any case of cholera in this town and neighbourhood; but on that day visited, along with Dr Service, a lad sixteen years of age, *at the Eglinton iron-works*, having well-marked symptoms of cholera. Dr Service also stated that on the Sunday previous, he had been called to a girl about five years of age, who lived a few doors from this patient, and found her in a somewhat similar state, but not so well marked, and she had rallied considerably on Monday morning, and the pulse having returned he thought her out of danger. This girl, however, became worse gradually, and died on the Thursday with symptoms of oppressed brain. She had been brought from Carnbrae[1] two days before Dr Service saw her, and at the time she left, her friends stated that there was no case of cholera there; but I observed from the newspapers, that on the same night she was seized there were thirteen cases of cholera at Carnbrae, most of which terminated fatally. The next case we were called to was, on Thursday the 5th, the wife of a miner, and next day her husband was seized. Both of these persons, I afterwards ascertained, had been in company with the person first mentioned while affected with the disease. I ought to have mentioned that he had bowel-complaint for two days before we saw him, and had been vomiting and purging in the pit amongst his neighbour workmen, and was assisted here by them in a very exhausted state. These three cases having all rallied from the state of collapse, the impression became general that it was not cholera at all, and no little hostility was displayed against the doctors for saying so. On Saturday I found, however, that bowel-complaints were very general, but no application was made for medical advice, the people being determined to treat themselves. On Sunday evening, however, the calls for medicine became frequent, and soon after midnight I had several requests to go to the iron-works. On going down I found that Dr Service had been there before me. Several died in a few hours after we saw them. As early as possible I reported the state of matters to the Parochial Board, who despatched me to Dalry for additional medical aid. I brought Mr Hunter along with me, and together we went through every house belonging to the works, numbering from eighty to ninety. We found about fifty affected with the disease, twenty-seven of whom were completely collapsed. What I wish especially to note is, that by far the greater number of these were miners, who wrought in the same pit with the two men who were attacked the previous week. To follow the progress of the disease at the iron-works would be foreign to my present purpose, further than to observe in reference to a statement made by Mr Grainger, in proof of the non-contagious nature of the disease, that not a single visiter to the Tooting Asylum took it—that the visiters to the iron-works enjoyed no such immunity. I had occasion to engage a woman as sick-nurse for a destitute family; she went there on Sunday evening, took the disease on Wednesday, and died on Thursday. A man belonging to Kilwinning went out to look after his daughter who was ill; a week after he came home he took the malady and died. A woman came from Kilmarnock to attend some of her friends who were ill; she also took the disease. Two boys came from Barrhead, but when they came the disease had broke out, and they could not get work, and were afraid to go into any house; they crept under a furnace near to some of the houses where the disease was; in the morning one of them was found seized with cholera. From the violence of the attack at the works, the inhabitants were struck with terror; every one that could get a house anywhere else fled from it. One man, with his wife and mother-in-law, fled to the Kilbirnie miners' houses near Dalry, and immediately on his arrival he took cholera and died, and in less than a week the disease broke out there. Another man fled to Stevenston and took the disease; in a few days it broke out there. Another went to Harlford, near Kilmarnock, and took the malady, and immediately it broke out there. Are we, then, to look on these occurrences in the relation of cause and effect, or view them merely as coincidences?" The above is the account given by Dr Craig of the occurrence of cholera in the Eglinton iron-works. We could have wished, in regard to certain of the facts communicated, and the statements he has made, that a greater amount of precision had been adopted, as, for example, when speaking of the introduction of the disease into Harlford and Kilbirnie. The whole narrative is, however, most interesting; what follows more directly concerns our present inquiry, and to it we solicit special attention. From the Eglinton iron-works the disease, as Dr Craig believes, was introduced into the town of Kilwinning, and in regard to this introduction he has recorded the following facts:—"For more than two weeks after the appearance of cholera at the iron-works," continues Dr Craig, "we had not a single case here, although these are little more than a quarter of a mile from this place. The first case was that of a miner who wrought at the iron-works; —rather a severe case, but he ultimately recovered. His wife took the disease slightly; and his father-in-law, an old man, who visited him four days afterwards, took the disease and died. The next case which I shall select as an example, from amongst many, exhibits very strong evidence of contagion. It was that of the gravedigger; he lived in an isolated part of the town, and had no communication with the disease, except burying those who died at the iron-works. He had assisted at about thirty funerals when he was seized. He recovered, and as soon as he was able went out among his relations. His father, an old man, died of cholera. So did his sister-in-law; her husband (being the grave-digger's brother); and two of her children had the disease in a mild form. Two of the gravedigger's sisters, both married women, also took it; the one recovered, and the other is at present apparently dying from the consecutive fever. The husband of the latter, and a son of the former, also took the disease, but in a milder form. *All these persons live a considerable distance from each other, where there had been no cases of cholera previously; but all of them had had communication with the gravedigger, or with one another while labouring under the disease, before they took it themselves.*"

---

[1] Monthly Journal, 1849, p. 855.

---

[1] Of Carnbrae, the supposed point of introduction, in Dr Craig's opinion, of cholera into the Eglinton iron-works, Dr Sutherland writes,—"The village of Carnbrae is situated at a short distance from Coatbridge, and is inhabited by a population engaged in manufacturing iron. It was attacked by cholera on the night of the 31st December, 1848, and suffered more suddenly and more severely than almost any other locality with which I am acquainted." The population did not exceed 1200; between the 31st December and end of February, when the disease disappeared, there were 1100 attacked—the disease was extremely severe—94 deaths occurred, about 50 of which occurred at the first outbreak of the disease, or within the first ten days.—(Report to General Board of Health. Appendix A, p. 120.)

To the epidemic at Carnbrae we shall afterwards refer, when discussing the "House to House Visitation" in cholera.

The method of introduction of cholera into towns and places formed a subject of strict inquiry by the Committee of the London College of Physicians; and in regard to the results of the investigation we find Dr Baly stating:[1]—"The data comprise accounts of the origin of the epidemic in 119 places, of which sixty-nine are district towns or villages, fifteen are parts of towns (parishes or districts), thirty-four are public establishments (prisons, lunatic asylums, and workhouses), and the remaining one is a private house standing isolated in the country. Now, according to the information received by the committee, the disease appeared subsequently to the arrival of infected persons, or the introduction of other possible vehicles of infection in seventy-three instances out of the whole number, viz., in fifty-five out of the sixty-nine towns or villages, in four out of the ten parts of towns, in thirteen out of the thirty-three public institutions, and in the private country residence. Before, however, any conclusion is drawn from these numbers, the cases respecting which the evidence is least satisfactory must be separated. Thus, among the fifty-five cases of towns or villages in which the first patient had come from an infected place, had communicated with persons from an infected place, or had received clothes used by cholera patients elsewhere, there are twenty-four cases in which either the interval between the first imported case and the next succeeding cases was very long (above fourteen days), or sufficient details are not given, or lastly, conflicting evidence exists as to the facts; and six out of the seventeen cases of town districts or public institutions, being in like manner supported by insufficient evidence, there remain only forty-three instances of the apparent introduction of cholera by human intercourse, which can be referred to with any confidence. In like manner, among the cases of apparently independent origin of the epidemic, there are several in which the evidence is unsatisfactory. But these being deducted, there are still left *thirty-seven instances* in which the first sufferers appear to have been attacked independently of infection introduced from without by human means." In the Appendix to Dr Baly's report,[1] are given the particulars in abstract concerning the introduction of cholera into the thirty-seven places alluded to in the passage now quoted. From the Appendix we shall extract the facts which are stated in regard to the first appearance of the disease at Long Handborough and at Tichmarsh, near Thrapston.

---

[1] Report, p. 157.

1. Long Handborough,—reported at length, the names being given by Mr T. Allen, Mr Shurlock, and Mr Palmer—and thus condensed in Dr Baly's Appendix.

First case.—A woman from Shoe Lane, London, which she left on 13th August, in the morning, reached Long Handborough at 8½ P.M.—felt ill before arrival; immediately afterwards seized with vomiting and purging.—Died in sixteen hours.

Second and Third cases, on the 16th August.—One lived in house in which first patient died; the other lived in a house fifty yards off, but had nursed the first patient.—Recovered.

Fourth case.—The child of a neighbour who had been frequently in house of first and third patients on the 21st August.—Died.

Fifth case.—On the 26th August, the father of this child died.

Sixth case.—On the 28th August, the mother of the child died.

Seventh case.—On September 1st, another neighbour, who had communicated through the medium of his daughter, died.

Eighth case.—Also, on September 1st, a boy who lived with this daughter of the seventh case.

---

[1] *Vide* Appendix to Dr Baly's Report, p. 396, and following pages.

Ninth case.—The child of third patient, and niece of first, on September 4th; diarrhœa existed before.—Died.

Tenth case.—Another child of the third patient, on September 6th.

2. Tichmarsh, near Thrapston—reported by Mr H. Williams:—

1st, A butcher had been to Peterborough, and visited a house there infected with cholera, on the 3d October. His bowels became very loose on the morning of the 6th; he soon became collapsed and died the same night. His bowels had been out of order for a month previous, and any excitement invariably set them going.

2d, His mother was attacked on the 13th, and lingered to the 24th. His wife, two daughters, and son, were also ill with severe diarrhœa.

3rd, The woman, who nursed the first case, was attacked in his house on the 13th, moved to her own 400 or 500 yards off on 14th.—Recovered.

4th, This woman's daughter was attacked on the 17th. Mother removed her to a house half-a-mile off.—Recovered.

5th, The husband of the last patient was attacked on the 22d.

Nine cases (six fatal) occurred within a few doors of last patient, between 21st October and 15th November. The sister of case "5," who nursed both cases 4 and 5, was attacked on the 4th November, and died in twelve hours.

Two men living next door to first case were attacked on the 9th and 14th November. Both died.

A woman who nursed them was attacked on the 17th, and died in twelve hours.

Did our space permit, we would willingly transfer others of the interesting cases with which the Appendix is occupied.[1] We recommend them to the careful attention of our readers, as well as the most interesting account of the origin of cholera in Sunderland from the pen of a most accomplished physician, Dr Brown.[2]

C.—We pass on to consider some of the facts upon the subject, which have been gathered during the present epidemic of cholera. The following statement was made by Dr T. Traill of Arbroath to the Medico-Chirurgical Society of Edinburgh in December last, and appeared subsequently in the form of an Appendix to Dr Alison's paper in the British and Foreign Medico-Chirurgical Review.[3]

1. The first case occurred at Arbroath (a town 16 miles

---

[1] *Vide* Appendix to Dr Baly's Report, page 196, and following pages.

[2] *Vide* Do., p. 341.

[3] For January, 1854.

from Dundee) on October 29th, in the person of Mrs Menmuir, a stout healthy woman, aged 55, residing in an open, airy locality in the outskirts of the town, in a row of newly-built, one-story cottages, with small garden-plots behind. She had returned from Dundee[1] on the previous day, where she had gone to wait on some members of her family, four of whom had died of cholera within four days. She had remained in Dundee two days. Died in 24 hours.

2. John Menmuir, husband of No. 1., had also been in Dundee; attacked October 31st; lived 24 hours.

3. Mrs Christie, daughter of Nos. 1 and 2, residing in same row of cottages; waited on No. 1; *had not been in Dundee;* attacked October 21st; died in 30 hours.

4. James Forbes, residing in same street, nearly opposite to No. 1, had been repeatedly in house of No. 3, and assisted in taking care of her children; attacked November 1st; recovered.

5. Mr George Winton, medical student, residing in a part of the town half-a-mile distant from the Menmuirs; visited frequently Nos. 1, 2, and 3; attacked November 2d; died after 13 hours' illness; had not been out of Arbroath for weeks before.

6. Christie, son of No. 2, attacked Nov. 2; recovered.

7. William Menmuir, son of No. 1, attacked November 3; recovered.

8. Jane Christie, daughter of No. 3, attacked November 4; recovered.

Numbers 6, 7, and 8, were attacked in the House of Refuge, to which they had been removed on Nov. 2. This completes the history of the Menmuir family, originally numbering sixteen, of whom ten were attacked by cholera;--four in Dundee, all of whom died; six in Arbroath, of whom three died and three recovered; the seven deaths all occurring within eight days. All the other members had repeated attacks of diarrhœa and choleraic diarrhœa.

9. Harriet Coyle, aged 2; attacked Nov. 4; died. This child was brought from Dundee two weeks previously, and had suffered from diarrhœa during that period.

10. William Frazer, half-brother of No. 9; attacked Nov. 5; died same day.

11. David Skygie, aged 32; attacked Nov. 5; died in seven

---

[1] The first case of cholera in Dundee occurred on the 15th of October—the last case reported to the Board of Health occurred on the 2d of February, 1854. During that period there were 578 cases, and 339 deaths. From Dundee the disease spread apparently to the westward—at all events in Lochee, a village two miles west from Dundee, and in a rural district, four miles from Dundee, there occurred 46 cases—and of that number 25 died.

hours; remote from the other cases and not known to have had communication with any of them.

12. John Smith; remote locality; not known to have had any communication. Attacked Nov. 7; died in consecutive fever.

13. Mrs Hutchen; died in consecutive fever; nearly in same locality as No. 11, but not known to have had any communication.

14. Alexander Naidle; new locality; no communication known.

15. Mrs Hunter, nurse in House of Refuge; also washed clothes in house of No. 11; died within twenty-four hours, Nov. 10.

16. Mrs John Smith, wife of No. 12; Nov. 10.

17. Mrs Hood; nearly in same locality as Nos. 12 and 16, but no communication known; died in twenty-four hours, Nov. 11.

18. George Rodger; new locality; no communication known; Nov. 11, recovering.

19. Mrs Foggo, resident one and a-half mile out of town in a solitary cottage; went to Dundee on Saturday, remained there till the following Monday in her daughter's house in Dudhope Street (centre of cholera locality); returned on Monday, was attacked while at work on Tuesday, and died on Wednesday, Nov. 16.

20. Edward Hutchen, husband of No. 13; attacked Nov. 18; now in consecutive fever.

21. Anne Nash; new locality. Attacked Nov. 21; died next day.

No. of cases in 23 days, from Oct. 29 to Nov. 21, . .		21
Of whom have died	13	
Recovered	5	
Under treatment	3	— 21

Of these cases, four were probably imported from Dundee, two were more or less in communication with one or other of these four, seven were *not known* to have had communication with any case of cholera. Four of these were attacked after removal to the House of Refuge,—a large, thoroughly-ventilated, modern house, previously used as a hotel. All, or nearly all, the other inmates suffered from diarrhœa, and several from choleraic diarrhœa, two of which ought probably to have been reported as cases of cholera. Several of the nurses also suffered from diarrhœa; and one of them, No. 15, died.

Dr Alison regards this history furnished by Dr Traill as affording *decisive evidence* of the disease having a contagious property,—that is, of cholera being communicated by intercourse of the healthy with the sick; and he fixes attention on the following facts:—

"1. The two first persons affected in the town of Arbroath had just returned from Dundee, where they had visited persons suffering from cholera. The affection of these two persons of course proves nothing as to the contagious property of the disease, because it may be said that it was by inhabiting a 'tainted district,' not by intercourse with persons already sick, that they contracted the disease.

"2. The *six* next persons who took the disease in Arbroath, and the *six first* of the inhabitants of Arbroath who took it without having left the town, had repeated and close intercourse with the two first or with No. 3,—that is, with persons already sick with the disease before they took it.

"3. All these six took the disease in the course of the week, between the 29th of October, when the first case—that of Mrs Menmuir—occurred in Arbroath, and the 5th of November, when the case of David Skygie—the eighth in Arbroath who had not been in Dundee, and the first in Arbroath who had not had known intercourse with the sick—occurred.

"Here, it will be observed," continues Dr Alison, "that in order to prove that the disease spreads by intercourse of the healthy with the sick, we do not *only* rely on the fact of a succession of cases, to the number of six, occurring among those who have had such intercourse, but on the fact of its occurring among them only during a whole week, out of a population of more than 15,000, many of whom were undeniably more exposed to the action of every other external cause that has ever been assigned as influencing the disease, excepting only intercourse with the sick; and we maintain, that a simple calculation of chances is enough to shew that it could not have happened in any one succession of cases, if the circumstance of intercourse with the sick had not aided in determining the appearance."

The conclusions arrived at by the Cholera Committee of the College of Physicians on this point are thus given by Dr Baly:[1]—

"1. That in towns, villages, and institutions situated apart from infected places, the disease has, in a majority of the cases reported to the committee, seemed to owe its origin to the introduction of infection by human intercourse. 2. That in a moiety of the parts of towns in other parts of which the epidemic was already prevailing, it appeared to take its rise independently of infection by human agency. And, 3. That in a few instances it is satisfactorily shewn that the disease was propagated to isolated public establishments independently of the known admission of any probable vehicles of infection."

---

[1] Report, p. 164.

So much for the present in regard to the facts and the results of enlarged inquiry concerning the actual origin of cholera in towns and places as favouring the doctrine of contagion. As lending additional evidence in support of it, we must notice very shortly a few other points.

1*st, The liability of attendants to contract the disease.*— One of the most remarkable and most complete facts in connection with this subject is given by Dr Robertson[1] in his statistics of the Cholera Hospital in Surgeon Square, Edinburgh:—"Since the 28th of October," writes Dr Robertson, "18 nurses have been employed at different times in the Cholera Hospital; 5 of these have had severe attacks of cholera, and 3 died. A sixth, who for twelve hours had frequent vomiting and purging, was treated as a cholera patient, and recovered. Yet, at the same time, a very large number of nurses were employed in the neighbouring buildings of the Royal Infirmary, and escaped the disease. The circumstances, habits, and diet of both sets of nurses are the same; the work is not more severe in the Cholera Hospital than in the Infirmary; the arrangements for ventilation and cleanliness are not defective in Surgeon Square; *but for nearly two months past no cholera patients have been admitted into the Royal Infirmary.* In illustrating the contagious properties of typhus fever, we are accustomed to regard evidence of this sort as tolerably conclusive."

"In the Woolwich Hulks," writes Dr Baly,[2] "9 out of the 30 convicts who died had attended on the sick in the Hospital Ship; and two hired nurses were attacked, and one of them died. In the Wandsworth Workhouse, 3 out of 8 persons fatally attacked had been engaged in nursing other cholera patients. In these instances, the nurses who suffered, and the patients whom they attended, were for the most part persons previously in the same state of health, taking similar food, and living in the same circumstances. In regard to both of these instances," Dr Baly adds, "that they are more readily explicable by the theory of contagion than on any other principle." The following is the statement of Dr Peacock in regard to the persons affected in the Royal Free Hospital, after the arrival of the children from Tooting:—

---

[1] Monthly Journal, 1849.

[2] Report, p. 177.

"From 20 to 25 persons who had not been at Tooting had free intercourse with the children during the first fortnight; about 14 nurses and 5 male attendants sleeping in the wards with them every night. Twelve of these attendants were attacked with severe diarrhœa; four passed into the state of cholera, and two died. All the other attendants suffered in a less degree from diarrhœa. At the same time there were 50 or 60 persons in another part of the building, none of whom suffered from any similar form of affection. We shall conclude with the mention of two other instances. Into St Thomas's Hospital, London, there were received 147 persons labouring under cholera, 66 of whom died; and 9 persons were attacked in the hospital. Dr J. R. Bennett states, that all these persons were treated in special wards, which did not communicate with any others; that of the nine attacked in the hospital, six were patients admitted for other diseases, who could have had no cummunication, except accidentally, with the cholera patients; that there were nurses, of whom one served in a ward in a distant part of the hospital, but the others had been solely occupied with cholera patients; and that a fourth nurse, who had been for some weeks in the cholera wards, and had left because she was no longer wanted, returned in a day or two as a patient with cholera, and died.

At King's College Hospital, only 123 cases of reputed cholera were received, forty of which were fatal; yet two cholera nurses and two patients, who had been admitted for surgical complaints some time previously, and who lay in a ward adjoining and communicating with the cholera ward, were attacked and died; and a fifth person, a woman attending her child ill with cholera, was attacked, but recovered. All these cases occurred within the space of a fortnight, before the epidemic had reached its height, and when only thirty-six cholera patients had been admitted into the hospital. No further cases of the disease arose after the patients were placed in wards which did not communicate directly with any others.

2*d*, Regarding the communication of cholera by other means than the direct intercourse with the sound, our attention is arrested by *facts which attest the communicability of the disease to washerwomen and other persons employed in occupations of a similar nature*, by means of the bedding, and body-clothes of persons ill of cholera, or who may have died from it.[1]

---

[1] More of the evidence on this point, and its direct bearing on the question of the mode of diffusion and propagation of cholera, will be considered hereafter.

"The fact which I observed," says Dr Craig,[1] in his report from which we have already quoted, "was that persons who washed the clothes or bed-clothes of those who were affected, were particularly liable to take the disease. Four cases of this kind occur to me at this moment, and all of these proved fatal."

One of the earliest cases which occurred during the prevalence of cholera in Edinburgh last October, was the person who acted as cook and washerwoman in the Cholera Hospital in Surgeon Square. She died. This woman, says Dr Lauder Lindsay in his account of the case, had long been intemperate, and was of broken-down constitution. She had suffered from a severe attack of cholera in 1849. This was the only person in the Surgeon Square Hospital who contracted the disease on the last occasion; though, like the experience in the London Hospitals in the former epidemic, diarrhœa, frequently occurring and often very severe, was incident to both physicians, and to the day and night attendants.

"In order to learn," writes Dr Baly, "whether the actual experience of the medical profession confirmed the current belief in this matter, it was made the subject of one of the queries issued under the authority of the Cholera Committee. Eighty-four communications were received in relation to the modes of diffusion of cholera; in thirty-five, seeming instances of infection by means of clothes, linen, or bedding, were stated in general terms, or with more or less of detail; and in sixteen others the occurrence of such cases was either asserted or admitted, though the writers, in several instances, demurred to the inference generally drawn from the facts. In six communications it was stated that no such facts had been met with; and in eight others it was shewn that sometimes, under circumstances in which the communication of cholera in this manner, if possible, would be expected, no such result ensued. When the information thus obtained is further analysed, it is found to afford very scanty evidence of a satisfactory kind in favour of the theory of contagion.

Upon a candid and careful consideration of the facts which have now been advanced, we think that the *possibility* of the communication of cholera by means of human intercourse does not admit of doubt. Examples of the origin and progress of the disease, such as those cited at pages 510, 513, and 517 of this article, demand this explanation, and cannot in our opinion receive any other. The evidence which has recently been adduced, and the facts which have been recorded, either with the view of opposing this doctrine, or which, on examination, appear to afford it no countenance, we shall now proceed to examine; and we feel that so far from our path being rendered more difficult by the admission we have just made, it has in reality been freed from many otherwise insurmountable obstructions; for assuredly the more we read, hear, and see of this mysterious disease, the more satisfied do we become, that while human intercourse is undoubtedly a means, it is neither the only, nor in all probability that one, by which most frequently the diffusion of cholera is accomplished.

---

[1] Monthly Journal, 1849.

(*To be continued.*)

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *A Treatise on Diseases of the Heart.* By O. B. BELLINGHAM, M.D. Dublin, 1853. 8vo, pp. 252.

2. *The Diseases of the Heart and Aorta.* By WILLIAM STOKES, M.D., &c. &c. Dublin, 8vo, pp. 689.

Second notice, continued from page 376.

That portion of the work of Dr Stokes which next comes under review is, on the whole, less satisfactory than that which has been already considered, nor is it difficult to account for this. If we turn to any of the earlier works on pathology, or even to that admirable manual of the older school of pathological anatomy, for which we are indebted to Dr Craigie (*Elements of Pathological Anatomy*, 1848), we are struck by the meagreness of detail in regard to the morbid changes which muscular tissue may undergo;—for nearly all that we really know on the subject we are indebted to very recent researches, and by these the inquiry has, after all, been only but commenced. Not fully availing himself of all the aid which a study of these might have afforded him, Dr Stokes' chapters on the diseases of the muscular tissues of the heart can never be a full and sufficient guide for those who wish really to master the subject in its details; but exhibiting, as they do, the same practical character which we have already shewn to be the characteristic of his work, and valuable as the records of the individual experience of a close and pains-taking observer, they will be found adapted to the wants of the more numerous body of practitioners, who desiderate what they call guides in practice, and care not a great deal whether the guides they follow are very fully versed in those intricate questions which concern the theory of disease.

Dilatation of the Heart.—Great stress is laid upon the rarity of this affection in an uncomplicated form, and the complications with which it coexists are classified under three heads:—

1. Diseases of the orifices.
2. Obstruction in organs remote from the heart.
3. Debilitated state of the cardiac muscles.

We have been especially pleased with the prominence which Dr Stokes has given to the fact, that dilatation of the heart, in its most common form, is one of a triple group of local diseases, in which the heart, lungs, and liver appear to be affected. It has frequently occurred to us, in practice, to see a patient treated for one of those conditions, irrespective altogether of the other two; and we have at present under our care a gentleman exhibiting the combination in a well-marked form, who lately, from a temporary change of residence, passed into the hands of another practitioner, who pronounced our diagnosis of Heart Disease to be wrong—that the liver was the organ affected, and injured the patient by putting him most unnecessarily through active treatment, supposed to be adapted to the state of the liver. Its connection with the gouty diathesis is also noticed, and is interesting in relation to the valuable papers of Dr Begbie on Gout, which have appeared in this Journal.

Dr Stokes does not concur with Laennec, that the *distinctive sign of dilatation* is the clearness of sounds during the systole and diastole of the heart, but thinks, on the contrary, that the frequent combination of weakness with dilatation of the heart (this weakness depending on organic disease) gives rise to feebleness of the sound. Our author does not think we are yet in a condition to determine what special diagnostic sign is to be accepted as evidence of dilatation of the left auricle. When the left auricle is affected, we meet with dulness and diastolic pulsation, synchronous with that of the ventricles, simulating aneurism of the aorta. A similar action may be produced in a ventricle, when its contractile force is diminished and its capacity increased.

In discussing the causes of dilatation, we find little that is new. No allusion is made to the ingenious speculations of Dr Gairdner, who is of opinion that a certain class of cases of dilatation are occasioned by " collapse of the air vesicles of the lungs, connected with obstruction of the tubes"—a pathological state, the existence of which is not as yet universally admitted.

It is the opinion of Stokes, and we may add of all practical observers of disease, that,—

> " Considered practically, we gain but little from considering the subject of hypertrophy of the heart, when occurring independently of obstruction or dilatation of the valvular openings; nor has medicine been much advanced by our attempts to study the signs of the lesion, in this or that cavity; for though the signs of disease in either ventricle may be declared by *a priori* reasoning, we are taught by practical medicine, that hypertrophy, with or without dilatation, is rarely confined to a single cavity."—(P. 272.)

In connection with the subject of dilatation of the heart, Dr Stokes next directs attention to cases of "*Palpitation of the Heart, with enlargement of the thyroid gland and eyeballs*:"—

"The following are the important features of the disease:—

"1. Increased force and rapidity of the heart's action, without fever, and of long continuance.

"2. Excited action of the cerebral and thyroid arteries.

"3. Enlargement of the thyroid gland, varying with the force of the heart.

"4. Enlargement of the eyeballs, without any disease of the orbits or brain."—(P. 278.)

Of a morbid state so characterised, several notices occur in medical works. Flajani describes the disease of the circulation and enlargement of the thyroid gland (1800), as did also the late Caleb Parry (1825); but for the best account of these interesting cases, we are indebted to the Dublin school;[1] and for the best *rationale* of their production, and for the only observations on their curability, to Dr Begbie of Edinburgh.[2]

Since the publication of Dr Begbie's paper, Mr White Cooper and Mr Haynes Watson have published some interesting cases of the same affection (*Lancet*, 1849), and more recently Dr Romberg of Berlin has furnished some instructive cases, for a notice of which we beg to refer to our former number.

In all the earlier notices of this affection, it is described as consisting essentially in some disease of the heart, and it is to Dr Begbie that we are indebted for first pointing out that the increased force and rapidity of the heart's action, with the excited state of the cervical vessels, and the concomitant changes in the thyroid gland, are only links in a chain of morbid sequences, of which the point of departure is not, as Dr Stokes alleges, in the heart, but owns a wider origin in an anæmic condition of the whole system. We have ourselves seen two cases of the enlargement of the thyroid gland, along with the palpitation, but without the affection of the eye—in both clearly traceable to anæmia, The first case was that of young lady who long suffered from uterine polypus, with its constant drain on the system. The morbid growth had been removed before she came under our care, but the thyroid gland was enlarged, and increased remarkably in volume under any excitement; of the effect of which a somewhat irritable temper gave us ample opportunities of judging. This lady was also seen by Professor Simpson. The second is still under observation; in a low anæmic state from a perpetual drain from cancer of the womb, with a permanent, but by no means uniformly large, hypertrophy of the thyroid gland.

---

[1] Sir Henry Marsh, *Dublin Journal*, vol. xx., p. 471; Dr R. L. Macdonnell, *Ibid*, vol xxvii., p. 200; Mr Hill, *Ibid*, p. 399. Dr Graves' Clinical Lectures, ii., p. 193.

[2] Dr Begbie on Anæmia, *Monthly Journal*, 1849.

We cannot help noticing it as one of those curious coincidences which occasionally occur, that Dr Stokes, while to all appearance ignorant of Dr Begbie's valuable contributions to the study of this disease, has so far departed from the earlier doctrine of the Dublin School, as to cease to regard the cases as arising from organic disease of the heart, and he is disposed to look for its origin in a permanently excited action of the heart.

He says,—

> "The essence of the disease appears to consist in functional disturbance of the heart, which may be followed by organic change."—(P. 297.)

It is the peculiar merit of Dr Begbie's paper, however, to have shewn that even the "functional disturbance of the heart" is secondary to the anæmic state of the blood, and that all the unpleasant symptoms may be removed by a course of treatment calculated to rectify that condition. We cannot resist presenting our readers with Dr Begbie's admirable summary of the treatment of this and other forms of anæmia:—

> "Our first object in the treatment of anæmia, and its secondary disorders, is to ascertain the exciting cause; and having arrested or removed this, to improve the condition of the blood by the free use of the preparations of iron, and a liberal allowance of animal food, unless there are complications of such a kind as to contra-indicate their use. Wine is seldom necessary and generally hurtful, but porter or ale may be taken with advantage. A change of air, travelling, and passive exercise, also do good. Perseverance, however, is absolutely necessary both on the part of the patient, and his attendant. *Iron*, the chief means of cure, in order to be effectual, must be continued for months; and perhaps, after an interval, again and again renewed. It will not, however, disappoint our expectations; for whether it act by invigorating and improving the digestive and assimilative functions, or by repairing and augmenting the red particles of impoverished blood, there is really no remedy in the hands of the physician whose virtues have been so thoroughly tested, and so deservedly recommended to our confidence and trust."

While fully concurring in this panegyric on the virtues of iron, the general practitioner will have found in his experience, that there is a class of cases of anæmia of very frequent occurrence, where iron cannot be exhibited. These are the cases where the anæmic condition has been induced by menorrhagia, whether that be symptomatic of functional disorder or organic disease.

Our chief reliance, in such cases, must of course be on the means (mechanical or other) which will check the too profuse discharge; but it is well to know that the place of iron as a means of remedying the anæmia may be supplied by the salts of manganese (sulphate or phosphate), or by arsenic, as recommended by Mr Hunt.—(*Med. Chir. Trans.*, vol. xxi., p. 227.)

The chapter on *Fatty Degeneration* we regard as one of real value. In the discussion of this subject (true views of which are only commencing to be developed), Dr Stokes has departed from his usual practice, and has studied some of its more recent literature, at least the writings of Hasse, Rokitansky, Smith, Omerod, and Quain, though we miss all reference to those of Gulliver, Paget, Sibson, Barlow, and Handfield Jones,—all published before the date of the issue of Dr Stokes' work from the press. He also quotes many interesting cases and remarks from the *Dublin Journal*, which do not seem to have received the attention which they deserved from subsequent authors.

Fatty Degeneration of muscular fibre has been described by Haller and Vicq. d'Azyr; and Corvisart, in his work on Diseases of the Heart, describes the same change as having been observed by some of his cotemporaries. Laennec also described it under two varieties, which have since been adopted by Hope and Hasse;—the first being accumulation of fatty tissue upon the heart and between its fibres; the second, the conversion of its muscular substance into fat,—a true transformation.

To this division Dr Stokes adheres, saying,—

> "The researches of subsequent observers, including Rokitansky, have not established any case that may not be referred to either of his varieties."—(P. 302.)

A reference to the excellent description given in Mr Paget's lectures would have served to shew, that besides the two forms described so meagrely by Laennec, Hope, and Hasse, and so fully by Rokitansky, there is yet a third form, possessed of a much deeper pathological import.

Rokitansky indeed describes three varieties, not two, as Dr Stokes (copying the blunder of Hasse, for he does not refer to the original) records,—

1st, Fatty accumulation external to the heart.

2d, Atrophy of the muscular fibre, with penetration of a deposed tissue.

3d, The deposition of fine yellow granules *in* and *upon* the muscular substance.—(*Rokitansky, Sydenham Society Translation*, iv., 206.)

None of these varieties, however, recognize the existence of the oleaginous deposit, otherwise than external to the substance of the fibres. Mr Paget, however, drew attention to the fact, that the contractile fibre itself is frequently found to have disappeared, and to have given place to oil, appearing either as minute opaque molecules, or real small drops. The elongated nuclei are found either to have disappeared, or to be disappearing, to give place to this new formation. And with regard to the pathological importance of this form, it usually affects the whole heart, while the others are generally localized, leaving large tracts of sound fibre, sufficient to maintain the contractile power of the organ. Speaking of the former varieties, Dr Quain observes :—

> " In making a section it is sometimes possible to obtain a small diseased point, occupying two or three fibres, surrounded by tolerably healthy texture."

Such states may (might we not say, *must,*) acknowledge a limited or local cause; while that described by Mr Paget must be accounted for by some change more widely diffused. It is of importance to notice, that while the deposition of fat on or about the heart is usually found in persons of a corpulent habit, the second and third varieties of Rokitansky, and that described by Mr Paget, may co-exist with a very opposite condition.

While we have thought it right, as pathologists, to note these facts, we by no means intend to call in question the truth of the following observation of Dr Stokes :—

> " If, however, we look on this matter in a practical point of view, we find that the general symptoms and history, in both forms of the affection, are much the same. It is very probable, however, that the chance of rupture of the heart is greater in the second than in the first form."—(P. 303.)

And more probable still, we may add, when the fibres of the heart have undergone the change which Paget has described.

We pass on to the *Diagnosis of Fatty Heart.* Our former remarks on, and quotations from Dr Stokes, will have shewn our readers that he is not one to be over-confident in the powers of our diagnosis in any disease; and therefore it is peculiarly satisfactory to turn from those monographs, where any attempt to diagnose the disease during life on general or physical grounds, is held to be next to impossible, to such an assertion as the following :—

> " The diagnosis of this condition is not only possible, but often free from difficulty, at least where the disease is confirmed. On the other hand, minor degrees of the affection are to be determined, less by direct signs than by some general characters."—(P. 320.)

The *Diagnosis*, in Dr Stokes' opinion, turns on three points:—

> "1*st*, The existence of the physical signs and symptoms of diminished force of the heart.
>
> "2*d*, The occurrence of certain symptoms, principally referrible to the brain, which indicate, either anæmia on the arterial, or congestion on the venous side, of the cerebral circulation.
>
> "3*d*, Symptoms referrible to the respiratory function, which appear to arise from deficient power in the right ventricle."—(P. 321.)

In regard to the first class of symptoms, they (in common indeed with all the others) are given in the most meagre possible form by Hope and Walshe, and not very fully even by Dr Stokes. Dr Quain, who has applied the numerical method to the elucidation of the symptoms, thus classifies them:—

> "These effects may be briefly enumerated, as being,—languid and feeble circulation, a sense of uneasiness and oppression in the chest, embarrassment and distress in breathing, coma, syncope, angina pectoris, sudden death, rupture of the heart, &c.
>
> "Thus among fifteen cases, difficulty of breathing was present in nine; syncope or faintness in eight; giddiness and coma in five; pain in the region of the heart existed in six; and in four of these amounted to angina pectoris.
>
> "Of the fifteen cases, fourteen died suddenly; of which ten may be said to have died by syncope, three by rupture of the heart; one died by disease of other organs."—(*Med. Chir. Trans.*, xxxiii. p. 151.)

In speaking of the pulse, Dr Stokes observes:—

> "The pulse presents various characters; it may be large, soft, irregular, and slightly collapsing. It may be irregular and unequal, and intermitting; it may be permanently slow, or altogether disappear for a length of time before death."—(P. 335.)

A most remarkable group of affections, however, have struck the attention of all observers:

> "These consist," says Dr Stokes, "of repeated pseudo-apoplectic attacks, of various degrees of intensity and duration. They are seldom followed by paralysis. Attacks of vertigo, dimness of vision, and syncope are observed."—(P. 335.)

This kind of coma was noticed by Dr Adams (*Dublin Hosp. Reports*, vol. iv.), and also by Dr Quain (*loc. cit.* p. 155), who gives it the name of "*Syncope lethalis.*"

One remarkable case is recorded by Dr Quain, where the patient was to all appearance dead, neither pulse nor respiration being perceptible. He was restored, however, by electro-magnetism. He died in a similar fit two years afterwards, having had almost daily attacks of a slighter kind in the interval. Dr Quain is of opinion that "in this condition has lain the hidden and frequent cause of many sudden deaths." He points to "the paleness of the features, the unaltered state of the pupils, the absence of stupor, and the feeble action of the heart," as enabling us to distinguish these cases from apoplexy.

The symptoms affecting the respiratory organs are thus described by Dr Stokes:—

> "The respiratory symptoms are divisible into three classes:—
>
> "*a.* Attacks of dyspnœa on exertion, similar to those observed in other cardiac diseases.
>
> "*b.* Gradually increasing difficulty of breathing, amounting to orthopnœa, and coming on spontaneously.
>
> "*c.* A form of respiratory disease, peculiar to this affection, consisting of a period of apparently perfect apnœa, succeeded by feeble and short inspirations, which gradually increase in strength and depth until the respiratory act is carried to the highest pitch of which it seems capable, when the respirations, pursuing a descending scale, regularly diminish, until the commencement of another apnœal period. During the height of the paroxysm, the vesicular murmur becomes entirely puerile.—(P. 336.)

There can be no doubt that without affecting any superlative minuteness of diagnostic power, this affection may often be distinguished when we have either a slow, regular, or quicker irregular pulse, with the head and chest affections described; or a slow pulse, a feeble impulse, and a weak murmur with the first sound. In the last case, it is usually complicated with disease of the aortic valves. It is indeed this tendency to complication which so much increases the difficulty of distinguishing the various heart affections from one another.

We cannot surely speak with confidence of the diagnosis of this affection in its earlier manifestations, when we find the history of two remarkable cases occurring in persons in whom every symptom was likely to be carefully watched. We allude to the history of the deaths of Dr Abercrombie and Dr Chalmers, in both of whom this state of the heart, never recognised during life, was found to exist after death. For the full particulars of these cases, we refer to the interesting account furnished by Dr Begbie to the *Monthly Journal* (March 1851), and by Dr Hunter to this Journal (vol. lxiii., p. 227.)

In the former of the papers referred to, Dr Begbie shews that with the exception of a slight attack of paralysis which each had long before death, there was no evidence of cerebral congestion. Faintness and syncope were not experienced by either previous to the last fatal attack. Neither did either of these distinguished individuals experience any dyspnœa. In both the pulse was naturally soft and weak, and in neither was the heart itself examined,—a sure proof of the absence of any ground of suspicion.

The interesting series of chapters which we have now examined, is appropriately concluded by one on "The Treatment of the Organic Diseases of the Heart." Dr Stokes shews the impossibility of separating each disease of the heart, and dealing with it in an isolated form; at the same time, each of the affections must be separately considered in regard to treatment. Let any practitioner ask himself, what are the real principles which guide him in the actual management of a case of Heart Disease, no matter what the nature or seat of the lesion may be? With the exception of a few ardent young men, who practise by loose rule, or hospital physicians, or clinical teachers, who must, to a certain extent, practise a systematic treatment, which their common sense tells them carries imposture on its face—however accurate may be the diagnosis as to the nature, extent, and character of the cardiac lesion—this little avails them in practice. The great points we attend to are, the general state of the constitution, the amount of actual disturbance which the balance of the circulation has undergone, and complications which exist.

What remedies do we, or indeed can we employ, which have any immediate effect on the discovered lesion? Is not our whole treatment directed to the complications which exist? The practice of Dr Hope in hypertrophy, which generally rules modern treatment, was to reduce the circulation as far as this could be done, short of producing anæmia. Dr Corrigan, in a valuable communication to this Journal (vol. xxxvii.), was the first to shew that in many cases hypertrophy of the heart, so far from being a disease, "is a wise provision of nature, by which the organ is thus made equal to the increased labour it has to perform." This view is quoted and ably supported by Dr Stokes. Perhaps, however, there has of late years been a tendency to a too exclusively maintained use of it, though we by no means accuse our author of this fault; certain it is, that could the hypertrophy be restricted within such limits as would cause it to be no more than an exact compensation for the valvular disease, we would err in regarding it as a disease. But although such cases are not uncommonly met with in practice—cases in which the balance has been so exactly preserved for long years, as to prevent any suspicion of cardiac disease from being entertained—yet these cases are the exception, not the rule. The violent action of the heart might not of itself portend this; but what means the general constitutional disturbance?—a disturbance which the physician is often fortunate enough to allay by those means which tend to tranquillize the inordinate action.

Dr Stokes concurs with Dr Latham in the opinion he has so strongly expressed as to the incurability of hypertrophy, and earnestly warns us against any course of treatment that would weaken the heart, or depress the system at large. A process of reduction is a dangerous one to begin, and may involve the patient in the risk of the more formidable state of atrophy.

Dr Stokes thinks, at the same time, that a small number of leeches applied from time to time are of service, though even this treatment should not be pushed too far. Cupping is objected to from the excitement it occasions; and issues over the heart, from their directing the patient's attention to the disease; while setons are open to the further objection of their insertion over a heart acting with violence, being sometimes attended with dangerous hemorrhage.

The exclusion of all the foregoing means leaves little else than sedatives to be considered. Of these Dr Stokes gives the preference to digitalis and hydrocyanic acid. Like most practitioners who have tried it, he has found little benefit to result from the endermic use of the former as recommended by Bouillaud, and prefers giving from three to six drops of the tincture three times in the twenty-four hours. When digitalis creates nausea he has recourse to prussic acid—he gives one or two drops of the medicinal acid in distilled water three times a-day. Narcotics, he thinks, valuable when there is pain; but under the head of narcotics he includes some which we prefer to use as sedatives, and from which we have found great benefit, even where no pain existed, *e.g.*, Indian hemp, hyocyamus, chloroform. Digitalis is more likely to act beneficially when the tongue is clean and fever absent. Such are the remedies recommended for what is called simple dilatation, which are likely to succeed, as Dr Stokes observes, under the following conditions:—

> "1. The disease occurring in a young or middle-aged person of good habit of body.
>
> "2. A healthy state of digestive functions as shewn by cleanness of tongue, the colour of the skin, and the condition of the urine.
>
> "4. Absence of fever.
>
> "5. Absence of complication with the signs of valvular disease, especially of those which indicate regurgitation through the aortic valves."—(P. 349.)

In following Dr Stokes' chapter on treatment, we experience some difficulty from the absence of all statement as to the indications which he would recommend us to keep in view. For example, he commences the next branch of his subject (" *Treatment of the weak, and probably dilated heart, with enlargement of the liver and pulmonary disease*"), with a strong panegyric in favour of mercury.

> " I do not wish it," he observes, " to be believed that by mercury we can cure dilatation of the heart; but many years' experience has convinced me that, by the use of this remedy, we can delay its production, remove the irregular action which assists in causing the disease, and above all, prolong the patient's life, and again and again relieve him from dropsy, and from pulmonary and hepatic congestions, even when they have arrived at a point which threatens a speedy dissolution."—(P. 352.)

And again, after shewing that the quantity required varies much—that in some ptyalism is demanded, while in others the establishment of diuresis is sufficient—he continues :—

> "The truth is, in these cases, we are not to be over timid in the repetition of mercurial medicines, for there is nothing more remarkable than the power which the patients exhibit of bearing repeated courses of mercury, not only without injury, but with extraordinary benefit, to the general health. In some, indeed, the state of aggravation of symptoms appears to be kept off for an indefinite period by the continued use of small quantities of the medicine. The patients will improve in flesh, appetite, strength, and appearance. In others, as in a remarkable case which I have lately seen, the repeated use of very slight courses of mercury, at short intervals of time, has preserved the life of the patient for several years, and enabled him to pursue a laborious profession. This gentleman has now had not less than 30 distinct courses of mercury. It is truly his *pabulum vitæ*, and neither in this case nor in any of the others in which I have seen the treatment pursued, were the injurious effects of mercury ever produced."—(P. 356.)

Now this statement will not appear extravagant, either to the practitioner who has empirically used mercury as recommended, or to the one who has philosophically considered the nature of the disease and the means best calculated to effect its removal. Organic heart disease, we repeat it again, is usually incurable, but the sufferings it directly inflicts are comparatively trifling; it is the secondary lesions which create the chief distress, which threaten life, and against which our remedial means are of avail. The pulmonary affection, and the hepatic congestion, with which dilatation of the heart is so often conjoined, are both remarkably under the influence of mercury. But our experience in these affections leads us to believe that it is chiefly as a diuretic that mercury is of service. We have never needed to call in its aid when the ordinary diuretics could be got to act, and we have seen its most beneficial effects when it merely put the system under the power of diuretics. We would not in these cases wait for the actual appearance of dropsy, or for the diminution of urine, as reported by the patient. We have often been astonished, in critical cases especially, in persons advanced in life, with the great amount of discomfort which would follow a diminution in the secretion of the kidneys, of which the patient was not aware, and which only actual measurement would detect. The old combination of blue pill, digitalis, and squills, often produces a marvellous effect. In the liver disease, associated with dilatation of the heart, we have found more benefit from following the plan recommended by Dr Chevers—blue pill, with taraxacum, and the compound decoction of aloes, with temporary abstinence from animal and saccharine food, and from fermented liquors. We cannot, however, concur in his recommendation of abstinence from fluids. It was an ancient, and we think cruel practice, to withhold fluids from dropsical patients, and cold water is itself a good diuretic.

Dr Stokes recommends that the exhibition of mercury should be followed by that of saline and vegetable diuretics in combination.

> " *Treatment of Fatty Degeneration.*—We cannot say," observes Dr Stokes, "whether a heart once in a state of fatty degeneration can ever be restored to health; yet it does not seem impossible that such a result might be obtained if we had to deal with an uncomplicated case, and could recognise the disease at an early period."—(P. 356.)

But the truth is, as Dr Stokes proceeds to shew, fatty degeneration usually occurs in advanced life, or combined with disease of the arteries, or with gouty, strumous, or anæmic conditions, so that the chances are all against our art. It is to Dr H. Kennedy (*Dublin Medical Press*, Dec. 12, 1849), whose name is not mentioned by Dr Stokes, that we are indebted for first demonstrating the curability of fatty degeneration of the heart, and for laying down rational indications for its treatment. An account of the discussion which followed the reading of his paper in the Dublin Society shews that few of the speakers were then disposed to adopt his views, and that the system of graduated exercise which he first suggested is now first supported by such an authority as Dr Stokes. Early hours, and graduated muscular exercise, are strongly recommended, with the use of such a regimen as will tend to nourish without increasing the bulk of the system, avoiding milk, soups, or fluids. We have seen great advantage from following the plan recommended by Dr T. K. Chambers (*Lancet*, 1850, p. 128.) Fat, oil, butter, should be interdicted. Very light meals should be taken at a time most favourable to rapid digestion, and should consist of substances easy of solution and digestion. Liquids should be taken, not at the meal, but half an hour after. Dr Stokes, without giving any reason, objects to alkaline drinks. Dr Chambers again observes :—

> "There is one class of medicines so universally applicable to all cases, that I think a trial of them should never be omitted. The chemical affinity of alkalis for fat, point them out as appropriate alteratives in this complaint, and experience proves that they are suitable to the state of the digestive organs. The most eligible one is the liquor potassæ, and it may be administered in much larger quantities than any other. If given in milk and water, we may safely commence with half a drachm, and raise the doze to a drachm and a drachm and a-half three times a day."—(*Lancet*, 1850, p. 129.)

In the confirmed cases in persons advanced in life, where the pulse is slow, with a tendency to faintness, our great reliance must be placed on the free use of wine or brandy, says Dr Stokes; but in the paper of Dr Begbie, already referred to, we are furnished with a valuable caution in regard to this method of treatment. In speaking of the *treatment of the cerebral complication of fatty heart*, Dr Stokes is inclined to adopt the views of Dr Law (*Dublin Journal*, 1st series, vol. xvii.), and regard it as the result of a deficient arterial supply. He cautions against employing depletion where the heart's impulse is feeble, the pulse permanently slow, and when aortic murmur exists. Dry cupping, with diffusible stimulants and sinapisms, seem most likely to be useful.

Such is the value of this work, that we have been perhaps too anxious to place before our readers a complete synopsis of its contents. While so doing, however, time and space have insensibly melted away, and we find the necessity imposed on us of more rapidly referring to what remains. The following eighty-six pages are occupied by an amplification of the author's well-known and valuable researches on the *Condition of the Heart in Typhus Fever*. We shall not enter on the consideration of that subject at all. It would be impossible to do it justice without discussing at large Dr Stokes' views of fever generally—views which, we suspect, have been not a little modified since the writing of this chapter. We say since the writing, for we are inclined to suspect that a somewhat longer interval than usual has elapsed between the composition and the publication of many chapters in this volume. At some future period the subject of fever must be discussed, and then these views will fall to be more appropriately considered.

Displacements of the Heart forms the subject of the eighth chapter. These may be transverse or vertical. In a work on diseases of the heart, we need occupy our attention less with the causes of this derangement than with the effect which it has on the motions or the organic state of the heart. The diseases which produce displacement are divided by Dr Stokes into two classes,—*1st*, diseases of accumulation, or those which produce pressure from without; *2d*, diseases where the volume of the lung having become diminished, the heart is drawn to the affected side. Of the former class, the most notable diseases are those of distension either of the lung itself or of the pleural cavity, tumours in the thorax, and, lastly, "the existence of hernia through the diaphragm." Of this last very rare accident, Dr Stokes has recorded several examples. He has missed, however, one of the fullest and most interesting cases, which was observed by Dr Alexander Wood, and recorded by the late Professor Reid, first in the former series of this Journal (Jan. 1835), and subsequently in his collected volume (*Physiological Pathological, and Anatomical Researches*, page. 521.)

In reflecting on the theory of W. T. Gairdner, already referred to (and we confess the more we reflect on it the more truth-like and valuable it appears), it has often occurred to us whether it is not possible that some cases of lateral displacement of the heart, for the cause of which we have been at a loss, may not have been produced by the state which he has denominated "*bronchial collapse*," the "*état fœtal*" of Legendre and Bailly, the "*apneumatosis*" of Fuchs. Under the head of concentric displacements of the heart, Dr Stokes comprehends a class of cases where, the volume of the lung undergoing much diminution from any cause, the heart is drawn towards it.

The following, according to Dr Stokes :—

> "The conditions in which he has observed concentric displacement of the heart—1. Chronic tubercle of the lung; 2. chronic gangrene of the lung; 3. atrophy of the lung, with dilatation of the bronchial tubes, the cirrhosis of the lung of Dr Corrigan; 4. diminished volume of the right lung, consequent on the absorption of an empyema. Of these the last is by far the most frequent, and by it is to be explained many of the cases of the heart palpitating at the right side in persons who enjoy perfect health."—(P. 459.)

It is manifest, then, that the lateral dislocation of the heart is a simple mechanical effect of *any* cause which produces diminution in volume of a lung. Now it has been well shewn by Dr Gairdner that collapse of the lung has a tendency, when it becomes permanent, to produce a peculiar form of pulmonary atrophy (*On the Pathological Anatomy of Bronchitis*). And we have little doubt—though so far as we are aware the connection has not been traced out—that this atrophy may in its turn produce lateral displacement of the heart. Certain we are that we have seen cases, chiefly occurring in old persons, where we believed the patient to be suffering from bronchitic affection, and where, when lateral displacement of the heart took place, we were inclined to think our diagnosis had been incorrect, but in which we believe collapse may have occurred as a consequence of bronchitis, and produced the mechanical displacement.

Passing over a chapter on *Rupture of the Heart*, in which we find little that is new, and the omission of much that might with profit have been noticed, we arrive at chapter x., entitled "*Deranged Action of the Heart.*" In it the subject of "*Angina pectoris*" is first treated of. Dr Stokes pays a well-merited compliment to those chapters in Dr Latham's book which discuss this subject; for indeed the papers of Dr Heberden, the monograph of Parry (*An Inquiry into the Symptoms and Causes of the Syncope Anginosa, commonly called Angina Pectoris*, 1799), and the chapter of Dr Latham's work on diseases of the heart, contain all that is really known respecting it. There is certainly nothing new in Dr Stokes' view of it, though what is said is well said. It is stated by many authors—for example by Dr Copland—that angina may occur without disease of the heart. Dr Latham makes the same statement, though more doubtfully; but, as Dr Stokes observes, he "gives no case of the kind as occurring in his own large experience." His own (Dr S.) opinion is as follows:—

> "It is greatly to be doubted that angina pectoris has ever occurred in a patient perfectly free from organic disease of the heart and aorta; and it is more probable that in cases so described the disease was overlooked than that the heart was perfectly sound."—(P. 482.)

Our author, after Dr Parry, defines angina as follows, remarking at the same time, that an attack in its greatest simplicity and greatest intensity is of rare occurrence:—

> "A paroxysmal affection, attended with pain in the præcordial region, generally propagated in the course of the nerves of Wrisberg. pallor of the countenance, and sensation of impending death."—(P. 483.)

In several cases we have observed—what, as far as we know, has not been before noticed—that the paroxysm is often accompanied with severe pain in the back of the neck. The nerves of the leg are also frequently affected. Dr Stokes seems to confirm an opinion long ago expressed by Dr Unwins, "that there is scarcely any malformation of the heart or its bloodvessels that has not been found occasionally after death from what would be considered angina pectoris." At the same time, while fully admitting the variety of lesions found associated with it, he arrives at this practical conclusion:—

> "It appears certain that angina is rare in active hypertrophy. If we place the cases of weakened heart, from any cause, in one category, and those in which the muscular energy is either not deficient or in excess in the other, we find a much greater number of cases of angina in the first than in the second series."—(P. 485.)

Two views of the nature of the paroxysm of angina have long divided the schools. The first originally propounded by Heberden seems to have received the greatest measure of support, and has recently been most ably and eloquently expounded by Dr Latham, who attributes all the symptoms to spasm of the muscular fibres of the heart. Dr Stokes is quite opposed to this doctrine.

> "Little as is known of spasm of the heart, that little is opposed to the idea of angina pectoris being produced by it."—(P. 485.)

And arguing from the fact that it is seldom found without organic disease—that it is usually associated with some form of weakened heart—and that the evil effects of that weakened heart will be augmented by associated organic diseases, he comes to the conclusion of Parry,—

> "That the symptoms of angina arises from a temporary increase of weakness in an organ already weakened."—(P. 486.)

And again,—

> "That the special group of symptoms described as angina pectoris by Heberden, Parry, Percival, and Latham, is but the occurrence in a defined manner of some of the symptoms connected with a weakened heart. Obstruction of the coronary arteries may, or may not, be present, and is probably not unfrequent; but, as a cause of angina, its action is remote, and its existence unnecessary. It is only by causing atrophy with fatty degeneration, as Dr Quain has shewn, that it appears frequently to produce angina, a condition which might arise from similar states of the muscular structure, even though the arteries remained pervious."—(P. 487.)

Now we confess this doctrine does not satisfy us. When we consider the frequent occurrence of weakened heart without angina, the permanency of the organic change, and the intermittence of the symptom, and the absence on the part of either Dr Heberden or Dr Stokes of any attempt to shew the *modus operandi* of the weakened heart in producing the angina, we are driven to a third theory, which is supported by Jurine, Desportes, Laennec, and Copland, and not alluded to by Dr Stokes; or else to do what, after all, does not appear very difficult, to reconcile the two opposing views which alone are discussed by the author. The third theory, is that angina is a species of neuralgia of the pulmonary and cardiac nerves, affecting the functions of the heart and respiratory organs, and extending by nervous connection to other parts.

But we see no incompatibility between the fact to which Dr Stokes has so forcibly directed our attention, and the spasmodic doctrine of the disease;—the apparent difficulty of reconciling them arises from the absurd idea which is prevalent, that spasm of a muscle implies augmented strength of its fibre. If, however, there be any truth in the views so strongly advocated on the subject of spasmodic diseases by Dr Alexander Wood (*Monthly Journal*, Feb. 1853, and *Ranking's Abstract*, vol. xvii., p. 40), the conditions which Dr Stokes has shewn to be those most frequently associated with angina are precisely those most likely to cause spasm, viz., "diminished contractility, with augmented irritability."

Arguing by analogy from the views of Abercrombie regarding ileus, Dr Stokes seems more than inclined to propound a new theory:—

> "If a partial weakness of the heart was to occur—a condition much more likely to arise than a partial spasm, or augmentation of contractile force—we would have in the heart a mechanical condition not very dissimilar to that in the intestinal tube, when affected with ileus. If the left ventricle was the seat of the paralysis, or temporary weakness, while the right continued to act, obstruction and dilatation would equally follow; and if the loss of power in the left ventricle was complete, syncope, or it might be death, would be the result."—(P. 488.)

As regards treatment we do not find any novelty. Stimulants with opiates are the principal remedies, and in the interval the treatment adapted to weakened or fatty hearts.

> "Recent experience," says Dr Stokes, "seems to shew that in the case of fatty heart, there is danger from the inhalation of chloroform. It may be, however, that the exhibition of a draught containing ten or fifteen drops of chloroform might act beneficially."—(P. 490.)

We would warn practitioners against trying this experiment. We have tried the internal exhibition of chloroform extensively in several varieties of heart disease. We are quite convinced that in all cases of weakened heart it is a dangerous remedy; not many months ago we saw a patient brought into an alarming state of collapse by the exhibition of chloric ether, which is just chloroform dissolved in spirit. She was suffering from fatty degeneration of the heart.

When discussing *simple neuralgia of the heart,* Dr Stokes makes the following observation,—the only one on this subject which the space remaining at our disposal admits of our extracting:—

> "The occurrence of neuralgic pain in the arm and shoulder is a circumstance which should always excite attention, for it is commonly a symptom of some form of intra-thoracic disease. The pain, especiaially where it depends on cardiac disease, appears to follow the course of the nerves of Wrisberg, and may be propagated to the fingers. It may be paroxysmal, intermittent, or permanent, and it is often relieved by external anodyne, or stimulating applications, or, when it is intermittent, by bark. This circumstance frequently leads to a misapprehension of its nature, and in this way various important diseases, of which it was for a time almost the only manifest symptom, are often overlooked. We may thus see cases of aneurism, various forms of chronic disease of the heart, the early stages of tubercle of the lung, some forms of pericarditis, and, I think, cases of cancerous tumours within the thorax, in which this pain has been an early, though often misinterpreted symptom."—(P. 492.)

Dr Stokes is not disposed to undervalue the difficulties of distinguishing between organic and inorganic diseases, like some authors of manuals.

> "The facility of making a correct diagnosis between functional and organic diseases of the heart is not so great as modern writers would lead us to believe; and we more often arrive at a just conclusion by instinctive skill, the result of experience and judgment, than by communicable rules of diagnosis."—(P. 495.)

Diagnostically he divides the cases of nervous palpitation into two classes. In the one there is regular, in the other irregular, action of the heart. In either inorganic murmur may or may not be present. Its occurrence is more frequent, and its diagnosis more easy, in the first than in the second case. Dr Stokes very properly shews the fallacy of the distinction so much insisted on by Hope and others, taken from the acoustic character of the sounds. He says:—

> "Organic murmurs may be low and soft; and that inorganic murmurs are occasionally harsh, loud, or musical. The diagnosis is to be made by a careful consideration of the following points:—
>
> "1. The previous history of the patient.
>
> "2. The existing vital symptoms.
>
> "3. The duration of the murmur.

"4. The amount of accordance between the physical signs, supposing them indicative of organic disease, and the history, general state of the patient, and condition of the cavities of the heart, as far as this latter can be ascertained.

"5. The state of the heart as to general excitability is important. * * * * * Murmur, wherever it may be seated, co-existing with a tranquil and regular action of the heart, is more likely to proceed from organic disease than the opposite. * * * *

"6. The seat, or point of greatest intensity of the murmur must be, if possible, determined. * * * *

"We must be cautious in rejecting the opinion that inorganic murmurs may not be seated in the mitral orifice.

* * * * * * * * *

"Confining ourselves to physical signs, the diagnosis of anæmic murmur will often stand as follows:—

"1. A murmur, coincident with the first sound of the heart, generally louder at the left side than at the right side.

"2. A clear, second sound.

"3. Murmur in the course of the aorta.

"Adding other considerations, we arrive at the following rule, which is applicable in many cases, viz., that in a patient whose age, general appearance, and history, are opposed to the supposition of chronic organic disease, in whom the pulse has not the character of regurgitation, and the signs of hypertrophy of the left ventricle are absent; the existence of a single murmur with the first sound, generally predominating towards the base, and accompanied by an aortic murmur, while, at the same time, the second sound is clear, should make us conclude that the signs proceed from anæmia."—(Pp. 496-7.)

The truth is, as Dr Stokes goes on to shew, the triple combination of a murmur with the first sound—a clear second sound, and yet murmur in the aorta—does not warrant the diagnosis of a purely anæmic condition, as is too commonly supposed. These are precisely the changes which disease of the aortic valves induces, when, while causing irregularity of surface on the ventricular side, it yet permits the valves to close during the arterial systole. This state is often associated with fatty heart. Where it arises in a young person, however, it usually indicates anæmic murmur. The cases which have most puzzled us are, where the two states coexist. We are quite certain that we have more than once met with cases in practice where the anæmic state has been caused by, or at least has followed, the actual cardiac disease; the difficulty is to decide how much is due to the one, and how much to the other. The combination of organic and inorganic murmur sets all existing rules of diagnosis at defiance, and we require a much larger number of observations than we possess, before we can apply induction to them. The general conclusion to which Dr Stokes appears to come is, "That in the diagnosis between functional and organic derangements of the heart, we are to place our chief reliance on percussion, the extent of the impulse, the direction of the apex of the heart, and the relation between the force of the heart and that of the radial pulse."—(P. 528.)

With this we must conclude our quotations.

We do not presume to enter at all upon the last two chapters which discuss Aneurism of the thoracic and abdominal aorta; when the second volume of Dr Bellingham's work appears, we may take up that branch of the subject.

Nothing but the exceeding interest and value of Dr Stokes' book could have warranted us in dwelling so long upon it. To have reviewed it would have been presumption, and we trust the analytical survey which we have given will lead our readers to study it for themselves. The most sagacious practitioner will rise from its perusal strengthened and encouraged by its contents, and the youngest and most enthusiastic will find many lessons of caution which will temper the ardour of his earlier contests with disease.

It is a book which rises high above the selfish sphere of modern authorship, by which expectant practitioners seek to achieve notoriety. It is an honest book, and that is saying much in the present day. It not only tells us what we know, but points out to us those things whereof we are ignorant. It puts in no claim for its author of peculiar wisdom, or peculiar sagacity; and yet its transparent truthfulness renders these qualities everywhere conspicuous. Scarcely a single page can be read without communicating some information. How different, alas! from the usual crudities of our day, where the truth to be found is but as the drop to the ocean! You feel as you read it that you have to do with one who has manfully, though with labour and pain, fought his way by actual experience, through error and doubt, until something like sunshine illumined his path.

His has been no closet study, dealing with the dry bones of theories long since passed into decay,—for him the book of nature has opened her pages and imparted all her freshness to his own;—nor have the ample materials which he has thus collected been suffered to lie confusedly heaped. By his own master hand they have been worked up into the large results and wide generalisations with which his book abounds, the value of which will become more apparent the more they are studied.

In some points, indeed, it may, as indeed we have felt it our duty to shew, be somewhat behind the recent advances of pathological science, and may not be of itself a complete guide to the entire study of the class of diseases of which it treats.

But as a book of consultation to the practitioner, it is unsurpassed, and we greatly mistake if it will not ere long be found on the library table of every physician. For pages so instinct with the actual history of disease, we would willingly sacrifice some of the book learning, a parade of which is now deemed so essential to authorship.

In this work of Dr Stokes, we have a plain record of the daily experience of one who has observed much, and thought much on what he has observed, not drawn out before us with tedious minuteness and wearisome detail, but classified and arranged, and presented in its results; and we are much mistaken if the effect of its perusal on every ingenuous mind will not be to quicken observation and deepen thought.

Often have we lamented that the cares and weariness, the engrossments and the interruptions of large practice, should prevent those best able to instruct from making the attempt. Our author is a noble exception, and makes us regret the more that he has so few rivals.

May he long be spared to put in practice the rules he has so well laid down, and to add to those accumulated stores of experience, which he has so often shewn his readiness to render available to others.

ART. II.—*Transactions of the Medical Society of the State of New York, at its Semiannual Meeting, held in June* 1852, *at the City of New York, and at its Annual Meeting in the City of Albany, held February* 1853. Albany, 1853. Pp. 352.

To any one who attentively watches the progress of medical science, the fact becomes apparent, how very much it owes to a few individual and zealous cultivators, who give a tone and character to the labours of those who are engaged in the same field with themselves. From this it naturally, and indeed necessarily, follows, that the mental characteristics of those who have done most for its cultivation become impressed on the science itself, and give to it, at each successive stage of its advancement, those peculiarities which are handed down as the foundation of distinct schools of medicine. Look, for example, at the influence which Aristotle exercised over the progress of medicine for many centuries, his tendency to refined speculation leading him to indulge in hypotheses and theories, so that it is questionable whether these did not do more to retard its progress than all that he added in the way of new information did to advance it. Or to come down to much later times; how many of the doctrines by which medical opinion has been divided—whether those of dogmatists or empirics, humorists or methodics, chemists and mathematicians, solidists or vitalists—are to be traced to the peculiar bent or bias of those great intellects to whom it was given to form the mind of the age in which they lived?

In the same way, in the many valuable contributions to medicine which now reach us from the other side of the Atlantic, we cannot fail to be struck with the peculiar impress which medicine has received from the idiosyncracy of that remarkable race, of whose parentage Britain may well be proud. While it is a natural tendency among those born and educated, as we in this country are, to regard with peculiar veneration all that bears the stamp of antiquity, and not rudely or ruthlessly to run counter to opinions which our forefathers have cherished,—a young and vigorous people are not, as one of the writers in the work before us has remarked, "to attach any great importance to the opinion of even our shrewdest ancestry, nor to think more highly of a custom, because it was baptised by age and confirmed by a century." Another peculiarity which those who are familiar with the medical literature of America cannot fail to have remarked, is the thoroughly practical and business-like character of all they write.

In this country we seem, after having enjoyed a better order of things during the prevalence of the doctrines of the eclectic school, to be rapidly breaking up into the old parties of humoralists and solidists, according as the fancy of the pathologist leads him to addict himself most to the study of chemistry or anatomy, with its now popular sub-branch, histology. It seems all the more refreshing on this account to turn to such a volume as the one before us, which contains a chronicle of the opinions of some of the best physicians in the state of New York. It is divided into two parts, the latter consisting of an abstract of two meetings of the Medical Society of the state of New York, held, the one at New York in June 1852, the other at Albany in February of 1853. This occupies about fifty pages, while the previous part of the volume contains the papers which were read at these meetings, or at least such of them as a committee appointed for the purpose considered it advisable to publish.

The first of these papers, by Dr Franklin Tuthill, extending to 26 pages, is devoted to a consideration of the best mode of registering births, deaths, and marriages. In this paper we are furnished with an elaborate account of the keeping of public registers, among all nations, from the earliest times; from this the author passes to a consideration of the state of the law as it exists in America. It would appear that the full value of medical statistics is scarcely appreciated in America, and many object to the system altogether. Some are to be found grounding their objections on the want of accuracy of the existing system; others being of opinion, that all legislation of this class is impracticable; a third class regard the process as a too costly luxury; while there are yet a fourth found to aver, "that the most perfect system in the most successful operation, would afford no substantial service to the world, and add nothing practically useful to the amount of men's knowledge." Having enumerated these objections, Dr Tuthill proceeds to state some of the advantages which a good system affords, and to show the causes of the errors of the present system. He first objects to the Carlisle papers being used as the basis of American calculation, which, to use his homely but forcible language, is "like cutting Jonathan's coat by his uncle's measure." He extols the system of registration introduced into England in 1838, and suggests its adoption in America, but without proposing any material alteration in its present cumbersome and most inaccurate plan.

The improvements in this system suggested by Dr Alison, or the pathological system of Dr Gairdner, do not seem to have attracted Dr Tuthill's attention.

The next paper is a case of ligature of the subclavian artery, by Dr W. H. Van Buren. A rapidly increasing false aneurism, extending from the arm to the axilla, resulting from a knife wound, was the cause of the operation, of which the history possesses no peculiar novelty. At the close of the communication, however, there is an interesting digest of the cases in which the operation has been performed. The whole number from which Dr Van Buren founds his data is 101, including operations on each of the three divisions of the artery, and on both sides of the body. The general results may be condensed as follows:

Operation performed on the artery within the scalene muscles, seven times on right, and once on left side. From those on right side deduct one (Dr Cole's case), who died of wound in the pleura; and one (Mr Partridge's), who died of inflammation; and the remaining five all died of secondary hemorrhage from the 12th to the 36th day. The only case where the operation was attempted on the left side (Dr Rodger's) died from secondary hemorrhage on the 15th day. The subclavian has been thrice tied in the middle part of its course by cutting across the scalenus anticus muscle; by Dupuytren, in 1819, successfully; by Auchincloss, in Scotland, in 1833, but his patient died comatose in 68 hours; and lately by J. C. Warren, in America in 1843, and again with success.

The axillary artery below the clavicle has been four times tied—and with but one fatal result—by Keate, an English army surgeon, in 1800; by Blasius, in 1813; by Chamberlaine, in 1815; by Catanozo, in Sicily, in 1835.

In the external part of its course, beyond the scalene muscles, the operation has been more frequently and more successfully performed.

To save valuable space, we shall endeavour to tabulate the results recorded by Dr Van Buren.

I. *Comparative mortality of operation on the different portions of the Artery :—*

	Within Scalene Muscles.	Between the Scalene Muscles.	Below the Clavicle.	On external portion.
No. of times performed	8	3	3	86
Deaths	8	1	1	33

II. *Causes for operation on external portion, with results :—*

Causes for which performed.	No. of Times	Recoveries.	Deaths.
True aneurism . .	38	25	13
Traumatic aneurism .	12	9	3
Wound of axillary artery .	14	7	7
Distal operations . .	5	2	3
Mistaken disease . .	3	1	2
Disease not stated (aneurism either true or traumatic)	14		

III. *Causes of death in fatal cases :—*

Secondary hemorrhage	12
Gangrene	6
Pleuritis or pneumonia	4

Exhaustion from suppuration	3
Irritation from sloughing	1
Hospital gangrene	1
Dyspepsia (cause not stated)	1
Irritation of phrenic nerve	1
Exhaustion from repeated hemorrhage	1
Encephaloid tumour of axilla	1
Cause not stated	3
Total	33

The third paper in the volume is entitled "*Statistics of the Deaf and Dumb*," *by Harvey P. Peat, LL.D., President of the New York Institution for the Instruction of the Deaf and Dumb*. "This paper has for its object—*first*, to present some calculations respecting the numbers of the deaf and dumb, and some considerations on the causes of deafness; *second*, to say a few words on the pretensions to cure this infirmity; and *lastly*, to speak of the efforts to ameliorate the condition of the deaf and dumb by education."

A very brief notice of the two first topics is all that is suitable in a medical review. Dr Peat commences his consideration of the first head, by shewing "that the number of deaf mutes in a given country does not vary greatly from a certain proportion to the population of that country;" and the general conclusion which he arrives at is, that, a few extreme cases excepted, there is no country inhabited by Europeans or their descendants in which, in a population of a million, there are less than 350 deaf mutes. In Prussia, which seems to represent nearly the mean proportion, according to the census of 1828, there are about one to every 1,550 souls. Of the extreme cases, the most remarkable are presented by certain districts of Switzerland and the adjoining Duchy of Baden. The Swiss canton of Berne contained, in 1836, no fewer than 1,954 deaf mutes in a population of 401,000, or nearly one deaf mute in every 200 souls. In that country, deaf-dumbness seems often to be connected with or complicated by the greater infirmity of cretinism.

A few hints are given which may guide future inquiries as to the question of the liability of different races to this infirmity. Warm countries seem to contain, on the whole, a smaller proportion of deaf mutes than cold countries, but the difference is not great, nor very uniform.

Dr Peat is of opinion that there are "certain periods when deafness becomes to a small degree epidemic in certain districts."

An attempt is made to ascertain the proportion between cases of congenital and of accidental deafness. Out of 1060 deaf mutes who have been pupils of the asylum at Hartford, the infirmity was congenital in 537 cases, the result of disease or accident in 469, and in 54 cases the cause was unknown. Dr Peat is of opinion that in Europe cases of accidental deaf-dumbness are less common than in America. An attempt is made to ascertain the diseases to which the loss of hearing is attributable. The statements on this point, however, being often obtained from ignorant persons, cannot be relied on, though it appears probable that "scarlet and spotted fever, ulceration (query, of the inner ear?), and inflammation of the head, are far the most frequent causes of loss of hearing among children."

In discussing the second head, Dr Peat shews how this affliction has ever been a fruitful source of quackery, and makes the following statement:—"Dr Itard, the late able and most distinguished physician of the institution for the deaf and dumb in Paris, who perhaps made more *post mortem* examinations to ascertain the immediate causes of deafness than any other man, believed that, in most cases of profound deafness, the cause was paralysis of the auditory nerve. In other words, in most deaf mutes the nerves of hearing are dead, and medical means have no effect on the dead." We suspect our author's very commendable zeal against quackery has in this paragraph carried him a little too far. At all events, we remember when a quack aurist of some notoriety visited Edinburgh and Glasgow some years ago, Mr Anderson, the intelligent superintendent of the Glasgow asylum, was at some pains to shew (and exposed himself to no small risk by doing so) that in cases of deaf-dumbness, the hearing was seldom *entirely* gone, and that, acting on a knowledge of this fact, the empiric got the credit for some apparently astonishing cures.

The methods of educating the deaf and dumb are scarcely admissible subjects of discussion in this Journal, and we therefore pass without farther comment to a communication *on the use of chloroform in midwifery, by George N. Burrell, M.D., one of the physicians of the Buffalo Hospital.*

Dr Burrell derives his experience from the results of 180 cases, but inasmuch as the conclusions at which he has arrived are generally received as established in this country, we need not enumerate them here.

PAPER V. entitled "*Observations on the Pathology of Bright's Disease,*" *by John Agwett, M.D.*, attempts to establish the following conclusions:—

1st, That the different varieties of what is called Bright's Disease, all have their first local manifestation in the secreting cells of the tubuli.

2d, That the best marked variety of this disease, the granular kidney of Dr Bright, is a fatty degeneration of the secreting cells of the kidney.

3d, That these different conditions of the secreting cells and of the urinary tubuli can be detected in the urine by microscopic examination, thus rendering the microscope second only to the stethoscope in importance as a means of physical diagnosis.

Several interesting cases are detailed in support of these views. In one case, after all the usual diaphoretics and diuretics had failed in removing the dropsy, "the patient remembered that tomatoes had a very decided effect in increasing his urinary secretion, and in acting upon his bowels in health. He sent for some that had arrived from Bermuda. They acted with great power in increasing the quantity of urine to a remarkable degree, producing watery evacuations from the bowels, and causing the rapid disappearance of the dropsical effusions, and at the same time a great improvement in his strength and comfort.

No. VI.—*Report on Diphtheritic Croup, by Morgan Lingden, M.D.*, has been so fully referred to by Dr Wood in his paper on the inflammatory diseases of the mucous membrane, in a former number, that we only quote the title here.

The remainder of the volume is occupied by excellent addresses, which are of too general a kind to analyse here—Reports of Lunatic Asylums, and a very learned paper bearing the title of "*The Atomic Theory of Life and Animal heat, as balanced by the Agency of Water.*"

In this most elaborate essay, the author undertakes to prove,—

"*1st,* That the alternate formation and combustion of atoms of wood build up, hold up, heat up, and vitalize creation;" and

"*2d,* That common salt, water, iron, coal, contain the six primary elements, whose chemico-vital interchanges produce the formation and transformation of wood and water for the generation and regulation of all vital heat, vegetable and animal."

The ingenious reasoning by which these propositions are supported scarcely admit of condensation, and as the whole subject is rather of too transcendental a nature for our pages, we content ourselves with this simple reference to it, and take our leave of this most interesting volume. The variety of subjects which it embraces has not admitted of more than the briefest indication of its contents, but we close its perusal with a firm conviction that our Western brethren are prosecuting medicine in a manly, vigorous, and independent manner, and we shall always hail with satisfaction the arrival of similar volumes of transactions in time to come.

ART. III.—*On Diseases of the Liver.* By GEORGE BUDD, M.D., F.R.S., Professor of Medicine in King's College, London; and Fellow of Caius College, Cambridge. Second Edition. London, 1852. 8vo., Pp. 486.

The present work appeared, first, in June 1845; and in the sixty-fifth volume of this Journal, January 1846, an account of its contents at considerable detail was given. The work was at the same time recommended to the attention of physicians, as presenting a correct and useful view of the actual state of knowledge, pathological and therapeutic, on the morbid conditions incident to the Liver.

In the second edition of the work which is now before us, Dr Budd has studied, with laudable industry, accurate and extensive knowledge, and much judgment, to render his treatise an instructive and serviceable guide to the physician on the Causes and Nature of Hepatic Diseases, and the treatment demanded by each form of disease.

The method of arrangement of the subjects is the same as in the first edition;—an introduction upon the structure and functions of the Liver; on the composition and properties of the Bile; and on the influence of Chologogue Medicines; then five chapters on—1. Congestion of the Liver; 2. On Inflammatory Diseases of the Liver; 3. On Diseases resulting from faulty Nutrition of the Liver, and from faulty Secretion; 4. On Diseases which result from the presence of Growths foreign to the Natural Structure; 5. On Jaundice; and lastly, an Appendix on the nature and causes of the Liver Fluke in animals, and the effects produced by the presence of that parasitical animal.

Under these different heads a considerable amount of new matter is introduced, the effect of which is to augment the treatise by the addition of eighty-five pages. It is proper also to say, that Dr Budd has been at great pains to improve his work, by the introduction of much accurate information, and especially by the re-examination of all the chemical analyses, by the aid of Dr Lionel S. Beale, a gentleman who has made himself very favourably known by several correct researches in the department of Animal Chemistry and Chemical Physiology and Pathology.

The volume is illustrated by four beautiful and accurate coloured engravings, illustrating the appearances and structure of gallstones, the effects of abscesses, and encysted tumours of the Liver, and the characters of melanosis of the gland.

As in the account of the work of Dr Budd, given in the sixty-fifth volume of this Journal, the contents of each of the chapters were examined at some length, it is unnecessary at present to go over the same ground. But it is proper to say, that the treatise is one of great merit, and it cannot fail to be most serviceable to medical practitioners. As such we have great pleasure in recommending it to the careful attention of the profession.

ART. IV.—1. *A Treatise on Acute and Chronic Diseases of the Neck of the Uterus, illustrated with numerous plates, coloured and plain.* By C. D. MEIGS, M.D. 8vo. Pp. 116. Philadelphia and London, 1854.

2. *An Inquiry into the Pathological Importance of Ulceration of the Os Uteri, being the Croonian Lectures for the year* 1854. By CHARLES WEST, M.D., &c. 8vo. Pp. 95. London, 1854.

These two volumes are laid before the profession under the auspices of different public medical Institutions. To a certain modified extent they represent these bodies, and have additional intrinsic weight and importance from this circumstance.

Dr West having received from the Royal College of Physicians of London the honourable appointment to the annual Croonian Lectureship for 1854, delivered before that body three lectures, which, as now quickly published, form the work under review. On the other side of the Atlantic, Dr Charles D. Meigs, the well-known and distinguished Professor of Midwifery in Jefferson Medical College, drew up, and presented in the name of a committee of the American Medical Association, a report on acute and chronic diseases of the cervix uteri. This report was read at the New York meeting of the association, and is now published as a separate treatise.

Both of these authors have been frequently before the profession, and have acquired much reputation for knowledge of their favourite studies—the diseases of women and children; and there can be no doubt that their present publications will have ample consideration bestowed upon them by the profession on both sides of the great ocean. As we might have predicted, *a priori*, from a consideration of the intellectual differences of Drs West and Meigs, their works, although nominally on the same subject, are at utter variance with one another as to style of writing and manner of handling of the subject. Likewise do we find that, for purposes of analysis or critisism even, they cannot well be fused. The work indeed of Dr West is so compact and closely reasoned, and altogether admirable, that the critic does well to become a mere analyst, and lay before the profession a view of the sound arguments and conclusions detailed in its three chapters. On the other hand, Dr Meigs' book is so loose and ravelled, and contains withal so much of physiology, of pathology, and of therapeutics, that is either unintelligible, or questionable, or erroneous, that to criticise it justly would involve us in too much labour, and swell our brief article into a book. In spite of the many good things in the volume, we must limit ourselves to the exhibition of some of its many peculiarities of style and of doctrine.

Dr Meigs' work is the antithesis of Dr West's. It comes, somewhat unfortunately for its author, under our critical pen at the same time as the English work. An ingenious writer has aptly compared two books in this predicament—a favourable one for the judge—to two bottles of beer. The one is excellent, full-bodied, sound, and clear. The other has qualities to recommend it; but tasted in comparison with its neighbour, it has no chance of the favourable expressions of gratification which it might have elicited had it come singly and alone to satisfy an unquenched thirst.

We hail both of the volumes with much pleasure, not only on account of their own great value, but also on account of the manly and honourable professional spirit in which they are written. The science and art are sure to be benefited by the deliberate publication of well-considered works such as these. For a time the progress of this department of professional study has been defaced and rendered repulsive, if not retarded, by injudicious and hasty publications. Some of these were, perhaps, justified by the apparent necessity for exposing the pitiable system which has been too much cultivated, of victimising the weaker sex, by an induced terror of the present and remote consequences of trivial complaints, and by the too frequent use in treatment of mechanical means of imposing appearance, shocking to the delicacy or pleasing to the pruriency of the patient. We hope this sorry stage in the advance of obstetrics is now for ever passed.

On this point Dr Meigs justly remarks,—

> "Even if it be true that some abuses, disgraceful to medicine, do exist, we are persuaded that such discreditable examples must be very rare among a class so generally acknowledged for wisdom and virtue and politeness, as the great body of the United States' physicians. Wherefore, we do not find ourselves called upon to discountenance and reprobate the prudent and necessary employment of modern methods and instruments, invented to improve the means both of diagnosis, and treatment of diseases of the cervix uteri; conceiving that this is a matter to be left to the conscience and judgment of our brethren, whenever the occasion may arise.
>
> "We must not repudiate, and, by wholesale reprobation, condemn and annul a good method of cure, merely because incompetent or wicked persons might or do abuse it to their own gain; to do so, would be to amerce and punish the sick and the miserable for faults of our brotherhood."

Dr Meigs commences by a description of the discharges which occur in cases of leucorrhœa. He describes the muciparous glands of the vagina as furnishing a thin watery mucus or else one of a creamy consistence, which in other instances appears to be butyraceous or concrete. The excretion from the follicles and glands of the canal of the neck, he says, is always gluey or albuminous, and resembles fresh white of egg; and if a patient informs us that she discovers a slimy transparent mucus, and especially if it appears at intervals of once a day or oftener, then we may take it for granted that she labours under inflammation of the neck of the womb. In this account, there is much confusion, and we believe error, the more unpardonable as the discharges from the vagina in health and disease are susceptible of exact description; and if Dr M. did not examine the discharges for himself, he had only to refer to the works of Donné, Whitehead, and Tyler Smith, for much valuable information on the subject, which should have had the place of his vague and meaningless descriptions. When we come to notice the work of Dr Tyler Smith on this special subject, which is announced as being in the press, we shall present our readers with a sketch of it.

Leucorrhœa is a weakening disorder, says Dr M., not because of the wastage of the material, but because of the disturbing effect in the general economy, produced by even slight modifications of the health of the uterus. Any one who has carefully observed much of the simple leucorrhœa here alluded to, which is common in delicate and scrofulous females, and which, contrary to Dr Meigs' advice and opinion, is oftener aggravated than removed by vaginal injections, will assuredly disagree with Dr Meigs. In such females slight modifications of the health induce leucorrhœa, which is of little importance in any respect except as an indication for tonic regimen and diet, which can alone satisfactorily remove the constitutional weakness or derangement and the leucorrhœa simultaneously.

Dr Meigs candidly admits that the treatment generally instituted in leucorrhœa is notoriously unsuccessful. It certainly will be so, if for every simple leucorrhœa astringent and caustic injections are resorted to, as recommended by Dr Meigs. The serious cases (that is, those not cured by the injections) are, he says, cases of disease of the cervix—and a vaginal injection for inflammation of the canal of the neck is simply ridiculous. Here again we find Dr Meigs to be simply ridiculous ; for if he tries, he will find that vaginal injections medicated, or of plain cold water, will cure the great majority of cases of leucorrhœa with morbid appearances of the cervix. Daily experience proves this; and surely Dr Meigs must have perused the work of Dr Bennet of London, the great patron of inflammation of the uterine neck. " Astringent injections (says Dr B.) are most valuable remedies in the treatment of inflammation of the lower segment of the uterus. Cold water not only acts as a wash or lotion, but has a decided therapeutic effect." We admit that there is a class of cases of leucorrhœa depending on disease of the cervix so great, that to treat by injections would be to trifle. In these the speculum and caustics are required. It is a great mistake, and one in which Dr M. does not stand alone, to consider such cases of inflammatory disease of the cervix as *the* serious cases. The reverse is the truth, as we have elsewhere shewn. When the disease is confined to the cervix, it is in a part completely within the control of the physician, and by proper remedies it can be dispelled. *The* serious cases are those where the uterine body is implicated. These cases are really serious to the patient, and difficult for the physician successfully to treat. Cases, also, of the first kind, namely, of simple leucorrhœa, are often serious from their continuance and difficulty of removal. Of the whole category those of disease of the cervix are the simplest. More need not here be said, as we shall in this review sketch the argument of Dr West's work, which proves the preponderating importance of disease of the body of the womb in the great run of uterine cases

Hastening through the book, we come to the subject of uterine hypertrophy, which we shall briefly notice.

> " Women, who complain of intrapelvic pain, and other disorders of the lower part of the body, are inclined, in general, to account for these incommodities by referring them to, what they call, womb complaints. This term is so vague and imprecise, that the pronouncing of it rarely excites any clear precise idea. Among these cases of womb-complaints, probably none are so common as those which depend upon retroversion of the womb. So frequently does this affection occur in the course of one's medical practice, that one almost acquires a disposition, in every case, before examination, to suspect it has some dependency upon *retroversio uteri*."

Dr Meigs, always facile, has no difficulty in accounting for the frequent occurrence of retroversion.

> " A great many women and young girls suffer themselves to acquire the bad habit of retaining the urine until a large quantity is accumulated within the bladder. Thirty ounces of liquid, in the urinary bladder, make a mass as big as a quart measure, and it cannot but thrust the uterus injuriously backwards, causing the neck to see-saw at the same time forward, and approach the pubis.
>
> " A long habit of this kind comes at last to ruin the uterine ligaments; so that the fundus, being wholly overset backwards, sinks down into the Douglass cul-de-sac, while the os takes a permanent place near the symphysis pubis; and this is retroversion. It is even sometimes a congenital malposition, as shewn by Morgagni and others.
>
> " But let it be remembered that the womb is constitutionally prone to set off on a race of hypertrophic development, and we may then understand how it shall be readily provoked to commence a process of hypertrophization, by the awkward, unnatural, and irritating posture it acquires in retroversion."

Gradually the doctor waxes quite marvellous, and describes a style of retroversion of the unimpregnated uterus, which we are sure nobody but himself ever saw.

> " Medical men called upon to diagnosticate in such cases, should be very careful, first to establish in their minds the ideal or standard, by which to compare ; otherwise they will be apt, upon finding the whole pelvis occupied, or blocked up with an immense and immovable solid mass, to conceive of it as a tumour ; whereas in fact it may prove to be no tumour, but only an overset and enormously hypertrophied uterus."

Here the doctor builds another castle in the air. Speaking of this uterine hypertrophy, he says,—

> "If the cause should prove to be a mere retroversion, we may count upon a cure if we can reposit the womb, and maintain it in its proper attitude in the pelvis. This is, in fact, all that we shall be called upon to do; for, this being done, the organ hastens to recover by involution its non-gravid form and substance, as after a delivery at term, or after an abortion."

To effect this supremely desirable object, Dr Meigs crowns his work by proposing a new annular pessary made of watch-spring, which will neither keep the womb in its place, nor keep its own place for many minutes, but will probably be sóon reported as at least equally successful in retroversion as the numerous other mechanical contrivances proposed for the same end. We shall not here proceed further with this inviting subject, because we shall have something to say about it with Dr West; and because we present our readers, in another part of the Journal, with an account of the admirable report of M. Depaul on the mechanical treatment of uterine displacements. Moreover, the subject was pretty fully discussed in our last number.

After pointing out the great imperfection of our knowledge concerning the diseases of women, and the reasons for it, Dr West at once proceeds to judge the question of the alleged importance of the so-called ulceration of the os uteri.

I. In the first place, he demolishes the *a priori* argument for the importance of ulcerations of the cervix, founded upon the *notion*, that this part receives a greater amount of blood, and is endowed with a higher degree of vitality than other parts of the organ.

> "It suffices to examine the healthy uterus for any one to satisfy himself of the smaller vascularity of the neck than of the body of the womb. It is the body which chiefly grows as the period of puberty approaches; it is the body to which the great determination of blood takes place during each menstrual period, and from the lining membrane of the body that the menstrual flux is poured out. The looser tissue, the large vessels, the congested mucous membrane characteristic of the menstruating uterus, are limited, or nearly so, to the body and fundus of the organ; and it is the epithelium of its cavity, not that of the neck of the womb, which is abundantly intermixed with the menstrual fluid. When conception takes place, it is the body of the uterus which first and chiefly enlarges, its mucous membrane which becomes developed to the decidua, its tissue which grows and is metamorphosed into muscular fibre; while the changes in the membrane of the cervix are limited to an increased activity of its mucous follicles, and the alterations in its substance to an increased formation of fibre-cellular tissue, with a comparatively scanty growth of muscular fibre. After delivery, the retrograde processes are much more striking in the body than in the neck of the womb. The mucous membrane of the

cervix, stretched during pregnancy till the folds which it presented in the unimpregnated condition are obliterated, resumes once more its former plicated arrangement, while that of the body is detached and reproduced again and again before the organ reverts to its former state. The cervix is less sensitive than the body of the uterus: the sound which passes along the canal of the former almost unfelt, generally finds the lining of the uterine cavity acutely sensitive. The cervical canal has been forcibly dilated, it has been incised; the tissue of the cervix has been burnt with the strongest caustics, or with the actual cautery, or portions of it have been removed by the knife, generally with no injurious consequence; often with so slight a degree of constitutional disturbance, or even of local suffering, as to surprise those who advocate, little less than those who condemn, such proceedings.

" But, if structurally so lowly organised—if physiologically of such secondary importance— if so much less subject than the body of the uterus to alterations in its intimate structure—and if so comparatively insensible even to rude modes of therapeutical interference—it certainly does appear to me that the assumption that some slight abrasion of the mucous membrane covering this part is capable of causing a list of ills so formidable as are to be attributed to it, ought to rest for its support upon some other and stronger foundation than any inference fairly deducible from anatomical or physiological data."

II. In the second place, he proceeds to inquire into the nature of the evidence on the subject, of the importance of ulceration, which can be deduced from anatomical investigation. Dr West begins by carefully noting the *post-mortem* appearances in 62 cases; of these 62, 13 were above 45 years of age; 49 were between 15 and 45 years; all of the former, and 30 of the latter, making a total of 43, were either certainly known, or concluded, with great probability, to have had sexual intercourse. The remaining 19 were believed to be virgins.

In nearly a half (29) of these 62 women, the uterus was found diseased. In nearly a half (8) of the 19 virgins, the uterus presented some sign or other of disease. In 5 of these 8 there was slight abrasion of the os uteri.

" There is certainly something at first not a little startling in the result at which we arrive, that the womb was found in a perfectly healthy condition in little more than the half of 62 women, none of whom died of uterine disease, nor were supposed to be suffering from any grave uterine ailment. But it may, it ought indeed to be, asked, what is the value of these appearances? Some of them may be of little moment, and the very frequency of their occurrence, instead of substantiating the opinion that they are of great importance, rather militates against that supposition. When ulceration of the os uteri was first observed, it was natural enough to attribute to it many symptoms, and to refer to its influence many structural changes. But what if such ulceration be found to be usually very limited in extent, and so superficial as to be unassociated with changes in the basement membrane of the affected surface, and exercising so little influence on the state of the uterus in general, as to be unconnected in a large number of instances with changes either in the interior of the womb, or in its substance; while induration of the uterine tissue and disease of the lining membrane of the womb are found independently of it, or of each other? Should such appear to be the case, it will, I think, be rendered in the highest degree probable that this abrasion of the os uteri has not the long train of sequences which have been supposed to follow it, but that it is of comparatively small pathological import; that it may be found to vary under the influence of comparatively trifling causes; and not infrequently to be dependent on functional disorder of the uterus, just as the mucous membrane of the tongue and mouth betrays the disturbance of the digestive system; that it may, in short, be the consequence, and sometimes the index, but rarely the occasion, of the ailments with which it is associated."

In 11 instances abrasion (*i.e.* ulceration) of the os was observed, without any other morbid condition of the womb.

Induration of the uterine tissue, which has been hypothetically ascribed to the so-called ulceration, was observed in 9 cases. In 5 of these it was not associated with any other morbid condition of the uterine substance. In 3 it coexisted with an ulcerated os, and in 1 with a morbid state of the interior of the uterus.

A morbid state of the lining membrane of the uterine cavity was found in 10 cases. In only 3 of these was there ulceration of the os of moderate extent.

"And thus," Dr West adds, "we have seen that, in by far the majority of cases, the ulceration, when present, was not merely trifling in extent, but that it had not given rise to so much irritation of the neighbouring tissues as to produce any appreciable congestion of the mucous membrane in its vicinity; while the changes in the uterine substance alleged to depend upon it were oftener present without than in connection with it; and, moreover, none of the alterations about the os and cervix of the womb were so considerable as those which were apparent in its cavity.

"Other evidence, indeed, must be adduced than that which analogical reasoning from the facts of physiology has suggested, or than that which examinations after death have furnished, before we shall be entitled to reject the opinion that inflammation of the cervix, and ulceration of the os uteri, are occurrences of very serious pathological importance,—the occasion of nearly all the ills which affect the physical well-being of woman."

III. The symptoms asserted to characterize inflammation of the neck of the womb, and ulceration of its orifice, are not met with constantly, or in a specially marked degree, in cases of prolapsus or procidentia uteri. On the contrary, if inflammation of the neck of the womb were as common as has been supposed, or ulceration of its orifice, the necessary occasion of such serious disorder of function and alteration of structure, we ought to meet with striking examples of these facts in cases of procidentia,—which we do not.

IV. What are the results of clinical observation concerning the course, symptoms, and importance of this ulceration? Here Dr West first refutes the notion of the peculiar susceptibility of the cervix to inflammation and ulceration from local injury:—

> "Observation," he says, "seems to shew that, be the causes of ulceration of the os uteri, of inflammation, hypertrophy, and induration of of its cervix, what they may, sexual excesses, at any rate, have no great share in their production. Four years ago, being anxious to satisfy my mind on this point, I examined, by permission of Mr Lawrence, forty women on their admission into the venereal wards of St Bartholomew's Hospital. Of these 40 patients, 18 suffered from gonorrhœa alone, 10 from gonorrhœa and syphilis, and the remaining 12 only from syphilis. The cases were unselected, and the examinations were made as soon as possible after the admission of the patients into the Hospital.
>
> In 27 instances, the os and cervix uteri were absolutely healthy, or presented only, and this but rarely, a slight blush of redness, deepening the natural hue of those parts. In 10 of the remaining thirteen, the ulceration, if indeed it deserved the name, was a mere excoriation not above a line in breadth, partially or completely circumscribing the os uteri, but associated with no other change of its tissue. In the remaining 3 cases, the abrasion was more extensive, surrounding the os uteri for about a third of an inch; and in the case of one of these three, that of a woman who had given birth to children, the lips of the os were noted to be elongated; this, however, was the nearest approach to a hypertrophied state of the cervix met with in the whole 40 cases; while in no instance was there any such alteration of the texture of the part as to deserve the name of induration."

Dr West then proceeds to the careful investigation of his clinical observations, and arrives at the following conclusions, to which he appears to have been guided by the encouragement to his views afforded by the results of statistical tables of his cases:—

> "1st. Uterine pain, menstrual disorder, and leucorrhœal discharges,—the symptoms ordinarily attributed to ulceration of the os uteri—are met with independently of that condition almost as often as in connection with it.
>
> "2nd. These symptoms are observed in both classes of cases with a vastly preponderating frequency at the time of the greatest vigour of the sexual functions, and no cause has so great a share in their production as the different incidents connected with the active exercise of the reproductive powers. But it does not appear that ulceration

of the os uteri exerts any special influence, either in causing sterility or in inducing abortion.

"3rd. While the symptoms are incidental in character in the two classes of cases, they seem to present a slightly increased degree of intensity in those instances in which ulceration of the os uteri existed.

"4th. In as far as could be ascertained by careful examination, four-fifths of the cases of either class presented appreciable changes in the condition of the uterus,—such as misplacement, enlargement, and hardening of its tissue, while frequently several of these conditions co-existed. An indurated or hypertrophied state of the cervix uteri was, however, more frequent in connection with ulceration of the os uteri than independently of that condition.

"5th. The inference, however, to which the last-mentioned fact would seem to lead, as to the existence of some necessary relation,—such as that of cause and effect,—between ulceration of the os uteri and induration of its cervix, is in a great measure negatived by two circumstances:—

"1. The number of instances in which an indurated cervix co-existed with a healthy os uteri.

"2. The fact that, while induration of the cervix was present in 25 out of 46 cases in which the ulceration of the os was very slight, it was altogether absent in 9 out of 16 cases in which the ulceration was noted as having been very extensive."

Having thus disposed of the fallacies of certain opinions, Dr West apologizes for not being able to supplant them by doctrines equal in simplicity and universality of application. He disclaims any such pretensions, and believes, on the contrary, that the different symptoms attributed to ulceration of the os uteri, instead of being produced by any such cause, arise from various causes. These are sometimes found in constitutional disorders, sometimes in ailments of the sexual system, of various kinds and seats.

In illustration of the influence of general constitutional disorder, he cites the case of the chlorotic girl having amenorrhœa and leucorrhœa, uterine and lumbar pains. Remedies improve the general health, correct the morbid state of the circulating fluid, and simultaneously dispel the uterine ailments.

"Or, in another and usually a much older person, the liver performs its functions but imperfectly: constipation, headache, an icteroid tinge of the skin, mark its disorder; at length menstruation comes on with unusual profuseness, and the signs of hepatic disturbance disappear. This may occur but once, or it may occur several times; the slightest error in diet, the slightest neglect of the state of the bowels, or some cause too trivial to be discovered, may produce the same derangement of the function of the liver, which may again give rise to the same train of symptoms, and cease on the recurrence of a similar hemorrhage. But nature's healing power is often ill-regulated, and erring in too little or too much; the regular periodicity of the menstrual function becomes deranged; many weeks pass without the appearance of the menses, and then they appear again and again with a frequency and profuseness which seriously impair the patient's health, which sometimes even threaten her life.

"Or, again, the kidneys, from unsuspected disease, perform their duties incompletely, and the blood, imperfectly depurated, its red particles diminished, its watery elements increased, pours forth at the return of each periodical congestion of the womb in larger quantities than when the patient was in health. Leucorrhœal discharges follow in the intervals of menstruation; pain is felt in the back; bearing down follows any exertion; the frequent micturition seems to indicate the sympathetic suffering of the bladder, while the loss of muscular tone, and the long persistence of congestion of the womb at every period, favour the actual prolapsus of the organ. The patient seems to be sinking under menorrhagia; she is really dying from the effects of granular degeneration of the kidneys, and consequent albuminuria.

"Or, lastly, to take another illustration of the mode in which uterine symptoms depend on constitutional causes, let us notice how important a part the gouty and rheumatic diathesis sometimes plays in the production of uterine symptoms. A casual attack of cold is in some instances referred to as the occasion of the patient's illness, while in other cases the ailment comes on by degrees, and with no definite exciting cause. Menstruation begins to be more painful than was its wont, often more scanty; an unusual degree of constitutional disturbance attends each period; the pulse at those times is very frequent, the skin hot though perspiring, and lithates abound in the urine. In the intervals profuse leucorrhœal discharges take place; the pain, though less intense, is yet severe, and is aggravated by trifling causes, or without any obvious reason. The pain at one time is most severe in the back, at another is referred to one or other iliac region, shooting down the legs, in the course of the crural nerve, or, like sciatica, affecting the back of the thighs; while occasionally, in addition to these abiding discomforts, the patient is kept in bed for a day or two at a time by slight feverish attacks, accompanied by wandering pains in the limbs, though seldom attended by inflammation and swelling of any of the joints.

"Cases of this kind are met with in all classes of society; and wherever they occur they are chronic in their course and difficult in their cure. Among the poor they do badly out of an hospital; for rest, uniformity of temperature, and a suitable diet, can seldom be obtained, while they do but ill in an hospital, since the absence of regular occupation and of exercise cannot but be injurious, while the listlessness of mind which the patient's state and circumstances engender is favourable to hypochondriasis, and to the exaggeration of any ailment of the nervous system. The affection is one not unusual among young women who follow the occupation of book-folding, working, as they are compelled to do, in a heated, but at the same time in a damp atmosphere, and for the most part in ill-ventilated apartments. A very obstinate case of it came under my notice some time since in a young person who had been compelled, by lameness from birth, to lead a very sedentary life, and who had always suffered from vague rheumatic pains. And, not long since, a washerwoman, whose finger-joints were enlarged by chalk-stones, but in whom menstruation had already ceased, applied at the Out-Patient room of St Bartholomew's Hospital on account of profuse leucorrhœa and rheumatic pains, which disappeared simultaneously under the use of colchicum and iodide of potassium.

"The rich enjoy no immunity from this ailment."

In a large proportion of cases the uterine symptoms are to be referred to deficient involution of the womb after delivery; and sometimes, to this condition, is superadded an inflammatory condition of the organ. Cases also occur where gonorrhœal vaginitis extends to the mucous membrane of the uterine cavity.

Dr West, in his last chapter, guards the reader against the notion that ulceration of the os is absolutely of no importance, and makes some good remarks on special affections of the os, known generally as the granular inflammations of that part. He also enters into some practical details, and broaches numerous views in uterine pathology which we might comment upon. They are not necessary parts of his "inquiry into the pathological importance of the os uteri," but form an additional ground for our recommendation of the book to all our readers.

ART. V.—*Class Book of Botany, being an Introduction to the Study of the Vegetable Kingdom.* By J. H. BALFOUR, M.D., F.R.S.E., Regius Keeper of the Royal Botanic Garden, Professor of Medicine and Botany in the University of Edinburgh, &c. 8vo., pp. 1114. Edinburgh, 1854.

Professor Balfour has made a name for himself as a teacher of Botany. This book embodies the substance of what he has been accustomed to communicate orally to his pupils. It is a work on Botany of the very highest order. It embraces every department of the subject. It is in every respect lucid in its arrangement, clear in its details, copious in its illustrations. The book before us is the picture of a man's mind who has no engrossing pursuit but Botany—whose soul is absorbed in the contemplation of his own science. Let no one apprehend that this is said with the slightest view to the disparagement of our author; we speak of it as in the highest degree to his praise. It is only by an exclusive devotion to one subject that such a book as this can be produced, exhibiting, as it does, every topic of interest, down to the most recent announcements, commented on in the justest terms. Even if our limits permitted, the practical character which belongs to this Journal would prevent us from attempting anything approaching to an analysis of such a work. We must content ourselves with extracting a well-condensed summary of a most interesting chapter on the diseases and injuries of plants.

"Recapitulation of the chief facts connected with the diseases and injuries of plants:—

"1. The common causes of diseases in plants are improper soils, ungenial climates, frosts, rains, drought, storms, parasitic plants, insects, and wounds of various kinds.

"2. Diseases frequently originate in the nitrogenous lining of the cell-walls, and thence are propagated to the contents of the cells, and to contiguous tissues.

"3. Diseases have been divided into—those arising from excess or deficiency of light, heat, air, moisture, or the ingredients of the soil; those caused by parasitic fungi; those attributable to the action of poisons; and those produced by mechanical injuries, and by attacks of insects.

"4. Some of these diseases are propagated by contagion, more especially those occasioned by fungi; others spread epidemically, and seem to depend on atmospheric changes and meteorological causes.

"5. Some plants are more predisposed than others to disease, and it is found that those plants are most liable to be attacked by parasitic fungi which are enfeebled in their growth.

"6. Plants grown in an ungenial climate and soil are very liable to disease; tender exotics in Britain are often injured by spring frosts.

"7. Plants are suited to particular climates, and it is impossible to increase by cultivation their power of enduring heat or cold; there is no evidence of plants being acclimatized by a process of cultivation.

"8. The sea-breeze, carrying with it saline particles, is often very injurious to trees. Some trees resist the effects of the sea-breeze better than others.

"9. The attacks of parasitic fungi are a very frequent source of disease. These fungi may either originate the disease, or aggravate and modify it after it is produced.

"10. Many diseases in cultivated grains, such as bunt, smut, rust, and mildew are referred to attacks of species of uredo, puccinia, and ustilago.

"11. Various remedies have been suggested, such as steeping grain in boiling water, in a solution of sulphate of copper, and in alkaline solutions.

"12. Other fungi, more especially species of erysiphe and oidium, give rise to mildew in plants. The vine mildew caused by oidium Tuckeri is said to be prevented by the application of sulphur.

"13. Ergot is a diseased condition of the ovary of grasses, which assumes a spurred appearance, well seen in diseased rye. It seems to be caused by an attack of a fungus called cordyliceps purpurea.

"14. The late potatoe disease has been attributed by Berkely and others to a fungus attacking the leaves and afterwards causing injury to the tubers. The disease seems to appear first in the leaves, which, when affected, present the same aspect as those injured by irritant gases.

"15. By others the disease is attributed to changes in the cells caused by cultivation, which render the plant liable to various epidemic influences; a nidus is formed for the spores of fungi, which accelerate decay, and give a peculiar character to the disease.

"16. Bollman says that drying, if conducted at a sufficiently high temperature, and continued long enough, is a complete antidote to the disease.

"17. Dry-rot is a disease to which the wood of trees is liable, alterations take place in the cells and vessels, and a fungus called merulius lacrymans or vastator is developed, which causes a very rapid disintegration of the wood.

"18. Dry-rot occurs also in wood after being cut, when it is exposed to damp, and when, at the same time, there is not a free circulation of air. Some woods are more liable to dry-rot than others.

"19. The modes of preventing dry-rot are keeping the wood dry and well ventilated, steeping it in a solution of corrosive sublimate (kyanizing), or in a solution of chloride of zinc (burnettizing), or causing it to absorb, after Boucherie's method, solutions of acetate of lead, or of pyrolignite iron, or of creosote.

"20. Some succulent plants are liable to gangrene, by which the parts become sphacelated and decay.

"21. Plants are also liable to be destroyed by the attack of phanerogamous parasites such as the dodder.

"22. Metallic and vegetable poisons destroy plants; the former are as irritants, the latter as narcotics.

"23. Acid and narcotic gases in the atmosphere are injurious to plants, causing disease and death.

"24. Many insects act injuriously on plants, either destroying them at once, or inflicting wounds which end in a diseased condition of the parts.

"25. The disease called the ear-cockle or purple is caused by the vibrio tritici or wheat-eel.

"26. Species of cecidomyia, aphis, and coccus are destructive to cultivated plants.

"27. Galls are produced by the attacks of insects (species of cynips), which deposit their ova in the bark and leaves of plants. Oaks and willows are frequently affected with galls.

"28. A species of adelges causes a peculiar cone-like formation on the branches of spruce.

"29. The wire-worm, which is the larva of a beetle, is very destructive to plants.

"30. Various remedies have been suggested to prevent the attacks of insects, such as quicklime, sulphur, turpentine, tobacco, and the liquor of gas-works."—(Pp. 703, 704.)

ART. VI.—*Transactions of the Pathological Society of London.* Volume Fourth. London, 1853, 8vo, pp. 286.

That the labours of the Pathological Society of London have tended in no small degree to the edification of its members, as well as to the instruction of the public, we entertain no doubt. The society has done much to foster in the metropolis a spirit of research in morbid anatomy previously unknown to all beyond a narrow circle. It has taken a high place among the scientific institutions of this country, and each successive volume of its Transactions reveals a great improvement in the character, as well as an increase in the number of its communications. We have been far more interested by the perusal of this fourth volume than by any of its predecessors, and we venture to pronounce that the society, now in its *eighth* session, has really, as well as chronologically, left the period of its infancy behind.

With increase of years, however, the Pathological Society must count upon increased responsibilities. It is not now under the necessity of catering for materials to fill up the interest of its meetings. On the contrary, we look to its council for a much more severe guardianship than hitherto of its scientific position. We no longer hesitate to say that the early volumes, with a large proportion of valuable matter, contained much that was unworthy of a place in their "Transactions," and not a little that was deserving of positive condemnation, on account of the manifest errors or gross carelessness of the reporters. These deficiencies were of course chiefly observable in regard to the more recondite parts of morbid anatomy; but they are all the more on this account to be avoided in future. We shall not now be satisfied with learning that "*evidence* was afforded of the *inflammatory origin*" of a certain undescribed appearance, or that certain tumours had or had not "*all* the *characters of malignancy,*" or that another "specimen" was "removed from the face of a girl," who "made a favourable recovery, and has remained well since." Neither shall we approve of being told in the heading of an article that a specimen presented "scrofulous disease," while the author makes no attempt at description of the "deposit," and leaves us, as he confesses himself, totally "uncertain whether the state of the diseased kidney presented the result of pyelitis or the deposit of scrofulous matter." We are anxious to avoid even the appearance of hypercriticism, or we could point out many instances in the former reports where the usefulness of a communication has been entirely marred, and its very object has been rendered liable to misconception by the inability or the neglect of the author to give such details as were absolutely necessary to render it worthy of a place in the Transactions. We shall confine ourselves to one example in the present volume involving errors, not of omission, as in those cases to which we have referred above, but of a more glaring character. At p. 44, a report of 17 pages, and a lithographed plate, are devoted to a case entitled "Hydatids from the left Lung, subsequently to the occurrence of Typhoid Fever, complicated with double Pneumonia." The case is shortly as follows:—A married woman, aged 45, is seized with fever, accompanied by an acute affection of the chest, and having passed through a severe attack, becomes convalescent, and dismisses her physician. Seven months afterwards he is recalled, and informed that she has for some time been again subject to cough, and that she has "frequently 'coughed up small bladders of water,' which invariably burst immediately before passing into the mouth, and filled the latter with a 'kind of salt water.'" The history is so far borne out by signs which the physician seems to have considered as affording satisfactory evidence of a cavity in the lower part of the left lung, accompanied by considerable constitutional disorder; while portions of the "small bladders" were produced, along with expectoration of "a dirty white or yellowish-green colour," consisting "of numerous exudation masses and exudation cells, plastic and pus cells, and *portions of nerve-tubes and of lymphatic vessels*," &c. &c. A representation of these structures, so ludicrously unlike nature as to defy commentary, occupies the lithographed plate mentioned above. As Burns said of the sculptures on the old bridge of Ayr, they are

> "Forms—might be worshipped on the bended knee,
> And yet the second dread command be free.
> Their likeness is not found on earth, in air, or sea."

We can only venture to guess, for we never can know, what has been actually under the microscope of the observer at the time this unhappy sketch was made. Hairs, threads, extraneous filaments of some sort, possibly enough; but certainly not the nerves, or lymphatics, or indeed any part of the texture of the human lung. It is marvellous that the members of the Society were not staggered by these representations, or indeed that any one acquainted with the barest elements of pulmonary pathology could allow himself to suppose that *such* nerve-tubes, and *such* lymphatics had been expectorated from a pulmonary cavity. We are not favoured with a delineation of these equally wonderful "little bladders," one of which "is now in the possession of this Society," being "equal in size to a hen's egg." We confess to a feeling of unsatisfied curiosity about these bodies, when we learn that "they are seen, in point of structure, to approach as nearly as possible to that of a mucous membrane,' and that "in specimens of the most organized," the author found "a few isolated capillaries," as well as *white* and *yellow* fibrous tissue. Most certainly no informed person will recognise the description as applying to an acephalocyst, nor do we know any species of cystic entozoon to which it would apply. Possibly a farther examination of the specimen in the possession of the Society might find something even more extraordinary,—distinct muscular fibres, for instance, or a pair of ureters, the discovery of which, in a recent case, enabled one or two acute members of the Physiological Society of Edinburgh to refer certain cystic structures whose origin was shrouded in mystery, to their proper place in the animal scale. If this suggestion lead to no useful result, then we give it up, merely noticing the remarkable fact, that after passing through the narrow air-passages without violence, these "little bladders" should "invariably" have burst in passing the glottis. As regards the pathological and practical commentary of the author, we do not hesitate at once to confess, that his views are too profound for us to follow. We do not understand "the reason why the organic structures within the sac of the hydatids *could not* pass into the pus-type;" and as little can we comprehend how the formation of hydatids can be "favourable to recovery" from a disorganizing disease of the lung.

If this strange farrago had appeared as a separate publication, we should have been too glad to leave it to the oblivion which would certainly have been its fate. But however little disposed to do unnecessary violence to the susceptibilities of the author, it is impossible for us, under the present circumstances, to pass it over in silence, or to exonerate the Pathological Society from a grave responsibility in lending its countenance to the publication of statements so questionable, and in their present form, so utterly devoid of scientific value. We shall be told that, according to the often-repeated statement of the Council, "the exhibitors are alone responsible for the descriptions given of the specimens exhibited by them;" but we cannot accept this view of the case. No Society can indeed be expected to guarantee the fidelity of *all* that it brings before the public; but we contend also, that no scientific body can divest itself, by such a manifesto as the above, of the responsibility which fairly rests upon it, with regard to communications of which it becomes the medium.

Least of all can the Pathological Society do so. The communications which it receives, and of which its Transactions became the depositaries, are not exclusively "demissa per aurem," but are in great part "oculis subjecta *fidelibus*." In theory, at least, every communication should be accompanied by a more or less sufficient display of the actual facts on which it is founded; and by the excellent practice adopted during the last two sessions, of occasionally appointing committees to re-examine preparations, the Society has, as it appears to us, fully admitted the principle that its records should not be a mere "Sepulchretum Anatomicum," but a series of observations bearing the stamp of superior value and authenticity, which they can derive only from having been submitted to an enlightened criticism, and an impartial but careful scrutiny. It would be going beyond our province to consider by what alteration of the present machinery the guarantee to which we allude is to be afforded to the public; but on behalf of the public, we beg most earnestly to call the attention of the Council to the matter, and to represent that by attempting to throw the entire responsibility of publication upon the exhibitors, they are simply evading a most important and necessary duty.

And now, having got rid of the burden which has weighed upon our souls, let us turn to the more pleasing features of these reports. We have said that they present a body of documents of great importance, and, according to our judgment, of a progressively increasing value. Much of this improvement depends, no doubt, upon the scientific spirit which has at all times prevailed in the Council of the Society, and the efforts of individual members to diffuse, both by precept and example, a knowledge of the principles of pathological science, and of its more recent progress. Much has also been gained by the appointment of the small committees to which we have adverted above. By these a great additional impulse has been given to recondite and difficult investigations; exhibitors have in some cases, no doubt, learned more than they came to teach; and the Society has obtained a new means of evoking the talents of its younger and more active members in the work of observation. For very able reports, framed at the instance of the Society, the present and the last volume are largely indebted to Drs Peacock, Quain, Brinton, Jenner, Bristowe, Toynbee, and Lionel Beale.

Among the original contributors to the present volume we may particularly mention Dr Peacock, who has a most interesting communication on the mode of closure of the foramen ovale. Dr Hale, *apropos* of a heart with a single ventricle, has given a valuable resumé of the present state of scientific information with respect to the production of cyanosis. Dr Hyde Salter records an extremely valuable case of chronic hydrocephalus, with malformation of the cerebellum. Dr J. W. Ogle, among a large number of very complete and satisfactory observations, has one communication of general interest tending to shew the occurrence of the so-called "fatty heart" under a great variety of morbid conditions.—(This paper will be found in our summary.) Mr Jabez Hogg has a well-recorded and beautifully illustrated case of enchondroma of the testicle. Mr Birkett's case of villous cancer of the large intestine is also worthy of notice as a piece of accurate and satisfactory histological investigation; but it appears to us that the facts before us scarcely justify the application of the term "cancer" to this growth, if indeed they do not tend to exclude it from that class. We may likewise refer to the important papers of Dr Handfield Jones on the mucous membrane of the stomach, and of Dr Bristowe on the *muscular* nature of fibrous tumours of the uterus. We do not at present unreservedly adopt the opinions of either author; but we shall not lose sight of these subjects.

In conclusion, we may say that the great variety of interesting details in the present volume precludes anything like an adequate notice of it in this place. It is essentially a storehouse to be drawn upon as occasion demands hereafter, and we can assure those interested in pathological science that scarcely any inquiry in connection with this department of medicine can now be adequately prosecuted without a reference to the Transactions of this Society.

ART. VII.—1. *Traité Pratique d'Auscultation; ou Exposé Méthodique des diverses applications de ce mode d'examen à l'état physiologique et morbide de l'économie; suivi d'un Précis de Percussion.* Par M. BARTH et M. HENRI ROGER. Quatrième Edition, révue et augmentée. Paris: 1854, 12mo, pp. 719.

2. *A Clinical Introduction to the Practice of Auscultation, and other modes of Physical Diagnosis, in Diseases of the Lungs and Heart.* By H. M. HUGHES, M.D., Assistant-Physician to Guy's Hospital, &c. Second edition. London: 1854, small 8vo, pp. 303.

3. *A Practical Treatise on the Diseases of the Lungs, Heart, and Aorta; including the Principles of Physical Diagnosis.*

By **Walter Hayle Walshe**, M.D., Professor in University College, London, &c. Second edition, revised and much enlarged.

The publication of these revised editions of three well known and useful works on the physical diagnosis of diseases of the chest, almost simultaneously with the appearance of several similar works reviewed in the present and in our last number, while it bears a pleasing testimony to the activity of our teachers, and to the demand of the students of the present day for the best class of text-books, almost precludes us from going at length into the examination of these works in detail. Yet we cannot avoid recommending them all to the notice of our readers, and at the sametime pointing out what we conceive to be the characteristics of each.

The work of MM. Barth and Roger has received numerous additions and improvements, and is undoubtedly, in its present form, the best general expression of the doctrines prevalent in the French school on the subject of auscultation. Nor can these authors be accused, like many of their countrymen, of a habitual ignorance of foreign discoveries; the references to authorities and original papers are numerous and well-considered, and if it sometimes happens that French literature receives rather more, and German or English literature rather less, than its due share of attention in relation to some points of modern inquiry, we must at least acknowledge that there is a fair, and on the whole, a successful attempt to embody in the work the most practical and important aspects of the subject, and in doing so to render to every man his due. The authors have "endeavoured rather to diminish than to increase the stethoscopic nomenclature;" and we must award them the credit of great judgment in this respect, as well as of great clearness of style, and general accuracy of statement. The book justifies the character it has acquired as a guide for the student in France, and yet it contains in some respects much more, in others considerably less, than a tyro will be disposed to look for in a "Traité Pratique." The admirable little resumé, in the last hundred pages, contains almost everything which can be considered *essential* in a manual of physical signs; but the plan of this, as well as of the detailed part of the work, is strictly systematic and synthetic, rather than truly practical. Every separate physical sign receives consideration, and is generally amply and well discussed; but the combinations of these, to be looked for in particular diseases, are nowhere succinctly and separately stated; the work is therefore [illegible] than for use [illegible] to a special [illegible] been arranged [illegible] if the [illegible] we refer [illegible] knowledge imparted [illegible]

[illegible] function of [illegible] practice of [illegible] and well. There are, likewise, scattered through the volume many hints calculated to be of great service to the inexperienced auscultator ; inasmuch as they are the fruit of mature personal experience, and are recorded in a way likely to give them a hold upon the memory, and an influence in practice.

The work of Dr Walshe has already taken a high position in medical literature, not only as a text-book, but as an original treatise on the diseases of the chest. The present edition is thoroughly revised according to the author's most recent experience, and many new dissertations on points of more or less importance have been added. The first part is devoted to the exposition of Physical Diagnosis, the second to the consideration of Special Diseases. In both parts the author has pursued his subject into the most minute details, and has laboured with most praiseworthy energy to render his experience available to the student on every important question of fact that can possibly be raised as to diseases of the thoracic viscera. Were we to " hint a fault," it would be that this encyclopedic treatment of the subject has been carried so far as to render some parts of the work burdensome to the student, and even to diminish the impression left by it, as a whole, on the mind of the experienced practitioner. The curt and dogmatic form, too, in which the author's opinions are stated, even upon controvertible points, sometimes makes us wish he had been a little less sparing of discussion in regard to the views of others. Nevertheless, Dr Walshe's book is too important a gain to science to be received by us with anything less than a hearty welcome ; and if in its numerous pages there shall be found a few questionable doctrines, we may safely leave them to that *best* species of criticism which a work of authority like this is sure to receive from the well-informed class into whose hands it will undoubtedly fall.

The present edition well deserves a place in every library.

ART. VIII.—*The Census of Ireland for the year* 1851. Part III. *Report on the Status of Disease.* Presented to both Houses of Parliament by command of Her Majesty. Dublin, 1854. Folio, pp. 150.

In the 63d and 64th volumes of the *Edinburgh Medical and Surgical Journal* (April and July 1845), we directed attention to the very valuable report upon Table of Deaths appended to the Irish Census for 1841, in which the reader will find a great deal of information as to the diseases prevailing in Ireland during the preceding decennium, and also on the past history of disease in Ireland. Mr Wilde of Dublin, to whose professional knowledge and literary research the public was indebted for this report, suggested to the census commissioners the attempt to procure returns of a kind quite unprecedented, viz., an enumeration "of all those persons who laboured under disease either at their own homes or in public institutions, upon the night of the 30th of March 1851." In company with the Registrar-General (Mr Donnelly), Mr Wilde now presents such a report; and they remark in the introduction, that "no similar attempt has been yet made to collect and tabulate the diseases presented on a single day in even a limited portion of any other country."

It would be superfluous to enlarge on the value and importance of this report. It is divided into nine sections, containing respectively,—

Sect. I. The Deaf and Dumb.
„ II. The Blind.
„ III. The Lunatic and Idiotic.
„ IV. The Lame and Decrepit.
„ V. The Sick in Workhouses.
„ VI. The Sick in Hospitals.
„ VII. The Sick in Prisons.
„ VIII. The Sick in Asylums.
„ IX. The Total Sick in Ireland.

The first four sections present, with certain exceptions, a view of all those persons who, without being affected with mortal disease, remain a permanent burden upon society for the term of their natural lives. The report exhibits the age, sex, and occupations of those so affected, and gives a general view of the provisions which exist for their relief or instruction. Sections V. to IX. contain the statistics of all kinds of sickness, whether affecting those reported in the first four sections, or others. These sections contain also many interesting details upon the history of the Irish Hospitals and Fever Charities, exhibiting the results of much judicious antiquarian research, in a terse and clear narrative of no small literary merit.

The details furnished to the commissioners, particularly in regard to general and temporary sickness, cannot, of course, be expected to be either absolutely correct, or even to approach to such an amount of correctness as may possibly be attainable in the case of fatal disease. Concealment on the one side, malingering on the other, incorrect information from every cause and motive, must be allowed to give rise to errors and deficiencies in these returns. For instance, we find that in all Ireland there were, "on the night of the 30th March 1851," only twenty-five cases of syphilis, and none (!) of gonorrhœa, *in private houses*. We should have thought from this that an Irish millennium was at hand (and, of course, justice to Ireland not far off); but on looking to the returns of the public institutions, we found that in these, on the night in question, there were 542 cases of the former, and 31 of the latter, disease; and knowing that the usual practice of hospitals is to refuse cases of simple gonorrhœa, we are disposed, on second thoughts, to put aside for the present the idea of any danger to the Union by the foundation of a "new Atlantis" in the Emerald Isle.

This volume is of lasting interest, and we shall probably, as our space may permit, avail ourselves of its pages in our scientific summary. It reflects great credit on all those concerned in its production.

ART. IX.—*Elements of Human Anatomy.* By T. G. RICHARDSON, M.D., Demonstrator of Anatomy in the University of Louisville. Philadelphia, 1854.

The first 80 pages of this work are occupied with a well-condensed account of the minute anatomy of the textures; part second contains the descriptive anatomy of the bones and joints; and, in the third part, the remaining anatomy is arranged regionally, under the title of "Dissections,"—a plan which the author considers as his chief improvement upon the system of most writers on anatomy. As the whole anatomy, general, descriptive, and regional, is comprised in 700 octavo pages, in a good readable type, the author has denied himself space for that exactness and originality which should characterise the writing of those who have profited by the opportunities of experience which our author informs us he has enjoyed. The work is illustrated by about 300 excellent woodcuts. Most of those on general anatomy are taken from various standard works in this department, especially from our own well-known "Todd and Bowman," and acknowledgment is made of the sources from which most of these have been derived. But we regret to observe that nearly all the illustrations of descriptive and regional anatomy are unacknowledged, leaving the uninitiated to suppose they are original, although in the greater number of them we recognise our "old familiar friends." The author has demonstrated his high opinion of our "Quain and Sharpey," by transferring to his pages whole series of their illustrations, and Mr Quain's "arteries" and Wilson's "vade-mecum" have been laid under similar contribution; but without a single acknowledgment of the source from which these beautiful drawings are taken, although many of them are original in these works. Those alone who have had experience of it, can fully know the trouble attending the production of a good original drawing, which is certainly quite as much a matter of property, if not more so, than the accompanying description; and we offer our sincere sympathy to the above authors in the feelings they must experience as they turn page after page of this work, and find them adorned with those woodcuts which it cost them so much labour to produce. Perhaps Dr Richardson intends the compliment that our English text-books are so well known to be the source, that even the mention of them was unnecessary; or perhaps he may have feared that the confession would suggest to the shrewd young men who fill the benches at Louisville, the idea of themselves going to the original source, and laying hold of our Quain and Sharpey for themselves. We are far from saying that one author should not copy the illustrations of another; indeed it is much better that the compiler of a text-book should use the illustrations of those who have devoted time to the production of original drawings—as those of Albinus, Sœmmerring, Arnold, Tiedemann, Swan, Quain, Maclise, and others; but it is surely a matter of common honesty to acknowledge the source. We know not how our American brethren view such matters, but we can tell our author that such a procedure in this country would render the name of its perpetrator a byeword in the profession.

We call on Dr Richardson, for the sake of his own reputation, as well as that of the University to which he belongs, and for the sake of the literary reputation of his countrymen, to rectify the injustice with which we charge him; and, if this be done, we shall be glad to receive and notice a second edition of his work.

ART. X.—*Veterinary Medicines, their Actions and Uses.* By FINLAY DUN, V.S., Lecturer on Materia Medica and Dietetics, at the Edinburgh Veterinary College, &c. Edinburgh, 1854.

"A merciful man is merciful to his beast," and the mercy is of that kind to which Shakspeare refers, as blessing him that gives and him that takes. The wisely-fed, healthy horse, does his master's work in a fashion that makes his backer welcome him with approving smiles; and the milky mothers repay the care bestowed upon them, by such returns of butter, cheese, and cream, as gladden the heart of the farmer's wife.

The work before us has for its aim the double end we have referred to, and might have borne as the motto on its title-page the curt inscription of the old tavern-signs, "Entertainment for Man and Beast."

The author is an accomplished veterinary surgeon, who has enjoyed the very best opportunities of studying and practising his profession. His general education has been a much more liberal one than the majority of those following his important vocation have the opportunity of obtaining; and the good use he has made of his advantages appears from the high character of the essays which he has published on different veterinary subjects, and the prizes which have been awarded him by the highest authorities in his profession, both in England and Scotland.

This work will extend his reputation. The style is admirably clear, emphatic, and graceful, but without needless display of literary skill, so that the book is very pleasant reading. The latest chemical, physiological, and pathological researches, which bear upon veterinary practice, are turned to account by the author, and he records several important original inquiries of his own into the actions of medicines on the lower animals. As a treatise on Veterinary Materia Medica, the work has no equal in our language, and as such we recommend it to the members of the profession to which it is addressed.

It is also, however, worthy of a place in every medical man's library, and especially in that of the country practitioner, who has too often been compelled to treat his faithful horse, his bountiful cow, or his friendly dog, when suffering from such ills as each is heir to, only by an uncertain application of the rules of his own calling, upon the half-true, half-false principle, embodied in the homely proverb, that "what is good for the goose, is good for the gander."

The work, we may further add, has the merit of being a dispensatory or pharmacopeia of animal medicines, and as such deserves a place in the book-shelves of every druggist's shop.

ART. XI.—*A Treatise on Hooping-Cough; its Complications, Pathology, and Terminations; with its Successful Treatment by a New Remedy.* By GEORGE D. GIBB, M.D., late Lecturer on Institutes of Medicine in Montreal, &c. London. 1854, 12mo., pp. 393.

The title of this work did not prepossess us in its favour, especially when we discovered that the "*new* remedy" of the author was *nitric acid*, dropped into *eau sucrée*, and used as a drink! and that its introduction, on the author's own shewing, was not due to himself, but to his "friend and former colleague," Dr Arnoldi of Montreal. It appears that "Dr Arnoldi has treated upwards of one hundred cases of pertussis with nitric acid with the most satisfactory results;" and in Dr Gibb's hands, sixty-seven cases have been cured "at intervals varying from two to fifteen days; but averaging between six and seven days." In writing a monograph on hooping-cough, the author must have found many remedies, in favour of which a *prima facie* case at least equal to this has been established. If his primary object had been to invoke the assistance of the medical profession in establishing this remedy as a useful one, it was possible to do so in the present day by means of journals and societies, without publishing an entire "Treatise on Hooping-Cough," &c., &c. If, on the other hand, he felt moved to lay before the public the results of his experience on the general subject, the little particular remedy might have been allowed to make its way among other novel considerations, without being specially set forth in the title-page, and thereby connected with the name of Dr Gibb in the advertising columns of the *Times*.

These points of form and propriety are of great importance to the welfare of the medical republic; and Dr Gibb must therefore excuse us, if, in directing attention to them, we may seem to bear hard upon what may have been merely an error of judgment on his part. The struggle after notoriety in the way of publication has made it impossible to treat medical books as mere literary and scientific enterprises; and we shall not fail in our duty, when it requires us also to wield the rod of strictly professional criticism. For the rest, we have no objection to Dr Arnoldi's remedy, and think it worth a trial, though not much commended to notice by the theoretical opinions of either of its advocates. The rest of the work of Dr Gibb displays much diligence in the accumulation of opinions and authorities; and, though neither closely reasoned nor rich in original thought, will be of use to the student of the disease of which it treats.

ART. XII.—*The Elements of Natural Philosophy, or an Introduction to the Study of the Physical Sciences.* By GOLDING BIRD, M.A., M.D., F.R.S., &c.; and CHARLES BROOKE, M.A., M.B., F.R.S., &c. Fourth Edition, 12mo., pp. 626. London, 1854.

This is a very much improved edition of a work of established credit. It is brief and comprehensive. It exhibits the newest discoveries in the several departments of Physics of which it treats. It well deserves the patronage of the medical profession. We can extract no more than the following short passage on the principle of the stereoscope:—

"If an image of a picture is formed in a common camera-obscura, it matters not in what position the instrument is held with regard to the picture, the relative position of all its points will be the same in the image; but when a solid object, as a house or a tree, is depicted in the camera, this is notoriously not the case, but the relative position of the several points of the image will vary with the position of the instrument, just as we see the object under a different aspect whenever we change our point of view; that is, a different picture is painted on the retina with every different position of the eye. Consequently, two slightly different pictures of any object in relief will be simultaneously impressed on the two eyes; the amount of difference of the two pictures depending upon the amount of relief or relative distance of different parts of the object from the eye. That this difference of the two pictures actually exists, we may convince ourselves by a very simple experiment: place a lighted candle about three feet in front of the face, and hold up the fore-finger between the candle and the nose, the finger and candle appear on the contrary sides of each other when viewed by the two eyes separately. Our knowledge of the principles of *binocular vision*, that is, of the mental impression derived from the combination of two simultaneous visual images, is due to the ingenuity of Professor Wheatstone, who first directed the attention of physiologists to the fact, that the mind derives the perception of relief or solidity from the combination or superposition of two dissimilar visual images, simultaneously depicted on the two retinæ; and this he has proved to be the case by shewing that if two pictures of an object be taken in the direction in which it would be viewed by the two eyes separately (for which purpose none can be so perfect as photographs), and these pictures be so presented to the two eyes, that their images may fall on corresponding portions of the retinæ, the mind of the spectator derives from them a surprising and irresistible impression of the actual solidity or relief of the combined picture."—(P. 559.)

ART. XIII.—*A Short Exposition of the Circulation and Nervous System with reference to Disease and Treatment.* By G. HAMILTON BELL, F.R.C.S.E., formerly of the Madras Medical Establishment. 8vo. pp. 67. Edinburgh, 1854.

We recommend this little work of Mr Hamilton Bell to our readers as containing interesting matter, both speculative and practical. We dare not say that we subscribe to the views here inculcated—but we are free to confess that we have perused the work with much interest. Many of our readers must be aware of the singular success which, in Mr Bell's hands, has attended the free use of the Tincture of the Muriate of Iron in Erysipelas. His success, together with that of his brother Dr Charles Bell, in the employment of this remedy is a most striking and important fact; yet with our ideas of the wayward character of erysipelas, we are not yet prepared to expect that it will become one of universal application in the treatment of that disease. One of the objects of the book before us is to point out certain theoretical grounds on which the muriate of iron should be of service in erysipelas. Mr Bell's theoretical views as laid down in this volume extend, however, considerably beyond that mark. They turn on the arterial circulation; on the venous circulation; on the nervous system with reference to pathology and therapeutics, and on the portal venous system. And the diseases to which these views are applied, besides erysipelas, are principally fever, continued and intermittent, and cholera. This notice is, indeed, short—but the book itself is not longer than some of our Reviews.

ART. XIV.—*Surgical Anatomy.* By JOSEPH MACLISE, Fellow of the Royal College of Surgeons of England. Second edition. London.

With the high merits of this work most of our readers are no doubt already acquainted. We are informed that the first edition of 1000 copies was exhausted in a few months after its completion, and are now presented with a second issue of the work. We recommend Mr Maclise's work as a most desirable one to all engaged in practice. Its design is to render anatomy useful in its bearing on all departments of practice, medical as well as surgical; and, in this design, Mr Maclise has succeeded in a manner for which no terms of praise could be too high, whether applied to the anatomical or artistical elements, which are so admirably blended in the plates. The senior student, also, will do well to possess himself of this work, and to follow its plans in making his dissections for surgical anatomy. We speak of the plates, in which the value of the work lies; of the accompanying commentary we feel disposed to speak at greater length than our present space permits, but may return to this when the edition is completed, as the first four fasciculi only have as yet reached us. Meanwhile, we take the opportunity of most heartily recommending Mr Maclise's work to all who desire to possess beautiful and valuable views of medical and surgical anatomy; and must add the farther recommendation, that the price is so very moderate as to place it within the reach of all.

ART. XV.—*Hand-Book of Chemistry, Theoretical, Practical, and Technical.* By F. A. ABEL, and C. L. BLOXAM. 8vo. pp. 724. London, 1854.

This Hand-Book of Chemistry has evidently been prepared with much labour. It contains a great deal of practical information. It is by no means designed to exhibit a merely speculative view of the science of chemistry. The authors manifestly have no idea of any other kind of chemistry but that which is studied amidst the hissing of furnaces, the bubbling of retorts, and the odoriferous fumes of gases and vapours. Its details are characterised by general accuracy. The work does not extend to organic chemistry.

PART III.

PROGRESS OF MEDICAL SCIENCE.

I. PATHOLOGY AND PRACTICE OF PHYSIC.

M. A. Lereboullet upon the Intimate Structure of the Liver and the Nature of the Alteration known under the name of Fatty Liver. Portal Prize Essay.

M. Lereboullet, in a monograph upon the above subjects, after an extended and very lucid explanation of his views of the structure of the liver, as derived from its examination in man, and the different orders of animals, both in the adult and fœtal condition, and from micrometric measurements of the constituents of the lobules in injected preparations from the human subject, sums up the results of his investigations in the following general conclusions:—

The organs which prepare the biliary fluid in all animals are cells; that is, hollow organic elements analogous to the utricles of vegetables.

The liver of the molluscs and of the crustacea, as the craw-fish, contain two kinds of cells, biliary and fatty.

These two kinds of cells multiply by endogenous generation. The fatty cells appear to be only transitory, and are transformed into biliary cells by the deposition of biliary granules within them, and the disappearance of the oily particles which they contained.

The liver of the vertebrate animals is composed of lobules, that is, of small aggregations of the secretory elements, which are so grouped as to form masses of variable dimensions, seldom exceeding two millimetres, or rather less than a line in diameter. These lobules are frequently more or less blended with each other. They are most readily distinguished in the liver of the hog, in which they are surrounded by a special capsule, in direct continuation with the capsule of Glisson. In the human liver the lobules are always more or less confounded with each other.

The two colours of the liver, which render visible its division into lobules, are not due to the existence of two distinct substances, nor to the greater or less accumulation of bile in the ducts, but depend solely upon the relative degree of repletion of the perilobular portal vessels, and of the hepatic veins which occupy the centre of the lobules.

When the stasis of the blood occurs in the portal veins, the periphery of the lobule is darker than the centre; the contrary state obtains when the portal veins are empty, while the central ones are filled with blood, as is seen in many pathological states, particularly in the fatty liver.

In the liver of the hog there exists, around each lobule, a true cellular envelope, which is easily demonstrated, and which separates the lobules perfectly from one another. The fibrous elements of this envelope are continuous with the cellular tissue surrounding the vessels, (capsule of Glisson). In man no trace of this lobular envelope can be demonstrated.

The hepatic lobule is, in itself, a small liver, composed of secretory cells, a capillary rete of afferent, and another of efferent, bloodvessels.

The secretory or biliary cells of the vertebrate animals are, like those of the invertebrata, true utricles.

The opposite walls of these closed sacs are more or less pressed together, but may be distended and rendered ovoid by treating them with chloroform. The examination of fatty cells also shews that the oil is developed in their interior, and distends their walls.

These cells habitually contain, 1st, A spherical nucleus, with a variable number of transparent, punctiform nucleoli. 2d, Gray or fawn-colored granules, dispersed through the cell or accumulated in minute masses (biliary granules). 3d, Very small oil globules, scattered among the preceding.

The existence of these several elements in the interior of the cells is not constant. The nucleus is occasionally wanting, the biliary granules are not always aggregated, and the fatty vesicles are not always distinct.

The dimensions of the nucleus are nearly constant, yet I have frequently found nuclei much larger than ordinary, and which may be regarded as included cells.

Cells containing two nuclei of equal size are sometimes met with; this circumstance, rare in healthy livers, appears more commonly in certain cases of disease of this viscus.

Although I have observed some endogenous cells in the human liver, I cannot affirm that this sort of cells exists in the normal state. They are, at least, always very rare in man and the mammalia as well as in birds.

Endogenous cells exist positively in reptiles (frogs and salamanders), and in fish. In the liver of fish only, have I found fatty cells distinct from the biliary cells; yet the oil globules contained in these cells were small and few in number.

In the liver of the fœtus of mammals there exist two kinds of cells; fatty cells in great numbers, and endogenous biliary cells, always much smaller than the preceding.

The fatty cells which composed almost the entire liver of the fœtus of a rabbit of fifteen days, were filled with oil globules of uniform size. In a human fœtus at term, I could no longer find special fatty cells, but found some endogenous biliary cells yet existing.

The predominance of fatty cells in the liver of the fœtus before term, and their existence in the livers of fish and in those of the invertebrata, confirm me in the opinion announced above, that the fatty cells are the first state of the biliary cells.

The great number of endogenous cells, whether fatty or biliary, in the inferior animals and in the fœtus, and the rarity of these cells in the superior vertebrata, authorize us to regard the biliary cells of these latter as having attained the full extent of their development.

The biliary cells are joined by their ends so as to form longitudinal series which converge towards the centre of the lobule. These longitudinal series are united by shorter transverse ones, so as to represent a network with meshes, polygonal or rounded at the periphery of the lobule, and elongated towards its central part.

Each thread of this network is double, that is, formed by two ranges of cells, which touch at their sides and leave only a linear interval between them. But these two ranges of cells are only in juxtaposition, separating easily by the slightest traction.

The cells which constitute the series are, on the contrary, adherent to each other; hence we frequently see simple series of cells yet adherent, after tearing a minute piece of the substance of the liver.

These series or chains of cells do not form tubes, as was supposed by E. H. Weber; the cells which compose them do not open into each other, but are, on the contrary, perfectly distinct and independent.

The network formed by the double ranges of cells pervades the whole thickness of the lobule, from the perilobular vessels to the central one. Hence it is inaccurate to speak of the secretion as taking place exclusively at the periphery of the lobule. The meshes of the network of cells are filled by the bloodvessels of the lobule.

The double threads of the biliary network are probably surrounded by a proper membrane, which would constitute the basement membrane of these secretory tubes, but this is so adherent to the walls of the bloodvessels, as to render it impossible to prepare and demonstrate it, in such a manner as to shew that the included biliary cells are only epithelial. Therefore, in the natural state, these secretory tubes within the lobules would be full, that is entirely occupied by the secretory cells, and hence their cavity is simply linear.

When we succeed in throwing an injection into these biliary passages, the injected matter distends the linear intervals just described, compresses the cells, and gives the appearance of a rete of capillary ducts, which takes the place of the network formed by the double ranges of cells.

The capillary biliary ducts of authors are then produced mechanically by the injection. These canaliculi have indeed no proper walls, the injected matter being in immediate contact with the secretory cells.

The rest of the lobule is occupied by a vascular rete, formed by the portal vein and the radicles of the hepatic vein. The meshes of this network adapt themselves exactly to the threads of the biliary rete, and vice versa, so that the two are closely interlaced. The mean diameter of the threads forming the meshes, and of the meshes themselves in either network is ·015 of a millimetre.

The threads of the bloodvessel net-work of the lobule are tubes with definite walls, and not simply channels. The existence of the walls of these vessels can be demonstrated, and their structure studied. The portal net-work occupies the periphery of the lobule; it is formed by small vessels which are given off, at short intervals, from the perilobular veins, and which immediately become capillaries. The meshes of this net-work are polygonal.

The net-work of the hepatic vein fills the central half of the lobule; its meshes are elongated, and terminate in the central vein, or intralobular vein of Kiernan.

The biliary secretion does not then take place in a circumscribed portion of the lobule, as some authors have stated; at the periphery, according to some, and at the central portion according to others, but in every part of its thickness, since the secretory cells exist in all parts of it, and are supplied at every point by bloodvessels.

Every lobule has its axis traversed by a vein, the central vein, which either terminates in a cæcal extremity, or divides into several diverging branches. These central veins either unite to form a branch of the hepatic vein, or open directly into one of its branches upon which the lobule is placed.

Upon slitting open an hepatic vein, the orifices of these central veins may be seen by the naked eye, or by the aid of a lens, generally placed at the centre of the lobule, whose outline may be distinguished through the walls of the vessels.

The biliary canals which leave the lobules are always multiple. They arise from all points of the surface of the lobule, and after having united with each other many times, like the roots of a tree, leave the lobule, and form one or more ducts, which, with the corresponding trunks of the portal vein and hepatic artery, are surrounded by a fibrous sheath, the capsule of Glisson.

The portal vein, after subdividing in the capsule of Glisson, furnishes ramuscles which encircle the lobules, but never form a perfect and complete vascular sheath around any one lobule, since each lobule receives numerous branches from the neighbouring portal veins; and it is from he reunion of all these perilobular branches, that the more or less complete vascular circle is formed around each lobule, and from which the lobular portal rete takes its origin.

The hepatic artery, which everywhere accompanies the portal vein, does not concur in the formation of the lobule. Its ramifications are expended upon the walls of the vessels and in the capsule of Glisson.

They have a particularly abundant capillary distribution in the subperitoneal cellular tissue at the surface of the liver. This network, which the hepatic artery forms at the surface of the liver, does not differ from the rete of the subjacent portal vein. They have the same dimensions, are continuous with each other, and thus really constitute but one.

The blood of the hepatic artery does not appear to concur in the formation of the bile, or, at least, the part which it plays in this secretion is very secondary.

The walls of the hepatic duct, ductus choledochus, cystic duct and gall-bladder, are lined by closed ovoid follicles, which form by their union small granular sacs, lying upon the exterior of the ducts, and furnished with an excretory canal which opens into the latter.

The closed follicles in these granular sacs are lined on their interior by an epithelial formed of small granular spherical cells.

(*Translated in Philadelphia Medical Examiner*, April 1854.)

Alterations in the Circular Measurement of the Thorax in Acute Diseases.

We think it right to notice the following observation, contained in a long memoir by M. Woillez in the *Archives Générales de Médecine* (April and May 1854), because if found to be a correct generalization, it may possibly lead to some more precise and valuable results than those which the author has deduced from it. It appears, according to M. Woillez, that " in the most various acute diseases there occurs," as compared with health, " a general enlargement of the thorax, not to be appreciated by the eye, and accompanied by a diminished elasticity of the chest, and that both these conditions may be determined by mensuration." The method used by M. Woillez in making this estimate is the ordinary measuring tape applied first on the one side and then on the other. The sum of the two results represents the " general circular capacity " of the thorax, and the author finds that the increase amounts on an average to about 4 centimetres (1⅗ inches), and sometimes to double this amount. The retrocession is sometimes rapid; it has been observed to amount to 6 centimetres (more than 2 inches) in four days.

The elasticity of the thoracic parietes is estimated by first applying the tape with as little pressure as possible on either side, then stretching it to the utmost, and observing the difference of the two measurements. The sum of these lateral differences gives the " degree of general elasticity," which amounts to 6½ or 7 centimetres (2½ to 3 inches) on an average. This general elasticity of the thorax undergoes, according to M. Woillez, a diminution in acute diseases, precisely contemporaneous with the increase of capacity formerly adverted to. Its range in disease has been found to be from about 4½ to 1⅗ inches.

As these two orders of phenomena are found to be frequently present when some of the ordinary symptoms or signs of thoracic disease exist, the author considers that they must be due to a " congestion of the lung," often, but not necessarily, connected with the condition of general fever. He attributes considerable importance to this pathological discovery, of which the method by mensuration is the only sure sign.

We have too much respect for M. Woillez, as an observer, to pass over this memoir without recommending it to the consideration of our readers; at the same time we cannot avoid indicating that, in our own hands, the measuring tape has not appeared to be capable of affording very trustworthy results within the narrow limits of difference pointed out by the author. The conclusions may be correct and important, but we should like to see them reaffirmed on more secure grounds; and we demur most emphatically to the inference from them as to congestion of the lung, which appears to us by no means to follow from the premises stated. Why should congestion of the lung be supposed to increase its volume, seeing that, as has been proved on the best authority, even hepatization does not do so to any appreciable extent? And why should congestion of the lung be presumed to exist in the absence of symptoms, rather than any other morbid condition? We must take the liberty of considering these statements as still very incomplete, and requiring in all points careful reconsideration.

On Chronic Hydrocephalus, and the Statistics of cases in which paracentesis was performed.

In the *New York Journal of Medicine* for May 1854, Dr George Blackman gives an interesting sketch of what is known upon this subject, founded on the materials collected by West,[1] Battersby,[2] and others, including himself. One remarkable original case is recorded. A child aged six months, well-formed and well-nourished, with the exception of the cranium and its contents, became subject to hydrocephalus very soon after birth. The head enlarged till the fourth month of life, when it suddenly decreased, the entire body becoming at the same time anasarcous. In a few days the anasarca again disappeared, the head resuming its extraordinary volume, and when placed under treatment was 30 inches in circumference. It was transparent when held between the eye and the light, and presented at its interior and posterior portions several bag-like prominences, which appeared as if about to burst from the pressure of fluid. Dr Blackman thought it right to recommend tapping, though without any hope of a cure; this was done on two different occasions, but the child died 12 days after the first tapping. After death the hemispheres of the brain were found almost entirely destroyed. The pons Varolii and medulla oblongata, however, were natural. Notwithstanding this remarkable extent of disorganization, the patient was able to see, hear, taste, and feel. [*Qu.* How long before death?

Several cases are referred to in which similarly extensive destruction of the cerebral substance took place.[3]

Frank, Gölis, Nelaton, and others have noticed the improvement or disappearance of hydrocephalic symptoms after various artificial or natural discharges or eruptions. Dr Hubbell has recorded a case terminating spontaneously in recovery after the appearance of an eruption on the scalp. The diminution in size of the head extended over a period of three months, while the eruption receded, and the child remained well.[4]

Dr Blackman next treats of the differential diagnosis between hydrocephalus and hypertrophy of the brain. The differences insisted on by Mauthner (Brit. and For. Med. Chir. Review, April 1846) are not suffisient for the diagnosis, nor are they always applicable. The skull in hypertrophy of the brain presents a hard unyielding case; in hydrocephalus it yields to pressure. On the whole, however, the diagnosis is sometimes difficult, and the more so, as the affections sometimes coexist.

---

[1] Medical Gazette, April 1842.

[2] Edinburgh Med. and Surg. Journal, July 1850.

[3] Dr Glover, Philadelphia, Journal of Med. and Phys. Sciences, Vol. 2, p. 159.
Dr Delafield, American Med. Recorder, July 1821.
Dr Kemmett, Edinburgh Med. Commentaries, Vol. vi., part 4.
Dr Baster, Phil. Transactions, Vol. 42.

[4] New York Journal of Medicine, May 1850.

In referring to Sir Walter Scott and Dean Swift, as cases of hypertrophy of the brain and hydrocephalus, Dr Blackman appears to us to have made too much of very scanty evidence.

Tapping the cranium for hydrocephalus was proposed by Hippocrates, and sanctioned by several of the Greek physicians. (Dr Blackman erroneously ascribes the first proposal of this operation to the Rev. Dr Stevens, in the case of Dean Swift). Most of the older surgeons since the revival of letters condemned the operation; and Petit asserts that none had lived after it more than 40 hours. This assertion is not borne out by modern experience. In Professor Rossi's case, quoted by Monro,[1] six lb. of water drained away in 20 days, the patient is alleged to have been "saved." In Mr Lizars' case, the patient lived 90 days after the first puncture, and was then going on favourably. Eighteen punctures were made in this time, and 76 ounces were drawn off. The symptoms after the punctures were never serious.[2] In Mr Russell's case the head was punctured four times, 19 ounces of fluid were removed, and the patient was greatly improved four months afterwards.[3] In a case recorded by Graefe,[4] there was great faintness after each operation, but the child lived in health at least 17 months; and two other instances of a similar temporary success are noticed (but with insufficient details) by Foucade[5] and Marsh.[6]

In several of Dr Conquest's cases, periods of from two and a half to five years elapsed and the children continued healthy, although considerable quantities of fluid had drained off. On the other hand, it is certain that a very large number of children only survive the puncture by a certain number of days or hours, and then die either of direct meningeal irritation, or from the reaccumulation of fluid and progress of the disease.

As might be expected under these circumstances, the opinions of modern authorities are at variance as to the eligibility of the operation. Dr West contends that there is no sufficient evidence of real success in the so-called successful cases, inasmuch as the disease is naturally of slow or intermittent progress, and the patients might have lived, without the operation, as long as they did after it. Gölis, Richter, Battersby, Dieffenbach, concur in condemning the operation. On the other hand, Malgaigne, Copland, Joy, Fergusson, give it a place among the legitimate means of art; and Dr Watson considers that it furnishes one of the best of the few chances of safety to the patient. It seems evident that, while only a small minority of lives will be saved by it, it may be cautiously performed in cases which resist other treatment, with some hope of relief to the oppressed organ, and without any considerable danger as regards its immediate effects.

On some of the Compounds of Urea, and a New Method for Determining Common Salt and Urea in Urine. By J. Liebig.

Quantitative Determination of Chlorides in Urine.—When the chlorine compounds of the alkalies are mixed with the pernitrate of mercury, these salts are decomposed into bichloride of mercury and into a nitrate of the alkaline base.

---

[1] On Hydrocephalus, p. 146.
[2] Edin. Med. and Surg. Journal, Vol. 17, p. 243.
[3] Ibid, Vol. 38, p. 43.
[4] Graefe and Wather's Journal, Bk. xv., 348.
[5] Lancette Francaise, Vol. 4, p. 188.
[6] Med. Gazette, Vol. 17, p. 985.

The pernitrate of mercury immediately produces in a solution of urea, a thick white precipitate, which does not occur with a solution of bichloride of mercury.

Upon these two facts depends the method for the quantitative determination of chlorine in a fluid containing urea and chlorides. If, for instance, we mix a solution of urea and chloride of sodium, and add slowly, in small portions, a weak solution of pernitrate of mercury, a white cloudiness will be produced, which immediately disappears upon agitation, the liquid remaining as clear and transparent as before; without the chloride of sodium, however, the solution would have remained permanently cloudy. If we possess, therefore, a solution of pernitrate of mercury, graduated and determined by means of chloride of sodium, we can, by its assistance, attain, in a very short time, the amount of chlorine contained in urine. This graduation is done in the following manner:—

Liebig confirms the fact, that, when pure transparent rock-salt, in large pieces, is covered with water and allowed to remain, with frequent agitation, for 24 hours, at a temperature of 12° to 24° C. (53·6° to 75·2° F.) the liquid will have dissolved an invariable amount. By evaporating 10 C.C. (cubic centimètres)[1] of this solution, he obtained (as Fuchs and Fehling did also) 3·184 grms. of chloride of sodium. If we now carefully measure out, by means of a pipette, 20 C.C. of such a solution (not adding the drops that may adhere to the point of the pipette) and mix it with 298·4 C.C. of water, then we have 318·4 C.C. of a diluted solution, containing therein 2 × 3184 milligrammes of chloride of sodium. In 10 C.C. of this solution are, therefore, contained 200 milligrammes of chloride of sodium. We then measure, by means of a small pipette, containing, when filled to a mark on the tube, exactly 10 C.C. of the above-mentioned solution, and let it flow into a small beaker glass, then add 3 C.C. of a solution of urea, which, in 100 C.C., contains 4 grammes of urea, therefore, in 1 C.C., contains 40 milligrammes of urea. The diluted mercurial solution (the strength of which is to be determined,) is poured into a burette, the height marked at which it stands, and poured drop by drop into the mixed solution of urea and chloride of sodium, which is kept in a state of rotary agitation; as soon as a decided permanent precipitate appears, the test is completed.[2]

If with 10 C.C. of a solution of chloride of sodium, 7·8 C.C. of a solution of mercury is required for the production of a precipitate, the solution is too concentrated to admit of a correct valuation; it must be diluted with an equal volume of water, and the experiment repeated. If, for instance, 15·5 C.C. of a mercurial solution is required for the production of a cloudiness in 10 C.C. of a solution of urea and chloride of sodium, then to every

155 volumes of this mercurial solution
45 " of water must be added, by which
———

---

[1] The cubic centimètre is 0·06103 English cubic inches; the gramme is 15·4336 grains. As the very accurate foreign burettes, (See Graham's Chemistry, vol. i., p. 551,) are graduated in cub. centimètres, and the French decimal system is likewise convenient for calculation, these measurements are left here unchanged.

[2] No regard need be paid to a mere opalescence of the fluid, as this depends upon the presence of foreign metals, and can easily be recognised as not belonging to the test, since, by the addition of a few drops more of the mercurial solution, the cloudiness does not increase. If the precipitate arises from the compounds of urea, this is not the case, for one drop more of the solution of mercury produces in this case an increased cloudiness.

we attain 200 volumes of mercurial solution, of which each 20 C.C. indicates exactly 200 milligrammes, or 1 C.C., 10 milligrammes of chloride of sodium. By means of a check experiment, the exactness of this measurement can be proved. In mixing 20 C.C. of the test liquid with 10 C.C. of a solution of urea and chloride of sodium, the degree of cloudiness, which is permanently obtained, must be kept in mind. For in the application of this test fluid to the quantitative determination of chloride of sodium, if more or less mercurial solution be added in excess, whereby the cloudiness becomes stronger or weaker, a source of error arises which can easily be avoided by a little practice.[1]

To determine now the amount of chlorides in urine, it is necessary, first, to remove the phosphoric acid contained therein. Liebig found that a mixture of one volume of a cold saturated solution of nitrate of baryta with two volumes of cold saturated aqua barytæ, was suitable to effect this. We add one or two volumes of this mixture to the urine that is to be examined; the liquid is then filtered from the arising precipitate; it is alkaline from the excess of baryta; this alkaline reaction must be removed by means of nitric acid, though care must be taken to add no more nitric acid than is necessary to restore a slight acid reaction.[2]

For this experiment, then, we must measure in a small pipette, exactly holding the amount, 15 C.C. of this fluid, corresponding to 10 C.Ct of urine, pour it into a small beaker glass and mix it, with constant stirring, with the mercurial solution. After the cloudiness appears, the amount of test liquid used can be ascertained from the burette; every C.C. used represents 10 milligrammes of chloride of sodium. Comparative experiment proved that the results obtained in the quantitative determination of chlorine by the above-described method, were as exact as those by the use of nitrate of silver.

---

[1] The above-described test fluid is intended for those cases in which no foreign salts and no excess of urea are present in the solution; but if employed, for the determination of chloride of sodium in urine, there arises a slight error which makes the amount contained in that fluid appear less than it really is. This error arises from the cloudiness appearing rather earlier, when foreign salt and much urea are present, as in such fluids the precipitate is rather more difficult of solution. A deposit of nitrate of mercury and urea is not produced in the fluid until this is saturated with it; the mercurial solution always contains free nitric acid, which dissolves more of it than water, and this more than a solution of nitrate of urea. Since urine generally contains more urea than was added to the chloride of sodium, in the graduation of the mercurial solution, this urea enters into combination with a part of the free nitric aid of the mercurial salt, forming nitrate of urea, by which the solvent power of the fluid for the precipitate is lessened, the latter appearing earlier, so that less of the test fluid is necessary to produce the reaction. This error may be entirely prevented, if to the 10 C.C. of solution of chloride of sodium, which has been mixed with 3 C.C. of a solution of urea, is added 5 C.C. of a cold saturated solution of sulphate of soda, and the test fluid then graduated. Pernitrate of mercury gives with a solution of sulphate of soda, a yellow pulverulent precipitate of turpeth mineral. If the sulphate of soda contains chloride of sodium, the addition of the nitrate of mercury does not produce a precipitate of turpeth mineral until the whole of the chloride of sodium is converted into bichloride of mercury; on this account the experiment is only affected by the addition of sulphate of soda so far that the free acid of the mercurial salt combines with the sulphate of soda to form an acid salt, whereby the same object is attained as by an excess of urea.

[2] It is better to acidify with nitric acid the whole filtrate and not merely the measured out 15 C.C. which is used as a test; a few drops more would not matter in 100 or more C.C., but added to the small amount employed for the determination, would injure the accuracy of the experiment.

Quantitative determination of Urea in Urine.—The method depends upon the precipitation of the urea by pernitrate of mercury. The explanation is as follows: If a diluted solution of pernitrate of mercury is gradually added to a diluted solution of urea, the free acid of the mixture being neutralized, from time to time, by aqua barytæ, or a weak solution of the carbonate of soda, a flaky, somewhat spongy, snow-white precipitate, insoluble in water, will be obtained. If the mercurial salt and carbonate of soda are alternately added, as long as a precipitate is formed, the mixture at a certain period will, upon the addition of carbonate of soda, assume a yellowish colouring, arising from the basic nitrate of mercury. If the fluid be now filtered, it will contain no appreciable amount of urea, for it has all been precipitated. Liebig found that this precipitate always contained one equivalent of urea, and four equivalents of oxide of mercury.

If pernitrate of mercury is added to a solution of urea, as long as a precipitate is formed, then the mixture, upon the addition of carbonate of soda, remains *white;* but if the original mixture be allowed to stand for a few hours, the nature of the precipitate becomes changed, and is then crystalline, and the six-sided leaflets of the combination with three atoms of oxide of mercury are easily recognised, and the clear liquid remaining above the crystals, which was previously precipitated white, now, by the addition of alkalies, gives a yellow precipitate. In the acid liquid, the combination with four atoms of oxide of mercury is therefore transformed into a combination containing less oxide; that is to say, a portion of the oxide enters again into solution. In order to ascertain if the requisite amount of mercurial salt has been added, to produce the compound of urea, with four atoms of oxide of mercury, the solution must be neutralized by carbonate of soda. If a drop of this mixture remains white, when mixed upon a watch-glass with a drop of the solution of carbonate of soda, then it can be safely asserted that free urea is present; but if a yellowish film appears upon the surface of the liquid, then the limit is reached, or rather overstepped; it is only necessary now to add a very slight excess of mercurial salt, to indicate that a sufficient amount has been added, for the entire precipitation of the urea. It is evident, that if the amount of mercury in the mercurial solution be known, the amount of urea contained in the liquid can also be known, from the amount of the solution requisite for the precipitation of the urea. Or, if to precipitate a known amount of urea, say 100 milligrammes, we have found it necessary to use a certain volume of mercurial solution, then in a fluid containing an unknown amount of urea, a like volume of the solution must indicate a corresponding amount of urea. We can calculate the amount of urea from the amount of mercurial solution used; if more or less is requisite, more or less urea is present. After this explanation, the method is easily understood. A solution of pernitrate of mercury must be graduated, with which an exact quantitative determination of urea can be made. Liebig, for this purpose, used a solution, of which 1 C.C. corresponds precisely to 10 milligrammes of urea. This solution must contain as much oxide of mercury as is necessary to form, with 100 milligrammes of urea, the nitric acid combination with 4 equivalents of oxide of mercury, together with a small excess necessary to indicate the precipitation of the urea, ascertained in the manner above stated. Liebig found, that for 100 milligrammes of urea, which, according to the calculation, required 720 milligrammes of oxide of mercury (in the form of the nitrate), 10 C.C. of the mercurial solution must contain 772 milligrammes of oxide of mercury, in order to produce, in diluted fluids, a distinct reaction of the mercurial oxide. Each C.C. of mercurial oxide must, therefore, contain an excess of 5·2 milligrammes of oxide of mercury. The most simple mode of obtaining this fluid is to dissolve pure mercury in pure nitric acid; it must then be warmed, and nitric acid added until no more red fumes arise; evaporate it then in the water bath to a syrup consistence, and finally dilute it with so much water, that in 100 C.C. of the diluted solution will be found precisely 7·140 grammes of mercury. This occurs if we add to 100 grammes of mercury (changed into nitrate of the oxide) so much water, that the volume of the fluid consists of 1400 C.C. On this account, it is, therefore, desirable not to add the whole calculated amount of water at once, but somewhat less. We then measure 10 C.C. of a solution of urea which contains in 200 C.C. four grammes of pure urea, and add from a pipette the approximatively diluted mercurial solution, until a few drops of the mixture upon a watch-glass, gives, with a solution of carbonate of soda, a decidedly yellow colour.[1]

Suppose, for this purpose, we have used 19·25 C.C. of mercurial solution, then for each 192·5 C.C. of this solution, we must add 7·5 C.C. of water, and make a new and final test. If, after the addition of 20 C.C. of mercurial solution to 10 C.C. of the above-described solution of urea, the appearance of the yellow colour is decided, then the mercurial solution can be used for the determination of urea in urine.[2]

For the determination of urea in urine, we must first prepare a mixture of two volumes of aqua barytæ, with one volume of a solution of nitrate of baryta, both saturated cold; and mix one volume of this alkaline fluid with two volumes of urine. For this purpose, use a small glass cylinder of the desired size, which must be first filled *twice* with urine, even to overflowing. The mouth of the cylinder must each time be covered with a glass plate, so that the excess may flow off. The same cylinder must be filled in the same manner, *once* with the solution of baryta, and this poured into the urine in a beaker glass; a precipitate arises from the mixture which must be filtered. From the whole mixture we must measure out for each analysis, 15 C.C., which corresponds to 10 C.C. of urine. Into this volume of urine, before neutralizing it (as is done in the determination of the chlorides), must be poured from a burette, the graduated solution of pernitrate of mercury, continually stirring it, and when no further precipitation (no thickening of the fluid) is perceived, then test it by pouring a few drops with the precipitate from the beaker glass on to a watch-glass, and adding to it a few drops of the solution of carbonate of soda, best done from a caoutchouc pipette. If, after a few minutes, the mixture retains its white colour, then more mercurial solution must be added to the mixture in the beaker glass, until, by testing with carbonate of soda, a decided yellow colour is perceived. Then read off the number of C.C. used, and correct the number obtained according to the contents of urine, in the manner given above. Should the urine contain no chloride of sodium, then everything has been done for the quantitative determination of the urea. A series of experiments have shewn, that if there is from 1–1½ per cent. of chloride of sodium in urine, it exercises, by means of the pernitrate of mercury, an influence upon the determination of the urea. For instance, if we add 20 C.C. of the graduated mercurial solution to 10 C.C. of a solution of pure urea, then carbonate of soda will produce in the mixture a decided yellow colour, of precipitated oxide of mercury. But if we add to the mixture 100–200 milligrms. of chloride of sodium, then try the test, the yellow colour does not appear from the addition of carbonate of soda. To effect this, 1½–2½ C.C. more of mercurial solution must be added. On this account the determination would be given too high by 15 to 25 milligrms. Such is the case also with urine; containing more than two per cent. of chloride of sodium, the error does not increase with the increase of the latter, but remains constant with certain fluctuations.[1]

---

[1] In order to obtain this solution we dissolve four grms. of pure urea in water, and dilute with water until the volume measures exactly 200 C.C.; by dissolving four grms. urea in 200 C.C. of water, we would obtain 201·75 C.C. of fluid, therefore 1·75 C.C. too much.

[2] *Observation.*—This test fluid is graduated by a solution of urea, which contains two per cent. of urea; 15 C.C. of this solution requires for the precipitation of the urea, as well as to indicate its completion, 30 C.C. of mercurial solution; we obtain 45 C.C. of mixture, in the whole of which exists $30 + 5{\cdot}2 =$ 156 milligrms. of oxide of mercury; each C.C. contains about 3·47 milligrms. of oxide of mercury. If the 15 C.C. of the solution of urea contains four per cent. of urea, and 60 C.C. of mercurial solution added, then we have 75 C.C. of mixture, in which are 312 milligrms. of oxide of mercury; in each C.C., 4·16 milligrms., therefore, 0·69 milligrms. of oxide of mercury more than is requisite to produce the original colour. We shall, therefore, be liable to an error, as experiments have shewn, in the analysis of urine, when the amount of urea is increased, whereby the apparent amount of urea will be diminished. In the above stated case we would have added, until the appearance of the original colour, not 60 C.C., but only 59·37 C.C. of mercurial solution. To avoid this error, we must, when experimenting upon 15 C.C. of urine, add to every C.C. above 30 of the mercurial solution that has been used for the precipitation of the urea, half the number of C.C. of water, *before* the test is made with carbonate of soda; if, for instance, we use 20 C.C. more, we must add 10 C.C. more of water. It will always be found necessary, after the addition of water, to add a few drops of mercurial solution, to obtain the indication of a complete precipitation. From the same reason, we must, in those cases where the amount of urea is only one per cent., add for 15 C.C. of urine, not 15 C.C., but 15·3 C.C. of mercurial solution. To avoid this mistake, which increases the amount, we must from each 5 C.C. of mercurial solution less than 30, that have been used, subtract 0·1 C.C. If we, therefore, use 25 C.C. of mercurial solution for 15 C.C. of urine, the amount, 249 milligrms., would be expressed by 24·9 C.C. of solution, &c., &c.

---

[1] In order to explain this peculiarity, it is evident, from the method of determining chloride of sodium by means of pernitrate of mercury, that a solution of urea containing chloride of sodium will not precipitate by nitrate of mercury, until the former is completely converted into bichloride of mercury. In a solution of 200 milligrms. of urea and 100 milligrms. of chloride of sodium in 10 C.C. of water, to which is added 20 C.C. of mercurial solution, the excess of mercurial salt, which should have given the yellow colour upon the addition of carbonate of soda, would not exist in the form of a nitrate, but as a bichloride of mercury, and it is evident that the change of the indication of complete precipitation is caused by the formation and presence of this bichloride. Instead of 3·46 milligrms. of oxide mercury in the form of a nitrate, the mixture contains an equivalent of mercury in the form of bichloride. The reason why the limit of reaction does not extend when the chloride of sodium exceeds two per cent., Liebig explains by the following:—If, to a solution of bichloride of mercury, diluted with water to such a degree that it yields with carbonate of soda a brown yellow precipitate of oxide of mercury, be added a few drops of nitric acid, and then carbonate of soda added to it, it will remain clear, no precipitate forms, or at least only a weak whitish cloudiness, out of which, after long standing, brown yellow leaflets will be deposited. In this condition the excess of bichloride of mercury exists in the mixture of urea with the mercurial solution; it contains the greater part of the nitric acid of the latter in free condition. By this nitric acid, a part of the carbonate of soda will be converted into bicarbonate, which does not precipitate the bichloride. If the mixture, in consequence of a greater amount of chloride of sodium, contains a greater

In a urine, now, that contains from one to one and a-half per cent. of chloride of sodium, we can, without anything further, obtain the exact number of milligrms. of urea in 10 C.C. of urine, by subtracting 2 C.C. from the number of C.C. of mercurial solution used, and also, if the amount of chloride of sodium fluctuates with different individuals, then the difference obtained in the amount of urea is exact, and can be compared with others. In the absolute quantity only is there an error, which, uncorrected, would amount to 15–20 milligrms. in 10 C.C. of urine.

In a determination, in which the absolute quantity of urea is sought for, the chloride must be removed, and the chloride of sodium be converted into nitrate of soda. This takes place by precipitation through nitrate of silver. For this purpose, we must prepare a silver solution from 11·601 grms. of melted nitrate of silver, dissolve it in water, and dilute it so that the volume of solution consists of 400 C.C. 1 C.C. contains 29·01 milligrms. of nitrate of oxide of silver, corresponding to 10 milligrms. of chloride of sodium. The mercurial solution (the preparation of which, for determining chloride of sodium, has been given above) corresponds with this solution; both exhibit the same amount of chloride of sodium. If we, accordingly, add to 10 C.C. of urine 12·5 C.C. of mercurial solution until the appearance of the cloudiness; in a similar volume of urine, by adding 12·5 C.C. of solution of silver, the chloride will be completely precipitated, and no silver remain in solution.

Suppose we have used to 15 C.C. of urine, precipitated by solution of baryta (containing 10 C.C. of urine), 17·5 C.C. of mercurial solution, then measure with the pipette,

30 C.C. of the same urine, and add
35 " of silver solution.
—
65

Now filter, and take from the fluid filtered, for the test, half the amount of the mixed fluid—therefore, 32·5 C.C.—in which exists 10 C.C. of urine. This is now mixed with the graduated mercurial solution, and the amount of urea obtained in the manner described, regard being paid to the dilution in consequence of the silver solution that has been added.—*Annalen der Chemie und Pharmacie*, 85. *Bd.* 3, *Heft. page* 289. 1853. *Translated in Philadelphia Medical Examiner*, April 1854.

On Erythema Nodosum. By M. SHANAHAN.

The author has written his inaugural dissertation for the Faculty of Medicine at Paris upon this subject; while he has particularly directed his attention to the following proposition:—The nodose erythema is not a variety of erythema, and not a simple cutaneous affection, but it is the manifestation or the consequence either of acute rheumatism, or of the rheumatic diathesis. The accuracy of this observation, which is due originally to Dr Begbie, is also borne testimony to by M. Legroux, physician to the hospital, Beaujou, in whose *service* M. Shanahan had the opportunity of witnessing the progress of two interesting illustrative cases.—*Archives Générales de Médecine, June* 1854.

Enumeration of instances, taken accidentally, of " Fatty condition" of the Heart in various Diseases.

The number of hearts examined was 143, and out of this number 100

---

amount of bichloride of mercury, then the free carbonic acid is not sufficient to prevent the precipitation of the oxide of mercury; there arises, therefore, a brownish yellow precipitate.

were in a more or less so-called "fatty condition," the muscular fibres presenting, when examined under the microscope, more or less confusion or absence of their natural striated condition; a black, dotted appearance having been assumed, or having distinctly-refracting rounded and yellow particles in their substance, arranged for the most part longitudinally down the centre. The presence of fatty matter between the muscular fibres, the "fatty growth," in opposition to the "fatty change," has not entered into consideration here. Out of the 143 cases examined as to the hearts, there were 97 with "fatty livers;" and out of the 100 fatty hearts there were 68 that were associated with a more or less fatty state of the liver; the other fatty livers occurring unassociated with fatty hearts. Moreover, it by no means invariably happened, that the most decidedly fatty hearts were allied with the most decidedly fatty livers, but this was generally the case.

The following is the list of diseases with which a fatty heart was found; but of course several cases in this list are enumerated more than once, as when, for instance, a patient had at the same time diseased heart and diseased liver. The majority of the patients had been suffering from disease for some months, but several had been ill only for a few weeks, and some had not been the subject of illness of any standing.

	Cases.
Tuberculosis	21
Diseased Heart	31
„ Kidney	38
„ Liver	3
Fever (Typhoid)	3
Pneumonia	8
Peritonitis	7
Carcinomatous Glands	2
Brain and Spinal Cord, including extravasation of blood into their textures	7
Ditto with a fibrinous diathesis	13
Erysipelas	1
Perforation of Stomach	1
Poisoning by Oil of Almonds	1
Burns	2
Gangrene and Atheromatous Arteries	1
Delirium Tremens	1
Aneurismal Varix of large vessels of Heart	1
Operations or accidents followed by Bronchitis or Delirium Tremens, and with or without the so-called secondary inflammations	15

With regard to the fatty conditions of the hearts, I have in my own tables considered them as either "slightly fatty," "decidedly fatty," or "very fatty," but in the present enumeration I have included them all under one denomination, only referring, as I shall presently do, to the "very fatty" and to the "slightly fatty" ones, in relation to the people in whom they were found. In speaking as above of the diseased livers and kidneys, I have not included those instances in which these viscera were diseased by such specific deposits as the carcinomatous or scrofulous; or again, those which were simply enlarged by extreme congestion: among the diseased hearts I have included indiscriminately hypertrophoid and dilated ones, but not such as were affected simply as to their valves, except when further mischief to the heart had resulted. Again, in speaking of tuberculosis, I have not included the old cretaceous or obsolete deposits, but only those cases in which the specific tendency was obviously in an active state. Lastly, when speaking of a "fibrinous diathesis," as I have termed it, I had reference to those cases in which there manifestly existed a proneness to the precipitation of the fibrinous element of the blood into various organs and cavities, and on to various free surfaces within the body; forming what is ordinarily called a "low inflammation" of the affected part. Under the supposition, which I believe to be tenable, that the atheromatous condition of the arterial system depends on this same tendency to fibrinous precipitation, this substance becoming subsequently converted into either a fatty or a calcareous one, I might well have been expected to have observed more intimately the condition of the cardiac, pulmonary, and cerebral arteries. This has, however, not been so frequently and so closely done as to warrant any decided statements on this point, but still I have several cases in which, on the one hand, so much atheromatous deposit existed in the coronary arteries of the heart as to make them firm, large, and brittle; but yet the heart has been in some instances only moderately, and in others very slightly, affected. One case in particular was worthy of notice, in which, although the coronary arteries were intensely atheromatous, calcareous, and fatty, yet the muscular fibres of the heart were only very slightly altered, but the entire organ was greatly atrophied, generally weighing but seven ounces, and that in a person fifty-seven years of age. Many of the hearts that were found to be fatty, and some that were exceedingly fatty, were firm and resisting; some were hypertrophied, others were dilated only, and others were both hypertrophied and dilated. Some that were very lacerable, and flaccid, and unfirm, were not very decidedly fatty; hence the observation not unfrequently made, on inspecting a weak and flaccid heart, that "it is certainly fatty," without any further examination, may be incorrect. This flaccid state has appeared to be rather dependent on imbibition, and a fluid state of the blood, as in fever, puerperal peritonitis, pyæmia, &c. Out of the 100 fatty hearts examined, I find that 23 of them are marked as being only very slightly fatty, and the average ages of the patients in whom they existed was forty years; whereas out of nine which are marked as having most decidedly fatty hearts the average age was only thirty-six. Of the 23 with slightly fatty hearts one patient was the age of sixty-one; two were of the age of sixty-one; two were of the age of sixty-five; and three of the age of fifty years; whereas of those with most extensively fatty hearts the highest age attained was sixty years, and one of the latter class was only of the age of ten years.

Dr J. W. OGLE, *in Transactions of the Pathological Society of London, vol. iv.*

Common Salt in Intermittent Fever. By Dr JOSEPH C. HUTCHISON of Brooklyn, New York.

Common salt was introduced into the treatment of ague in France by M. Montdezert and M. Piorry. It was not countenanced by the Academy of Medicine; nevertheless Dr Hutchison has given it a trial in Missouri, and reports favourably of its effects. His paper contains long details of 22 cases, of which the following is a recapitulation:

Age.—9 were under ten years of age, 6 between twelve and twenty, 4 between twenty and forty, and 1 at forty.

Sex.—7 were males, 12 females, and 1, sex not known.

Race.—16 were white, and 4 black.

Proportion of Cases cured, benefited, &c.—Of the 22 cases reported, in 12, or 54·5 per cent.

In 3 of the cases, or 13·6 per cent., one paroxysm only occurred after the remedy was commenced. It was completely successful, therefore, in

15 cases, 68·2 per cent. In 3 cases the paroxysms were postponed or moderated. One patient vomited after each dose, so that the salt was not retained in sufficient quantity to have produced any marked antiperiodic effect. In 4 cases the remedy was not prescribed a second time, the patients objecting; an increased dose might have arrested the disease. One patient did not take all that was prescribed. In one case only, after fair trial, was there no obvious good effect from the remedy.

Permanency of the Cures.—In three of the patients only, for reasons which have been elsewhere stated, was I enabled to ascertain with any degree of accuracy the permanency of the cures. One patient, three times under treatment, had longer intervals of immunity from the disease each time when checked by the salt, that when quinia had effected the same purpose; and when last heard from, five months had elapsed without a return of the malady. It was said of one patient that the disorder did not return so soon as it had previously done when checked by quinia; and of another, that she had not relapsed twelve months after the paroxysms had been checked by nine drachms of the salt, although they had previously returned quite frequently after the use of quinia. So far as the evidence goes, therefore (which, however, is too limited for a general conclusion), it indicates the superiority of the chloride of sodium over the usual remedies in the permanency of the cures effected by it. And here we should not lose sight of the favourable influence that *may* have been exerted by the quinia before the salt was prescribed.

The difficulty of effecting positive cures of intermittent fever by any remedy or course of treatment, however rigidly pursued, is very great, and sometimes impossible, even though prophylactics be continually used, as long as the individual remains exposed to the cause which developed it. The writer can here speak emphatically, because he has, on two different occasions, been compelled to "fly his country" in order to get rid of this harassing pest. In a number of cases the paroxysms *would* recur every two or three weeks, notwithstanding quinia with Vallet's mass and other remedies to relieve the disordered viscera, including counter-irritation over them, were diligently plied.

Duration of the Disease, and general Health of the Patients.—In a large proportion of the patients the disease had existed a very long time. Of most of them, it is noted, that they had been its victims from six to twelve months. By this it is not to be understood that the disorder then commenced *de novo*, but that it had recurred more regularly and with shorter intervals during that period than previously; for many of them had been its victims for a much longer time, and indeed a few could scarcely remember any period of their lives when they were not from time to time subject to the disease. In four cases the patients had never had the disorder before; and in most of them (all but the very recent ones) there was of course more or less impairment of the general health, with visceral obstruction.

Dose and Mode of Administration.—The quantity given varied from eight to twelve drachms during the apyrexia. At first, eight drachms were given, but the amount was subsequently increased to nine, ten, and even twelve drachms in one instance, with obvious benefit. Children required somewhat larger proportional doses than adults.

Mucilage of elm was selected as the vehicle, on account of its convenience, and because it sufficiently disguised the remedy, which was deemed a matter of importance; for it would have lost much of its efficacy, or have been repudiated altogether, had the patients known they were taking simply common salt; as it is well known to physicians that the influence of the mind upon this disease is very considerable. The following was the formula used:

℞. Chloridi sodii ℥iij.
Ulmi pulv. ℥iij.
Aq. bullientis f℥viii.

Infuse two hours and strain. This forms a saturated solution. Dose, a tablespoonful every two, three, or four hours, so that five or six doses may be taken during the apyrexia. It was not deemed necessary to precede its employment by evacuants, because the patients had recently used such remedies during their former attacks; and moreover, I preferred to use the salt alone, because its real value could thus be better determined. When it is necessary to precede the use of the salt as an antiperiodic, by emetics or cathartics, perhaps there is nothing better for the purpose, in ordinary cases, than the same remedy administered in emetic doses, which will usually produce also moderate catharsis.—*New York Journal of Medicine*, March 1854.

[We have only one remark to make upon these statements. Dr Hutchinson does not tell us if he tried the elm mucilage, *without the salt*, in any of his cases. If the "influence of the mind" would, as he says, have been capable of impairing greatly the efficacy of the remedy, may it not also have been capable of giving an apparent virtue to an inert substance?]

On a Peculiar Form of Malignant Inflammation of the Lips and Face, resembling Malignant Pustule. By W. Parker, M.D., Professor of Surgery in the College of Physicians and Surgeons, New York.

There have come under my observation, within a recent period, several cases of a peculiar form of inflammation of the lips and face, which resembles somewhat phlegmonous erysipelas, but more strikingly, especially in its commencement, malignant pustule, and, in its subsequent progress, carbuncle. It, however, differs from these affections in some essential particulars, which will be noticed after giving the details of the following cases, which illustrate the peculiarity of this form of disease.

Case 1.—I first saw this patient on the 18th of last December. He was a young man, aged 23, merchant, of good character, temperate habits, and in the previous enjoyment of good health. About a week before I visited him, a small pustule made its appearance upon the central portion of the lower lip, just below the edge of the vermillion border. It became painful, had a livid areola, gradually but slowly enlarged, and finally broke and began to discharge. The pain increased, and the swelling extended downwards upon the chin. At my first visit, about this period, the tumefaction had reached as low as the os hyoides, and had extended over the right side of the face to the head; it was hard to the feel, of a livid colour, insensible, and had now much the appearance of a carbuncle. The lips were greatly tumefied, everted; gums swollen, and of the same livid colour; tongue moist; inside of mouth unaffected; ptyalism considerable. The lower lip, about the seat of the original pustule, appeared gangrenous. The pulse was 120, rapid and feeble, respiration unaffected. He was able to get up and sit in the chair, but was suffering from great depression of the vital powers. The course pursued consisted of deep scarifications of the lips and yeast poultices to the swelling, and stimulants to sustain the general system. The swelling continued to extend, involving successively the neck, face, and finally the head. He died on the following day, the 19th, late in the evening.

Case 2.—I visited on the 15th of January, a patient, aged 45, merchant, suffering from what appeared to be a carbuncle of the under lip. He was of a good constitution, temperate habits, and in the enjoyment of good health, previously to the present attack. Four days before I saw him, he was supposed to have cut the lower lip slightly, and applied to it arnica.

The inflammation commenced at this point, the lip swelled largely, became everted, had a livid colour, was tender, hard, and the seat of a burning pain. At several points there were small sloughy apertures, discharging thin pus. The constitutional symptoms were considerable, but not sufficient to confine him to his room. The treatment consisted of free incision and yeast poultices to the lip, and sustaining remedies for the general system. Portions of the lip sloughed, but he recovered.

Case 3.—Mr W., aged 26, married, furniture dealer, of good habits, and hitherto perfect health, discovered a small pustule on the under lip near the right angle of the mouth, on the second of April. It was tender on pressure and had a hard base, but attracted no other attention. During the night the disease extended considerably, involving the whole lid and the right side of the face in a hard, livid, and painful swelling. On the evening of the second day his physician first saw him, and found the lip greatly swollen, of a livid colour, and the seat of a burning pain. He scarified the parts for the purpose of local depletion, and also applied leeches. The swelling continued to extend, involving the right side of the neck and face to a great extent. I saw him on the 7th, at 11 A.M. His symptoms were then most unfavourable, pulse 130 per minute, intermittent every seventh or eighth beat, weak and small; respiration rapid, moaning; skin warm and most; urine free; pupils much dilated; mind clear. He complained of oppression about the chest, and had not been able to obtain sleep. Both lips were involved in the swelling; were hard, livid, and insensible; the whole side of the neck and face was similarly affected, the eye being nearly closed. The frontal vein was livid, red, and prominent, and the veins of the cheek were also visible, as if distended. The treatment consisted of deep scarifications of the lips, and yeast poultices to the part, with anodynes and stimulants. I visited him again at six o'clock, P.M., and found him rapidly failing; treatment of no service. He died the same evening.

Case 4.—I was called, April 10, te see Miss S., aged 30, occupied as a governess, of good constitution, whom I found labouring under the same difficulty as in the preceding cases. Her history was almost precisely similar. Five days before, while in the possession of apparently perfect health, she first observed a small pustule on the lower lip, just below the red line of mucous membrane; it was regarded as a small boil, and no attention paid to it. On the following day the pustule had enlarged somewhat, was hard, and had a livid areola, but she continued about her employment; she spent a feverish, restless night, and on the next day called her physician. The disease gradually extended, assuming the appearances already noticed, and for two days no danger was apprehended. Her symptoms now became much more unfavourable, and at this period I first saw her. She was lying in bed quite insensible; deglutition difficult; respiration laborious; right side of body paralyzed; lips large, everted, and cold; right side of face, neck, and forehead swollen like the lip, hard and purple; right eye protruded; pupils dilated and insensible. On making an incision into the lip, the cellular substance was found filled with small deposits of pus, which were forced out on slight pressure. As she was moribund, treatment was of no avail.

From the history of the foregoing cases it is evident that this disease differs from erysipelas, for which it has in several instances been mistaken, in its origin in a pustule, without a chill or other constitutional disturbance, the hardness of the swelling, its purple or livid colour, insensibility, and absence of much pain. It differs from carbuncle, which in some features it resembles in the class of individuals which it attacks—they being young, temperate, of sound constitution, and in the previous enjoyment of good health—and in its rapidly fatal course. Carbuncle, on the contrary, occurs by preference in persons enfeebled by age or vicious habits. It differs again from true malignant pustule, to which in its origin it seems allied, by attacking persons who have not been affected by poisonous wounds, or who have been liable to the introduction of animal poisons into the system.

This disease would therefore seem to be peculiar, having many points of resemblance to other similar affections, but still not so closely allied to any one to warrant its classification under the same head. In every instance which has come under my own observation, the pustule has been seated upon the lower lip, and from this point the inflammation has spread. In a fatal case related to me by a physician, in whose practice it recently occurred, the pustule was seated upon the side of the nose.

Although the nature and progress of the disease shew a vitiated state of the system, in no instance have I been able to trace the attack to the contact of poisonous matter, or its reception into the system in the food or drink. In every instance the patient has been in the enjoyment of good health, and the progress of the disease, though rapid, has excited so little local and general disturbance as not to excite alarm until a short time before its fatal termination. The general symptoms are of a typhoid character, the vital powers being evidently depressed either by the influence of the disease itself, or, which is more probable, the cause upon which the development of the disease depends.—*New York Journal of Medicine, May* 1854.

II. SURGERY.

Paracentesis of the Thorax.

The *Archives Générales de Médecine* contain an interesting report by Dr Marrotte in regard to this operation, founded on the investigations of a committee, composed of M. M. Gendrin, Trousseau, Legroux, Hardy, and Marotte. The nature of the report does not admit of a satisfactory abstract being made, but we subjoin a brief notice of some of the more interesting points.

Dr Marotte begins by drawing a distinction between the methods of operating. He ranges these under two great heads,—1st, Those which have for their object a free opening, and consequently permit a free communication between the cavity of the pleura and the external air. To these he gives the name of the operation for Empyema. 2d, Those other methods which have for their object to evacuate the contents of the cavity of the pleura, means being taken to exclude the air. He distinguishes these by the title of Thoracentesis.

Regarding the former operation Dr Marotte remarks, that as the admission of the atmospheric air is an inevitable consequence, and forms the great objection by the opponents of the operation, it seemed necessary for the committee to investigate carefully its effects. The report recognises these under three modes of action, mechanical, irritating, and putrefying. In reference to the *first* of these actions of the air admitted into the pleural cavity by the operation, as compressing the lung, and so interfering with its functions, Dr M. well remarks, that this objection has in great measure arisen from what has been noticed in cases of wounds, where the lungs and other thoracic viscera were in their normal condition, from vivisections, and from the result of pneumo-thorax; but as he observes, in cases where the operation is necessary to evacuate purulent or sero-purulent matter, the cavity of the pleura is distended by the fluid, the lung is compressed to its minimum, and the heart and organs displaced. Hence, when the opening is made, and the fluid drawn off, the patient experiences direct relief; for the pressure of the air on the surface of the lung being in great measure counterbalanced by that entering the bronchi, the actual compression is lessened. The amount of flattening of the side of the chest on which the operation has been performed (another objection to this operation) depends upon accidental conditions, such as adhesions or condensation of considerable portions of the lung, preventing it re-occupying its normal position, and several cases are cited to shew that under favourable conditions very little deformity may result.

2d, As regards the irritating action of the air, the conclusions are that its morbific effects on the pleura are neither so absolute nor so severe as might be supposed from the statements of the opponents of the operation of empyema. M.M. Gendrin and Trousseau have satisfied themselves that small quantities of air are not productive of bad effects, that the irritation varies according to the quantity of air admitted, the duration of its application, and finally, according to the dynamic condition of the pleura itself. At the same time it is admitted, that in large quantities it is generally productive of intense inflammatory action.

3d, The admitted air considered as giving rise to putrefaction. The results of the investigations of the committee seem to shew that there is no proof of such effect. That the fetid purulent matter drawn off subsequently is not dependent on the effects of air, as it has been found equally fetid in cases where all air had been carefully excluded by the plan of operation; and that on the contrary in several cases where the pus was from the first fetid, the fetor gradually diminished after evacuation by a free opening allowing air to enter.

In regard to the plan of operation which carefully excludes the air "Thoracentese," the general conclusions are, that it is best suited for cases of hydro-thorax, or collections of thin sero-purulent matter, and that much will depend upon the extent to which the lung will be able to return to its normal conditions. If the lung, on the fluid being drawn off, distends freely, the chances of cure are of course greater, but if confined by adhesions, or so condensed as to leave the cavity of the thorax very partially occupied, then there is always considerable fresh effusion, and that sometimes of an unhealthy character.

Some of the consequences of this form of operation are noticed. These are,—

1st, Sharp pains immediately after the fluid is drawn off, which Dr Marrotte attributes to the breaking up of recent adhesions, under the expansion of the lungs.

2d, Cough is almost a constant, and not unfavourable symptom, and is dependent on the re-entrance of air into the bronchial tubes, which have for some time been unaccustomed to the stimulus.

3d, Syncope. This is most likely to occur when the fluid is drawn off too rapidly.

4th, Thoracentesis causes a determination of blood to the lungs. In most cases this is only marked by the sputa being tinged or streaked with blood; but in some cases it leads to hemoptysis, or pulmonary apoplexy. This determination to the lungs may also lead to the rapid reproduction of the effusion; and one case is mentioned where the patient died suffocated the night after the operation, from this cause. It seems also to give rise to sanguineous effusion in some cases, and different degrees of pneumonia and pleurisy in others. The wound and its subcutaneous track generally heal readily, but in feeble unhealthy subjects the course of the wound may suppurate, and ultimately constitute a true pleural fistula. *Lastly*, Although less liable to cause flattening of the chest than the operation of empyema, thoracentesis is by no means free from being sometimes followed by considerable contraction and deformity of the affected side of the chest.

In concluding the report, Dr Marrotte draws attention to some consequences of paracentesis of the thorax, which though less local and less direct, nevertheless are most important to be kept in mind.

The operation is often followed by sweating and increased secretion of urine. Sometimes both secretions are increased simultaneously. These conditions Dr Marrotte esteems as analogous to the critical excretions noticed in pleurisies treated medically, and hence considers that the operation may produce benefit indirectly, by removing the obstacles which opposed these favourable excretions. On the other hand, he points out that the drawing off of the serous fluid, and its reproduction, is not unfrequently followed by debility and an anemic condition, which he ascribes to the serous depletion, and hence considers that this effect should form a contra-indication to the performance of the operation in debilitated patients.—*Archives Générales de Médecine.*

Cystic Tumour, occupying the true Pelvis, and causing Hernia of the Bladder.

M. Perrin mentions, that in examining the body of an old man who had died of apoplexy, he found a cystic tumour remarkable not only for its bulk, which filled the whole of the true pelvis, and occupied a considerable portion of the hypogastric region; but also and especially on account of the displacement of the pelvic viscera which it caused. M. Perrin, in ascertaining the exact position of the tumour, found that it so displaced the bladder, that the antero-superior portion of that viscus was protruded into the scrotum, forming a hernial swelling in such a manner that the form of the whole organ assumed the shape of a horse shoe, embracing the pubis in its concavity. The protruded or hernial portion formed a large tumour, elongated and perfectly similar to an ordinary inguinal hernia. On applying the taxis, urine flowed by the urethra, and the swelling became flaccid. On dissection the coverings were found to be the skin, dartos, and a quantity of fat, within which was the protruded portion of the bladder much thickened. The portion protruded consisted of the bladder much thickened. The portion not protruded consisted of the "*bas fond*" of the bladder, raised and dragged behind the pubic symphysis. The contracted portion between the two sacs crossed over the left horizontal ramus of the pubis, external to the spine of that bone, and was closely attached there by numerous adhesions. The ureters in descending to the bladder, were connected to the sides of the pelvic tumour.

The patient had always enjoyed good health till the sudden attack of which he died. The catheter had been frequently used during life without any difficulty being experienced in introducing it. M. Perrin states, that on sounding the bladder after death the instrument passed easily.—*Gazette Medicale de Paris.*

Ligature of the Gluteal and Internal Iliac Arteries, for Hemorrhage from a Wound of the Hip.

On the afternoon of the 8th November 1853, Dr Tripler was called in haste to see a man said to be bleeding to death from a cut; accompanied by Dr Bertody he repaired immediately to the spot. They found the patient in a state of syncope, and deluged with blood. On inquiry they learned that he had been engaged in pulling down a fence, and while straining at it with both hands, it gave way, and he fell forcibly to the ground. Immediately behind stood a broken wine bottle, with a shaft some five inches long, and three inches wide at the base. This shaft had penetrated the right buttock within an inch of the ischiatic notch, had ranged forward to the inner aspect of the thigh, and denuded the neck of the femur. On passing their fingers into the wound they felt their points on the inside of the thigh, with apparently nothing but the integument intervening. They satisfied themselves that neither of the trunks of the principal vessels were wounded, then plugged the wound with lint, applied a suitable bandage and compresses, and sent the man home.

On the morning of the 10th, when the deep compress was removed, the hemorrhage burst forth afresh. It was again arrested by compresses and a bandage. The hemorrhage, however, recurred that night, and on the nights of the 11th and 12th, on the latter occasion to an alarming extent. Dr T. supposing that the probable source of the bleeding was the internal circumflex, compressed the external iliac artery in the groin till he procured assistance. The artery being still so compressed, the bandage and compresses were removed, and the wound thoroughly explored, when it was discovered that pressure on the external iliac did not control the hemorrhage, but that the finger on the gluteal in the wound seemed to do so. Further examination, however, shewed that this was not of itself sufficient, but that compression of both vessels was required to arrest the bleeding.

At half-past ten on the morning of the 13th, after another thorough exploration of the wound, and an anxious consultation with Drs Mott, Bertody, &c., it was decided that the gluteal should first be tied, and then if farther operation was required, a ligature should be placed on the internal iliac. Dr Tripler enlarged the wound upon the buttock, and exposed and tied the gluteal artery as it emerged from the pelvis.

All bleeding was at once arrested, the clots were removed from the wound as far as they could be reached, the wound itself kept open for about an hour to watch the effect.

There being no return of the hemorrhage during this time, the wound was closed by some points of suture, and bandaged. At two P.M. the bleeding returned with as much force as ever. The wound was again examined, but the force of the circulation was too weak to afford the current of blood necessary to guide them to the wounded point. Some brandy and water was given to the patient, and in a few moments the bleeding sluggishly returned. Firm pressure was made on the femoral artery by each of the gentlemen present with no effect, and all were satisfied that the hemorrhage did not proceed from any branch of the external iliac.

Ligature of the internal iliac was decided on, and Dr Tripler at once performed the operation. The patient rallied at the first, but gradually sank and died on the evening of the 16th, three days after the operation. Dr T. could only succeed in getting a hurried autopsy. "There had been sufficient peritonitis to produce adhesion of that organ to the tissues in relation with it, but without injection of its vessels, deposit of fibrin, or effusion of serum. The ligature was found embracing the internal iliac an inch below the bifurcation, and a firm coagulum already deposited above the point of ligation."

This case suggests considerations of a practical nature, as to the probable source of the hemorrhage, and also as to the treatment proper to be adopted in such cases. In regard to the former, it is to be regretted that the description of the wound is not so particular as to enable us to be perfectly certain of its exact anatomical position; but, judging from the description given, that the shaft of the bottle, three inches broad, wounded the buttock *one inch in front of the ischiatic notch*, and ranged *forward to the inner side of the thigh*, we are led to the conclusion that the wound must have corresponded to the *lower* part of the sciatic notch;

and as the gluteal artery escapes at the uppermost part of the notch, it would be almost impossible that any of its larger branches could have been wounded, far less the short main trunk of the gluteal. From careful examination of what seems to be the track of the wound indicated by the description, on dissections of the gluteal region, we think it almost certain that the ischiatic artery and the large anastomosing branches of the internal circumflex were the vessels which furnished the bleeding, and hence the reason why neither pressure on the common femoral, or in the gluteal region alone, sufficed to command it. We can easily understand how the ischiatic artery would be compressed when the fingers were passed upwards in the wound, with the intention of pressing on the gluteal. As to the complete arrest of the bleeding on ligature of the gluteal, we think its subsequent recurrence, in full force, in the course of at most three hours after the operation, shews that the temporary arrest of the hemorrhage was merely due to the feeble state of the circulation at the time, in the same way as it had ceased spontaneously on two former occasions, and not in consequence of the ligature of the gluteal.

As regards the treatment, there are few more embarrassing and difficult cases to treat than such as that recorded, and, therefore, we would be very loath to make any remarks which might savour of fault-finding. But, on the other hand, the very difficulty of these cases, and the promptitude and decision of action required, render it imperative to examine the treatment adopted, so as to glean lessons which may be of service in deciding our practice in similar trying circumstances.

From a review of the whole case, we cannot help thinking, that, had Dr Tripler, on the violent recurrence of the hemorrhage, on his removal of the first dressings, and before the patient had been reduced by the repeated bleeding, freely enlarged the wound, and looked for the bleeding points, whilst the circulation still had sufficient force to furnish a jet to guide him, without making up his mind to cut down on a particular trunk, he would have very likely succeeded in directly securing the wounded vessels; or, supposing the bleeding points not discovered, and the hemorrhage still recurring, then ligature of the internal iliac, combined with moderate direct compression in the wound, would have been had recourse to, more warrantably, with *better* chance of success.

Case of Hydrocele and Congenital Hernia, radically cured by the injection of the pure Tincture of Iodine.

The patient was admitted into the Hotel Dieu on the 18th November 1853, under the care of M. Jobert. He stated that, until seven years of age, the left side of the scrotum was of its natural appearance, though rather less than the right. When sixteen years of age, in consequence of a violent effort, there appeared a tumour in the course of the left inguinal canal, extending to the bottom of that side of the scrotum. On his admission to the Hotel Dieu, the appearances were such as satisfied M. Jobert that the patient laboured under a congenital hernia and hydrocele, separated by a constriction. The constriction formed a sort of incomplete diaphragm, due to the congenital contraction marking the point of separation between the tunica vaginalis and general sac of the peritoneum. This partial diaphragm or valve supported the viscera above, whilst its inferior surface was in relation with the collection of fluid around the testicle. The fluid contained in the lower swelling could be made to disappear on pressure, but reappeared whenever the pressure was removed. It was quite translucent.

M. Jobert says,—"I operated by inserting the trocar obliquely through the thickness of the tunics of the scrotum, making it enter slowly by a double movement of pressure and rotation, so as to make it puncture the parietal layer of the tunica vaginalis. The entrance of the instrument was announced by the absence of resistance, and the facility of moving the trocar in the interior. An injection of iodine was then thrown into the tunica vaginalis and hernial sac, the liquid passing up to the point where pressure made by an assistant over the upper part of the inguinal canal sufficed to prevent any communication between the tunica vaginalis and general peritoneal cavity. Only about a spoonful of serous fluid escaped by the canula. Next day the scrotum was swollen, and moderately inflamed. On the 5th of December the inflammatory swelling was diminished, and the whole extent of the inguinal canal was filled by a cylindrical cord in front of the spermatic cord. When he left the hospital, this cylindrical mass of lymph extended along the whole length of the spermatic cord, from the abdomen to the testicle, and firmly filled the canal so as to prevent any hernial descent, either on coughing or on making any exertion.—*L'Union Medicale, 4th April* 1854.

Five fractures occurring in a Child at the same time; successfully treated by the Starch Bandage. By Geo. M. Dewey, M.D., of Keytesville, Mo.—On the 16th of August, 1853, I was called to see a little girl, named Maria White, aged six years. This child, while at play with an older sister in a flour mill, became entangled, by her clothes, with an upright shaft, around which she was carried very rapidly, for three or four minutes, before the mill was stopped. On one side of the shaft was a pile of bags of wheat, on the other mill-stones, leaving a space of some eighteen inches through which the child was carried; some part of her body or limbs struck the bags or stones at every revolution of the shaft.

The following was the result; fracture of the left humerus near the insertion of the deltoid; fracture of the left femur through the middle third; compound fracture of the right femur, at the lower third, with protrusion of the upper fragment, and considerable venous hemorrhage; fracture of the right tibia and fibula at the upper third. I found the patient, one hour after the accident, with feeble pulse, cool skin, and great prostration of the nervous system. No pulsation in the right leg, in consequence of displacement, and effusion at the seat of fracture. Gave brandy and laudanum, which soon brought on reaction.

I immediately called in consultation Dr Isaac P. Vaughan, of Glasgow. By his advice and assistance we applied the starch bandage to all the fractured limbs, and kept the patient under the influence of chloroform during their application.

On the following day, fearing gangrene of the right foot, in consequence of obstruction to the circulation, and the bandages having hardened a little, I divided the bandage on the right limb through its whole length, and cut out a circular piece corresponding to the wound in the thigh. I found the foot cold, livid, and vesications appearing in several places. Applied warm poultices, and at the end of five days warmth gradually returned. For the first ten days following the accident there was considerable constitutional disturbance, and the child being very irritable and ungovernable, I kept her under the influence of morphia most of the time.

Sept. 5. The wound in the thigh having healed, and the bandages becoming loose, I split them longitudinally, and took out an inch or more, brought them together, and applied another bandage over.

23d. Removed all the bandages. Perfect union had taken place in all the fractures, without deformity, except in the right leg, which is a little crooked, and slightly shortened.

February 2, 1854. My patient is playing about with great glee, and with a very slight halt in her gait.

Section and Cauterization of Nerves in cases of Tic Douloureux.

M. Jobert de Lamballe, in a memoir on the treatment of neuralgic affections by the application of the actual cautery, applied either to the skin over the course of the affected nerve, or directly to the nerve after its section by operation, mentions a case of severe tic of the infra-orbital nerve, in which he made an incision through the mucous membrane of the mouth at its reflexion from the lip to the upper jaw, divided the nerve at its emergence from the infra-orbital canal, and then applied two small cauteries at a white heat to the deep surface of the wound. The operation was performed on the 20th January 1854, and there had been no return of the tic on the 1st of March, at which date M. Jobert states the condition of the patient was as follows :—

1st. There is insensibility of the right side of the upper lip (levre droite) and the right cheek.

2d. Water and other drinks are only appreciated by the left side of the lip.

3d. The right side of the upper lip droops a little below the level of the left, both it and the corresponding cheek lie against the teeth, and the tongue is used instinctively to separate them from the teeth.

4th. The patient experienced a feeling of weight on right side of the face.—*L'Union Medicale, 13th April* 1854.

M. Jobert in remarking on the case says, the loss of motor power in the parts supplied by the divided nerve, suggests reflections as to the correctness of our present views of the physiology of the fifth nerve, as proving that the superior maxillary is not only a nerve of sensation, but of motion. But surely a little consideration might have suggested to M. Jobert two things. 1st. Physiologically, there is nothing very surprising that when the excited motory nerve is divided, the corresponding motor filaments should cease to be called into action. 2d. Anatomically and surgically, when we consider the position of the nervous plexus formed by the terminal branches of the portio dura, and the infra-orbital of the fifth, it seems to us the incision necessary to reach the infra-orbital nerve, and the subsequent application of the cautery would be quite sufficient to account for the paralysis of the parts referred to.

New process for the cure of Radical Hernia by M. Maisonneuve.

A young man aged twenty-two, afflicted with inguinal hernia for several years, and for which various bandages had been applied without success, entered the hospital under the care of M. M., anxious to submit to any treatment likely to relieve him of his infirmity. The hernia was a direct inguinal one, incapable of being retained by any means. In these circumstances, the patient was submitted to the following operation on the 28th April:—The patient was placed on his back, and brought under the influence of chloroform, and the operation performed in the following manner. By means of the index finger of the left hand, he pushed, as one would do the finger of a glove, a portion of the skin of the scrotum into the inguinal canal, and then so far into the abdomen, till the end of the finger could be felt through the abdominal parietes, about three centimetres beyond the canal. (1er temps.) The operator next, by means of a bistoury in his right hand, made a longitudinal incision of about 1½ centimetres in length, through the abdominal parietes resting on the end of the finger. This incision included the skin, cellular tissue, abdominal muscles, and peritoneum. (2^{me} temps.)

The skin of the scrotum was now pushed through this opening by means of the index finger, and being thus made to protrude, was divided longitudinally to the extent of two millimetres. (3^{me} temps.) Two needles, each armed with a single thread, were immediately passed through the one and the other lip of this small incision, which was then enlarged to the extent of a centimetre, so that the finger of the operator passed through it. M. M. now finished the operation by uniting, by means of the needles, the edges of the scrotal incision with that of the abdominal one. Ten sutures were then used in order to give the union the desired solidity.

The operator now withdrew his finger from the kind of finger of a glove which he had pushed before him, and which was now transformed into a canal, open above at the abdomen, and below at the scrotum. No accident interfered with this first act of the operation. Re-union was completed without difficulty, and so soon as the 8th of May, M. M. commenced to cauterize with nitrate of silver, and nitric acid, the cutaneous canal which obstructed the herniary one. At last, on the 18th May, when the cauterizations had produced the complete destruction of the epidermis in the course of the canal, M. M. completed his operation in the following manner. He incised the skin of the scrotum around the species of funnel which was formed inferiorly towards the scrotum, which caused the tension to which the pushed up skin was subjected to cease, by permitting it to remount into the canal. Lastly, the vast circular wound resulting from this incision was re-united by means of suture, so that, with the exception of a linear cicatrice, the scrotum shews, at present, no traces of the operation.

As to the result of the operation, it is as perfect as possible; the hernia no longer exists; the scrotum preserves its form and normal disposition; and of the whole of this operation so formidable in appearance, there rest but a few almost imperceptible traces.—*L'Union Medicale*, 8*th June* 1854.

We think that the case requires no comment; but we may say that in noticing it, we do so merely as a surgical curiosity, assuredly not as a plan to be followed.

III. DISEASES OF WOMEN.

Treatment of Uterine Displacements by Intra-uterine Pessaries. M. Depaul.

We here present a condensed view of the essay read to the Academy of Medicine of France in the name of a committee appointed to report on a case of death from this treatment, described by M. Broca. The reading of M. Broca's account was followed by M. Cruveilhier's description of a similarly disastrous case. Soon after, M. Depaul read the following report on the former:—

After some preliminary remarks, the reporter proceeds to demonstrate,

1. That it is a fallacy to attribute to uterine displacements accidents (morbid organic changes and symptoms) which have generally quite a different cause.

2. That in most cases sufficient importance has not been attached to another much more frequent pathological condition (engorgement), which may exist alone, and produce the same symptoms, and which may also be the cause of displacement.

3. That already the science possesses a treatment at once rational and simple, which is every day, in the hands of experienced practitioners, affording the most satisfactory results, and such as can well bear comparison with those which are erroneously attributed to uterine *redresseurs* (instruments for maintaining the uterus replaced); further, that the science is not without resources against those rare displacements which are uncomplicated, but produce certain disagreeable symptoms that demand treatment, nor for those more common cases which are the simple results of another morbid condition which may be aggravated or retarded in the cure by it.

4. That the facts which have been adduced, when submitted to criticism, are far from having the signification ascribed to them; that, on the contrary, almost all testify to the inefficiency of the new method.

5. That in spite of all that has been advanced, it is necessary no longer

to allow ourselves to be deceived, but to take into serious consideration the facts, already too numerous, although all are certainly not known, which prove that the most formidable accidents, and death itself, often result from mechanical manœuvres which at first sight are repugnant to reason.

INSTRUMENTS.—The first (sounds) are destined chiefly for exploration, and have an incontestible value in the diagnosis of certain uterine affections. Kiwisch and Simpson are wrong in disputing the invention of them. Before them, Recamier had introduced instruments of a special form to the fundus of the uterus, and had used them for exploration and other purposes. As we shall soon shew, Amussat has practised since 1827 the catheterism of the uterus in order to prepare its internal surface for the contact of another instrument, intended to remain permanently there ; but it is necessary to add, that no one has better understood than our colleague M. Huguier all the advantages to be derived from the sound in the treatment of uterine diseases.

The sounds of Kiwisch, Huguier, and Simpson are here described.

Lastly, It appears that the origin of the uterine sound is much older than is supposed, and that it should be ascribed to Osiander, who employed a stylet for dilating the uterine orifice, and for bringing the uterus into its normal direction. A similar instrument was subsequently employed by Dr Mackintosh of Edinburgh, and it was not till after this that it was modified and put in vogue by Dr Simpson.

Intra-uterine Pessaries.—The first conception of these instruments has not yet been assigned to its real author, and when, in the discussion of 1849, M. Velpeau laid claim to the priority of invention, I was surprised (says M. Depaul) that another voice was not raised to claim it. Perhaps the eminent professor of the faculty, M. Amussat, did not think the innovation a matter reflecting much credit on him. It is to be hoped that he will now give his reasons for abandoning this plan of treatment.

M. Quetier, in his thesis in 1828 on retroversion of the uterus, says—

" M. Amussat has several times used a means which has perfectly succeeded ; it consists in introducing a small ivory stem into the cavity of the uterus ; but it cannot be done, he says, but with the greatest caution, and should not be practised till after the sensibility of the cavity has been destroyed by previously introducing a sound of gum-elastic. This procedure has the advantage of rendering impossible the retroversion of the uterus, as happens with the ordinary pessaries, which merely elevate the cervix, while the intestinal folds, by their pressure, cause the uterus to bend back again. Before giving this plan our entire confidence, it is better to wait till it is sanctioned by farther experience ; for there is reason to fear that it may bring on acute inflammation of the uterine cavity."

After this, M. Quetier reports a case from the *leçons orales* of M. Amussat in 1827.

Thus, there can be no doubt that M. Amussat has the first claim to the invention of the treatment of displacements by intra-uterine pessaries ; and all that has since then been done, has merely been the development of his idea, and the improvement of his instrument.

M. Velpeau's intra-uterine pessary, which he seldom used, now twenty years ago, was somewhat different from Amussat's.

It was not till much later that the pessaries of Kiwisch and Simpson appeared. The latter has described several instruments.

Valleix supports the use of the pessary with a pubic branch externally. Rigby has made some modifications in the instrument.

The chief modification in the instrument is by Valleix, who has shortened the intra-uterine stem.

The Normal Direction of the Uterus in the Unimpregnated State.

M. Depaul has concluded from careful observations on women shortly before and after death, that it is impossible to determine the position of the living uterus by post-mortem examination.

Further, he has examined fifty women who had never been mothers, and whose ages were between seventeen and thirty years.

In thirty-two cases the axis of the uterus corresponded to that of the abdominal strait or brim. In seven it was more or less anteverted. In four it was more or less retroverted. In three it was manifestly anteflexed. In four it was in a state of retroflexion.

In forty-eight women who had never been pregnant, and whom M. Gosselin examined with a view to verify or disprove Boulard's statements of the great frequency of anteflexion, the following was observed:—In eighteen the uterus was in its normal position. In sixteen it was anteflexed, so that the finger could feel from 1 to 2 centimetres of the anterior supravaginal portion of the uterus. In eleven the anteflexion was only slightly marked. In three he was left uncertain after the examination.

In Valleix's clinical lectures for 1852, there are mentioned:—

Eleven cases of anteflexion, of which seven had previously been pregnant, four had not.

Seventeen cases of anteflexion subsequently observed by Gallard, of which six had previously been pregnant, four had not, seven were not asked the question.

Nine cases of anteflexion seen, in 1852–53, in the wards of Huguier, of which eight had been pregnant, one had not.

Two cases of anteflexion seen by Huguier in 1854, of which one had had two children, one had had one.

In all, thirty-nine cases of anteflexion; of which twenty-three had previously been pregnant, nine had not been so, seven were not ascertained.

The uterus is subject to every form and variety of version and flexion, and combinations of these.

In France the partisans of mechanical treatment have completely neglected the displacement downwards.

What may be considered the normal position of the uterus, is when it is in the axis of the brim of the pelvis; but if the different displacements, with all their varieties, are taken for pathological conditions, much more than the half of the female sex should be looked upon as patients, and treated in consequence. Luckily it is not so, and no one has gone the length of making such a statement. But since intra-uterine pessaries have come into use, the influence of displacement upon the health of women has been singularly exaggerated; and accidents have been too frequently ascribed to it, which were produced by a different pathological condition.

M. Depaul then proceeds to prove this by an analysis of the cases of Valleix, of Gassail, and of Piachaud, who, although supporting the mechanical treatment, admits having seen it produce serious hemorrhages, and twice cause perforation of the uterus.

1. *Do displacements exist in women who are quite in good health?*

M. Depaul cited the eminently valuable opinion of Professor Dubois, who in 1849 stated that apart from the derangements of menstruation caused by the flexions, he considered them as quite inoffensive. The other displacements, descent, anteversion, and retroversion, he considered perfectly innocent except when in an extreme degree. He agreed with Lisfranc in thinking these displacements exercised no injurious influence but what they derived from pathological complications, especially chronic inflammation.

In M. Gosselin's 27 cases of anteflexion already noticed, the women had no uterine complaint, and this physician has observed numerous other displacements without any symptoms. Numerous other cases of uterine pains without displacement he has attributed to an inflammatory or neuralgic state.

Depaul has observed 27 cases of displacement without other appreciable lesion. In ten of these there was retroversion, and of these only one had symptoms at all referable to it. In four who had retroflexion, five who had anteflexion, and five who had retroversion, there were no symptoms. In one where the uterus rested on the perineum there were symptoms.

2. *When displacement coexists with another affection, is it not sufficient to cure that other affection in order to remove the displacement, or at least the symptoms it is alleged to produce?*

Depaul supports the affirmative by an appeal to the results of practice in these cases, in his own and in other hands.

3. *Do not uterine affections, apart altogether from displacement, produce the symptoms ascribed to the latter?*

The affirmative of this question is so well known and frequently observed that it is unnecessary to go into details.

Nevertheless it is necessary to add that in a very small number of cases the displacement alone (especially descent) is a cause of uterine ailment and demands a special treatment; and that its presence may have an injurious influence upon, and retard a cure of other lesions; but for such indications we possess numerous therapeutic means which never or almost never compromise the health and especially the life of the woman.

After describing the treatment applicable to cases of displacement, M. Depaul proceeds to analyze the cases of M. Valleix, and to shew that they bear an interpretation quite contrary to that given by their laborious observer.

He then subjects the instruments of the same practitioner to criticism, and points out that although constantly maintaining that the accidents from the treatment were of no moment, and that cures almost invariably resulted from their use, yet he never ceased to make most important alterations of the instrument. And at last even M. Valleix has now come to give up the treatment by intra-uterine instruments left in the organ. He now merely rectifies the organ by the sound in cases of retroflexion and retroversion, and tries to maintain the rectification by vaginal pessaries; and, wonderful to add, the cures are as complete as with the intra-uterine pessary. This is the most satisfactory condemnation of these instruments.

Depaul then points out that M. Valleix is much deceived in supposing that the treatment by intra-uterine pessary has many partisans. He reports that Dr Simpson has published only opinions, not facts, but quotes a letter from an actual observer of the results of his practice. This letter details the accidents which frequently accompany the introduction and use of these instruments, and ends by saying that experience has shewn that they often enough cause hemorrhages, rectal fistulæ, ulcerations of the rectum, metritis, metroperitonitis, and pelvic abscess. More than once death even has been the final result.

Dr Simpson himself admits that in exceptional cases they may cause so much irritation as to require to be withdrawn; sometimes also they induce menorrhagia.

Dr Churchill characterizes the treatment as a dangerous experiment. He has seen pelvic abscess produced by the instruments. Dr Ashwell has also seen serious accidents follow their use. Dr Gream has seen injurious results follow. Dr R. Lee has observed the most deplorable consequences to follow their use. Dr Oldham does not venture to use the intra-uterine pessary. He records two cases of death from it. Dr Montgomery is convinced of the inefficacy and danger of the treatment. Dr Matthews Duncan, in a recent publication, has expressed almost the same opinion.

M. Depaul then points out the danger of generalizing the use of the sound in exploring the uterus, and relates many cases where in the best hands abortion has been brought on, and where death has even resulted.

He then describes a host of cases of most unfortunate accidents, including death from the use of intra-uterine instruments. Cases also of Cazeaux, Huguier, and Nonat are mentioned, where the instrument was not only dangerous but completely useless.—(*Gazette Medicale, Mai et Juin.*)

[The general conclusions to be derived from the discussion are, that displacement is not to be regarded as in itself a morbid condition, except in rare and extraordinary circumstances, and does not itself demand treatment; further, that intra-uterine pessaries are useful only in very rare cases; that they are dangerous instruments and must be used with great caution.]

IV. MEDICAL STATISTICS.

The Deaf and Dumb in Ireland in 1851. (*From the Report on the Status of Disease, in* 1851, *by the Census Commissioners.*)

True and uncomplicated muteism may be divided into two classes. The first is congenital, or that with which the patient is born; it is either functional or organic: in the latter case, it usually arises from some defect of organization, either in the mechanical apparatus of hearing, the auditory nerve, or in the great nervous centre itself. The second is that proceeding from disease acquired subsequent to birth, but which occurred so early in life, that speech had never been fully acquired; or, from the deafness being so intense, that speech was either entirely lost or greatly impaired in after life. In the latter class, the acquired disease, particularly if it occurs within the first few years of life, reduces the patient to the position of a mute equally with the former, and so places both in the same division in a social and statistical point of view; but, as there are many topics of scientific interest appertaining specially to each class, they have, when necessary, been kept separate, and the calculations belonging to each are given in distinct columns in the several tables. The information derived from the parents or friends, as to whether the person was born deaf and dumb, must be received with caution, as, in a great number of instances, the query is answered in the negative, the relatives and attendants being generally unwilling to admit the fact of congenital defect, and stating that they were sure the child heard well until it was twelve, fifteen, or eighteen months old, although it is not always possible, without special attention being directed to the subject, to form a decided opinion as to the amount of hearing possessed by an infant of such tender age. Upon a strict inquiry, however, it will generally be found that the premises do not warrant the conclusion, that complete deafness did not exist at birth, the instances related being usually those in which the child noticed general vibration, such as that produced by a piano, &c., and not distinct sounds, like those of the human voice; and the more intelligent the class, the more likely are these opinions to prevail. All mutes are not totally devoid of hearing, and, moreover, the visual power in that class being, at a very early age, called into special action, attention is often mistaken for hearing. We have, therefore, in arranging the tables, placed in the class of congenital deaf mutes, all such cases as were not satisfactorily accounted for, by either accident or disease, producing manifest lesion of the organs of hearing, or such as were justly attributed to affections of the head or the nervous system subsequent to birth.

One table exhibits the numbers and condition of these two great divisions, the "Deaf and Dumb," both congenital and acquired, and the "Dumb not Deaf," or dumb only, in the several provinces, counties, and cities in Ireland. In the primary division, the first section gives us the numbers and sexes of the true deaf mutes, or those born without the sense of hearing, and who, consequently, never acquired the faculty of speech. They number 3534—viz., 2030 males, and 1504 females. The members of this class are not otherwise affected, and, although excitable, are generally intelligent, and susceptible of education. The proportions of both these to the population in the different localities, and the relation of the sexes, are shewn in another table.

We are inclined to think that 3534 congenital deaf mutes (otherwise unaffected) is a less number than actually exists in the country. To these may fairly be added 292 of those returned as uncertain or unknown, that being the proportion which the numbers returned of the congenital and acquired bear to one another. The entire number of children under five years of age, returned as deaf and dumb for all Ireland, amounted to only 227; but, according to the proportion which exists between the number of deaf and dumb 5 years old and upwards (4520) and the total population of the same ages (5,909,634), the number of deaf and dumb under five years of age should be 492, the total population of that age being 642,690. This estimated increase of 265, added to the cases of congenital paralytic, or idiotic deaf and dumb, would make the really deaf mutes of Ireland amount to 5012, exclusive of accidental omissions. Deficiencies in the returns for early life must have been expected, owing to the difficulty of determining the question of muteism under eighteen months or even two years; and also the unwillingness, as already stated, of parents even to entertain the idea of want of hearing, until their children are at least two years old. Moreover, many children do not speak plain before four or five years of age.

In the second section of the first table are placed those who have become deaf and dumb from accident or disease acquired after birth; they number 419 persons, 216 males, and 203 females, being in proportion to those in the former section as 1 to 8·43. The third section, amounting to 327—186 males and 141 females—includes those originally returned as deaf and dumb, but concerning whom, from the causes already specified, no accurate information could be obtained upon the secondary inquiry, but 292 of whom belong, in all probability, to the first section. In the fourth section are placed those deaf and dumb, who were at the same time idiotic, paralytic, or both, and numbering 467 persons.

In the second division of this table are placed the "Dumb not Deaf;" they amount to 433, of whom 259 were males, and 174 females. The majority of those proved, upon the third inquiry, to be either idiotic, paralytic, or both. They could hear, but did not speak; or, if they made any attempt at the latter, it was so inarticulate, either from defective mechanism of the organs of speech, or inability of apprehension, that they were incapable of being understood. Their affection, like that of the true deaf and dumb, was either congenital or acquired after birth, but chiefly the former.

Heretofore, it has been generally believed that want of speech is solely and invariably the result of impaired or total loss of hearing. Nevertheless, it appears from the faithful reports of those medical men who examined and inquired into the cases, as well as from our personal observation of some of those included in the returns, that instances of simple and uncomplicated idiopathic dumbness, independent of deafness, though rare, really do exist. The details of some of these cases are embodied in this Report, and assist to shew that the very minute inquiry here instituted with respect to the deaf and dumb, has not only materially added to the general and vital statistics of this country, but has also increased the stock of knowledge upon the subject of muteism generally, and has thus, it is hoped, enlarged the boundaries of science, and conferred a benefit upon humanity. The total number of "dumb only, without other defect," and who were neither paralytic nor idiotic, amounted to 143, of whom 85 were males and 58 females; 129 were born so, and 14 became so after birth.

The idiotic and paralytic, included in the second, third, and fourth sections of this division, who laboured under defect of speech, but possessed hearing in a greater or less degree, amounted to 290, of whom 174 were males, and 116 females. Of these, 48 were paralyzed, 168 were idiots, and 74 were both idiotic and paralytic. The gross total of the idiotic and paralytic, who were both "deaf and dumb," and "dumb not deaf," amounts to 757. Many of these were defective in stature, and otherwise deformed; 19 were deaf, dumb, and blind, and one was, in addition, idiotic, paralytic, and a cripple. The particulars of such instances are given in the remarks upon special and peculiar cases at page 22. Of the 48 paralytic, 23 males and 15 females had been born so; 6 males and 4 females became paralyzed after birth. Of the 168 idiotic, 92 males and 71 females were born so; 6 lost their intellect after birth, and, in 1 instance, the date of the disease was unknown. Among the 74 who were both paralytic and idiotic, 44 males and 21 females had been born so, and 9 persons had acquired these affections subsequent to birth. Of the entire number contained in this division, 24 were in public institutions, and the remainder were under the care of their friends.

Of the entire number of deaf and dumb contained in Table I., 33 persons, 18 males and 15 females, were confined in lunatic asylums or jails at the time of taking the census. Of these, 23 were lunatics, and 10 idiots; 13 of them were born mute, and, in 20 cases, the particulars could not be ascertained; 4 of them were educated previous to their insanity. Several other insane deaf and dumb persons were in the custody of their friends throughout the country.

According to the most authentic and recently published tables, we find that in all Europe, so far as authentic information can be obtained, the proportion of deaf mutes to the population is 1 in 1593, a number bearing a great similarity to the Irish returns. The Dutchies of Luxembourg and Wurtemburg, and the Kingdoms of Tuscany, Bavaria, Belgium, and Holland, have the fewest, while Sardinia, Norway, and parts of Switzerland, exhibit the greatest number of deaf mutes in proportion to their populations, the average of the former countries being 1 in 2209, and in the latter 1 in 642. In some of the Swiss cantons the proportion of the deaf and dumb to the population is as high as 1 in 206, and in these localities muteism is generally combined with cretinism and idiotcy. In a country like Ireland, completely insulated, limited in extent, and possessing much sameness on its surface, one would not expect that the proportion should alter very much in the different provinces and counties; still we find the following varieties:—in Leinster, 1 in 1474; Connaught, 1 in 1499; Ulster, 1 in 1318; and in Munster, 1 in 1317. Deaf-dumbness, arising from all causes, prevails most in the rural, and least in the civic districts, *i. e.* those towns whose inhabitants amount to 2000 and upwards, exclusive of those in workhouses. Thus there are, comparatively, fewer cases in proportion to the population to be found in the towns of Drogheda, Galway, and Belfast, and the cities of Cork and Dublin, the average for these places being 1 in 2355. The city of Limerick is the chief exception to this rule. Generally speaking, the flat champaign counties, such as Roscommon, Westmeath, Dublin, and Kildare, present the fewest cases—the average in these being 1 in 1935; while Mayo, Limerick, Donegal, Waterford, and Wicklow, and also Tipperary, Tyrone, and Fermanagh, the former lying chiefly on the sea coast, and the majority presenting mountain ranges, have a greater number of deaf and dumb, in proportion to their populations, than the remaining counties, their average being 1 in 1068, and in Wicklow the proportion is as high as 1 in 1031. Leitrim, Clare, Down, and Antrim, are also high.

Comparing the congenital with the acquired cases, we find some remarkable differences, caused by localities. In the former class, the proportions are 1 in 1872 in the civic, and 1 in every 1517 in the rural; whereas in the latter the reverse obtains—that class being in the proportion of 1 in 9519 in the civic, and 1 in 11,749 in the rural—the congenital form of disease existing most amongst the rural population; and that acquired after birth prevailing most in crowded cities and towns, where children and young persons are particularly exposed to accidents, scrofulous affections, and deleterious influences, arising from density of population, unwholesome food, impure air, and insufficient sewerage. Moreover, the populations of cities and towns being in great part recruited by immigration from the rural districts, the deaf and dumb are more likely to remain with their friends in their native homes than those possessing speech and hearing.

The sexes of the deaf and dumb, both congenital and acquired, are as 100 males to 7660 females; such being in accordance with the usual law which appears to regulate the sexes in this class. Among the congenital cases, he proportion is 100 males to 7450 females; and in the acquired, 100 to 9336—shewing in the former an excess of males over and above the general proportion of that sex. It must, however, be remarked, that according to the present census there is rather an undue proportion of females above males in Ireland.

The ages at which the greatest number were returned were from 15 to 30.

Two Tables shew, by ages and sexes, the occupations of the deaf mutes, both congenital and acquired, and also the occupations of the parents of the deaf and dumb. Of the entire 4747 mutes, there were 2830 either unoccupied, mere children, or those whose employments (if they had any) were not specified in the returns; 234 were receiving instruction at the varous educational establishments, or from private teachers at their own homes; and 135 were mendicants or strollers, many of whom were, however, at the time of taking the census, resident in the workhouses; 661 persons, including 25 females, were labourers, or agriculturists; 260 household or farm servants; and 76 farmers.

From an examination of the table, we observe what a variety and extent of arts, trades, and handicrafts the deaf and dumb are capable of being employed upon. Of these trades the most numerous are boot and shoe makers and binders, 72; weavers, 66: tailors, 27; carpenters, 14; smiths and workers in iron, 7; masons, 6. The occupations of sailors, fishermen, and gardeners are not usually considered as trades. Among the special female occupations, we find milliners and seamstresses, 111; spinners, 45; and laceworkers and embroiderers, 51. Six persons were employed as teachers in the different schools for the deaf and dumb. Several of these trades and occupations were taken from choice, but a large proportion were either taught at the educational establishments, or were acquired from the parents or other near relatives of the deaf and dumb, as shewn by another table, which exhibits not only the occupations of the parents of the deaf and dumb, but also the condition of the offspring formerly mentioned. From this we learn that the great bulk of the deaf and dumb in Ireland are derived from the agricultural and labouring classes, the latter including servants and pensioners. Among the other trades we find, that in 122 instances the fathers of the deaf and dumb were weavers; 47 were boot and shoe makers, 46 carpenters and cabinetmakers, 35 masons and bricklayers, 30 smiths, 16 butchers, 26 were sailors or fishermen, 28 hawkers and dealers, 17 gardeners, and 15 shopkeepers; 43 were of the gentry, mercantile, and professional classes, and the mothers of 26 were milliners and dressmakers.

Tables were compiled shewing the marriage state among deaf mutes, together with the result as regards the offspring of such marriages. From these we learn that 98 deaf and dumb persons—60 males and 38 females—were married. In 86 instances—54 males and 32 females—only one parent was deaf and dumb; from the marriage of these, 203 children resulted, among whom there was but one instance of muteism, a male, in the county Limerick. Six instances have been recorded of the inter-marriage of deaf and dumb persons; their offspring amounted to 13, of whom one only, a female, in the city of Dublin, was deaf and dumb. An instance occurred in the county Cavan of a deaf mute, son to a man deaf only, but whose uncle and aunt were both deaf and dumb. There were, however, in our returns, 19 cases in which one parent was deaf and dumb, but concerning whose offspring no accurate information could be procured; and in the foregoing enumeration of offspring are included some instances of children under one year of age, whose hearing and speaking powers were consequently unknown. It is remarkable, that while muteism is often manifest in several members of a family derived from a common stock, the defect is seldom transmitted direct from parents to children; thus according to the returns of the Hartford Institution, United States, we learn that in ninety-one instances where both parents were deaf and dumb, in only four cases were the children similarly affected.

Of the entire number, 2892 were cases of single mutes in each family; the sexes being in the proportion of 100 males to 72 females. In each of 337 families, there were born two mute childern; in 111 of these instances the mute children were both males; in 53, both females; and in 173, a male and female. In 137 instances there were three mute children born to each family; in 17 of these cases they were all males; in 16, all females; in 49, two males and a female; and in 55, a male and two females. Thirty-seven instances presented of four mute children, born of the same parents; the combinations of sexes among these varied considerably, but the most frequent was that of two males and two females, of which 14 cases occurred. Nine families had each five mutes, and in three there were six, the particulars of which and the relations of the sexes may be learned by reference to the table. One instance presented of seven mute children, all females, born to a family in the county of Sligo, three of them being twins; and from the county of Donegal we have had a return of eight mutes—5 males and 3 females—born to the same parents, of whom 3 are living and five dead. Eleven cases, "dumb only," but not included in the table, had brothers and sisters deaf and dumb; and in eleven instances of acquired muteism, other members of the family were born deaf and dumb. By far the greater proportion of the deaf and dumb were ascertained to be first children.

[As regards the causes of deaf-muteism, the report of the commissioners gives numerous details. As regards these, however, and likewise as regards the rare and remarkable instances of defect which are given in detail, we must refer to the report itself, or to the interesting little pamphlet of Mr Wilde, "On the Physical, Moral, and Social Condition of the Deaf and Dumb," which likewise contains an account of the methods of instruction and their history, too long for quotation, and too important for abridgment.]

2. *Extract from Annual Report of the Royal Edinburgh Asylum for the Insane, for the year* 1853.

Post-mortem examinations were permitted in 61 cases, being an increase of 6 over last year. They were made with much care, and the following are the results:—

Of those examined, 3 had been cases of Acute, 2 Chronic, and 1 Periodic Mania, 23 Dementia, 3 Dementia with Epilepsy, 1 Mania with Epilepsy, 1 Melancholia, 1 Moral Insanity, 1 Delirium Tremens, 11 Monomania, 12 General Paralysis, 2 Congenital Imbecility.

Calvarium was of unusual thickness in 32 cases; 1 of Chronic Mania, 1 of Acute Dementia, 13 of Dementia, 1 of Dementia with Epilepsy, 5 of General Paralysis, 8 of Monomania, 1 of Moral Insanity, and 2 of Congenital Imbecility.

Calvarium was thinner than usual in 13 cases; 3 of Acute Mania, 1 of Chronic Mania, 1 of Melancholia, 3 of Dementia, 2 of Monomania, and 3 of General Paralysis.

Diploe was absent in 14 cases; 1 of Acute Mania, 1 of Acute Dementia, 6 of Dementia, 3 of Monomania, 1 of Moral Insanity, 1 of General Paralysis, 1 of Congenital Imbecility.

Elongation of Anterior Clinoid Process on right side, was observed in one case of Mania, with Epilepsy.

Atheromatous Deposit in arteries of brain, was found to a great extent in one case of General Paralysis.

Increased Thickness of Dura Mater was found in 18 cases; 1 of Acute Mania, 1 of Moral Insanity, 4 of Dementia, 2 of Dementia with Epilepsy, 3 of Monomania, 6 of General Paralysis, and 1 of Congenital Imbecility.

Thinness of Dura Mater was noticed in 5 cases; 3 of Dementia, 1 of Monomania, and 1 of General Paralysis.

Ossific Deposit in Falx Cerebri existed in 2 cases; 1 of Dementia, and 1 of Monomania.

Adhesion of Dura Mater to Calvarium existed in 17 cases; 3 of Acute Mania, 1 of Chronic Mania, 1 of Melancholia, 3 of Dementia, 1 of Dementia with Epilepsy, 5 of Monomania, 1 of Moral Insanity, 1 of General Paralysis, and 1 of Congenital Imbecility.

Opacity and Thickening of Arachnoid was found in 39 cases; 2 of Acute Mania, 1 of Periodic Mania, 1 of Chronic Mania, 1 of Acute Dementia, 9 of Dementia, 1 of Dementia with Epilepsy, 1 of Melancholia, 7 of Monomania, 2 of Moral Insanity, 12 of General Paralysis, and 2 of Congenital Imbecility.

Crystalline-like Deposit over general surface of Arachnoid was noticed in 2 cases; 1 of Dementia, and 1 of General Paralysis.

Congestion of Membranes was noticed in 19 cases; 2 of Acute Mania, 1 of Periodic Mania, 1 of Chronic Mania, 7 of Dementia, 1 of Dementia with Epilepsy, 2 of Monomania, 4 of General Paralysis, and 1 of Congenital Imbecility.

Adhesion of Membranes to Cortical Substance was found in 10 cases of General Paralysis.

Serous Effusion into Sac of Arachnoid existed in 53 cases; 3 of Acute Mania, 1 of Periodic Mania, 1 of Chronic Mania, 1 of Acute Dementia, 18 of Dementia, 2 of Dementia with Epilepsy, 1 of Mania with Epilepsy, 11 of Monomania, 2 of Moral Insanity, 11 of General Paralysis, and 2 of Congenital Imbecility.

Sub-Arachnoid Serous Effusion was found in 47 cases; 3 of Acute Mania, 2 of Chronic Mania, 1 of Periodic Mania, 1 of Acute Dementia, 17 of Dementia, 2 of Dementia with Epilepsy, 10 of Monomania, 2 of Moral Insanity, and 9 of General Paralysis.

Sero-Sanguinolent Effusion into Sac of Arachnoid was found in 5 cases; 1 of Chronic Mania, 2 of Dementia, 1 of Dementia with Epilepsy, and 1 of General Paralysis.

Sub-Arachnoid Sero-Sanguinolent Effusion occurred in 2 cases; 1 of Mania with Epilepsy, and 1 of Dementia.

The Convolutions of Cerebrum were noticed to be remarkably diminished in size in 4 cases; 1 of Dementia, 1 of Epilepsy with Dementia, 1 of General Paralysis, and 1 of Congenital Imbecility.

Wasting of Optic Nerves and Commissure occurred in 1 case of General Paralysis.

Paleness of the Gray Matter was noticed in 24 cases; 1 of Acute Mania, 1 of Periodic Mania, 3 of Melancholia, 13 of Dementia, 3 of Monomania, 1 of Moral Insanity, 3 of General Paralysis, and 1 of Congenital Imbecility.

Gray Matter was of a dark tint in 10 cases; 1 of Mania with Epilepsy, 1 of Dementia, 2 of Monomania, 6 of General Paralysis.

Gray Matter was of a Violaceous tinge in 3 cases; 1 of Chronic Mania, and 2 of General Paralysis.

Gray Matter was softened in 31 cases; 1 of Acute Mania, 2 of Chronic Mania, 1 of Acute Dementia, 10 of Dementia, 5 of Monomania, 1 of Moral Insanity, and 11 of General Paralysis.

White Matter was softened in 16 cases; 7 of Dementia, 1 of Dementia with Epilepsy, 5 of Monomania, 3 of General Paralysis.

Serous Effusion into Lateral Ventricles was found in 37 cases; 2 of Acute Mania, 1 of Chronic Mania, 1 of Melancholia, 11 of Dementia, 2 of Dementia with Epilepsy, 6 of Monomania, 2 of Moral Insanity, 11 of General Paralysis, and 1 of Congenital Imbecility.

Sero-Sanguinolent Effusion into Lateral Ventricles occurred in two cases; 1 of Acute Mania, and 1 of Mania with Epilepsy.

Foramen of Monro unusually large in 15 cases; 1 of Acute Mania, 1 of Chronic Mania, 3 of Dementia, 1 of Dementia with Epilepsy, 4 of Monomania, 1 of Moral Insanity, 3 of General Paralysis, and 1 of Congenital Imbecility.

Crystalline-like Deposit in Membrane of Lateral Ventricles was found in 8 cases; 1 of Acute Mania, 1 of Chronic Mania, and 6 of General Paralysis.

Cystic bodies in Choroid Plexuses occurred in 27 cases; 2 of Acute Mania, 1 of Chronic Mania, 1 of Periodic Mania, 8 of Dementia, 1 of Epilepsy with Dementia, 1 of Acute Dementia, 6 of Monomania, and 7 of General Paralysis.

Disorganization of the right Corpus Striatum was found in a case of Monomania.

Pineal body was unusually large in 4 cases; 1 of Chronic Mania, 1 of Mania with Epilepsy, 2 of Monomania.

Absence of Grit in Pineal Body in 19 cases; 1 of Acute Mania, 1 of Chronic Mania, 1 of Mania with Epilepsy, 1 of Acute Dementia, 6 of Dementia, 1 of Melancholia, 2 of Monomania, 1 of Moral Insanity, 4 of General Paralysis, and 1 of Congenital Imbecility.

The general conclusions to be derived from the preceding abstract are, that, in a large proportion of the deaths, well-marked appearances were found indicating increased vascular action, or slow inflammatory processes affecting the membranes of the brain, and producing thickening and serous effusions. These appearances were most constant and most distinct in the deaths from General Paralysis, where there was also found a peculiar kind of softening of the gray substance of the brain; and on examining this part of the brain under the microscope, the cells of which it is principally composed appeared to be enlarged and altered in form. In these cases, *adhesion* of the membranes to the surface of the convolutions was present in all instances where there was not such a quantity of serous effusion interposed as to prevent it;—in stripping off the membranes, small layers of gray matter were stripped off with them, particularly on the lateral surfaces of the hemispheres.

The brain was carefully weighed in every case, and the weights, with those of other viscera, together with the specific gravity of the gray and white matter of the brain, recorded in Tables. * * *

It would be out of place to enter into a detailed examination of the inferences to be derived from these data, and from those of a similar kind which have been recorded in the Reports of previous years; but I may be permitted to state briefly that they seem to warrant the following conclusions:—1st, That the absolute weight of the brain is increased in persons who have died insane; 2d, that the weight of the cerebellum is increased in a greater degree—the cerebellum being heavier in proportion to the cerebrum in the insane, than in persons dying of other diseases; 3d, that the specific gravity of the brain is also increased in the insane, and that this increase is greater in the cerebellum than in the cerebrum. If these conclusions are borne out by further observation, they may lead to curious and interesting speculations as to the functions of the cerebellum, and the influence exercised through this organ over the regulation and control of the thoughts, as well as those (hitherto attributed to it by some anatomists) over the voluntary muscular movements.

THE

EDINBURGH MEDICAL AND SURGICAL JOURNAL.

1st OCTOBER 1854.

PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Cases of Aneurism.* By JAMES MILLER, Professor of Surgery in the University of Edinburgh, &c., &c.

1.—*Femoral Aneurism—Ligature of the external iliac—Recovery.*

The following case presents points of interest in the performance of the operation, and in the subsequent progress of the tumour. The narrative is taken from the journal of the hospital, as drawn up by Mr Bowen, to whose kindness, skill, and care, both patient and surgeon are much indebted, in the faithful performance of his duties as clerk in charge.

"William M'Cormack, a plumber, æt. 27, admitted into the Surgical Wards, Royal Infirmary, Edinburgh, under the care of Professor Miller, 27th Dec. 1853.

"Upon examination a large ovoid and circumscribed tumour is seen lying over the region of the common femoral of the left side, extending about 3 inches above, and 2 to 2½ inches below Poupart's ligament. There is a heaving pulsation in every part of the swelling, synchronous with the heart's impulse, appreciable by both the sight and touch, especially by the latter, when a very expressive thrill is imparted to the compressing hand. By pressure on the superior femoral artery below the tumour, the pulsation in the latter is increased, and a distinct 'bruit de soufflet' perceived on auscultation. The integuments over the tumour are not discoloured; but a few enlarged veins and the cicatrix of a bubo are visible. At present there is no pain in the tumour, but there is more or less weakness, with numbness and cramps in the lower extremity.

"About the 1st September 1853, the tumour first appeared in the form of a small swelling over the common femoral of left side; it was soft and compressible, but returned to its original dimensions as soon as pressure was removed. Distinct pulsation existed in it from the beginning, accompanied with cramps and numbness in the left lower extremity. The patient can assign no cause for the appearance of the tumour, except that his occupation of plumber has frequently required him to lift and carry heavy weights. A month after the appearance of the swelling, the patient strained himself in lifting some heavy weights, and immediately afterwards experienced a 'racking pain' in the region of the tumour, which then for the first time assumed a reddened appearance, and continued in that state for some days. Since that period the swelling has gradually increased in size and hardness, preventing the patient from following his usual occupation so well as formerly. He had syphilis about eight years ago, but no mercury seems to have been used in its treatment. The general health appears tolerably good.

"*Dec.* 29.—A consultation having been held, there was not the slightest hesitation in pronouncing the tumour to be an aneurism of the left common femoral artery.

"*Jan.* 14.—Professor Miller proceeded to tie the external iliac artery. Upon the application of the ligature, the pulsation in the tumour immediately ceased, and it felt more elastic and soft. Four hours after the operation, smart hemorrhage came on; the wound was opened up, and two vessels in the muscular wall were secured; it was then carefully sponged out, and again closed by stitches—the patient under chloroform. About six to eight ounces of blood may have been lost.

"*Vespere.* Pulse, 120; heat of skin, natural. Sol. Mur. Morph. ʒi. immediately, and to be repeated, if need be.

"*Jan.* 15.—Passed a good night. Pulse 120, soft and compressible; no pulsation whatever in the tumour, and little if any change in the temperature of the leg. Strict antiphlogistic regimen has been enjoined; and the patient has been lying on his left side on a water-pillow, with his back raised and body flexed; the abdominal muscles are thus relaxed, and the discharges can escape freely from the dependent opening.

"*Jan.* 16.—Passed another good night; pulse 130. ℞. Ext.

Alcoh. Aconiti gtt. v., Aquæ Puræ ℥viij. ft. mist. de qua sum. coch. unum mag. secundis horis.

" *Vespere.* Pulse 112. ℞. Ol. Ricini ℥i. Aq. Cinnamom. ℥iss. m. s.s. Complains of pain in the wound, and has some flatulency ; a slight discharge of blood and pus now coming from the wound; water dressing continued; no pulsation in tumour; limb warm and comfortable, and kept flexed and supported with soft pillows, wadding, &c.

" *Jan.* 17.—Has no unfavourable symptom; has had his bed made, and looks cheerful and comfortable ; bowels not yet opened, but there is a desire to pass a stool. Hab. Enema Commun. et repet. s.o.s. Soon after this the bowels were freely moved without pain or effort.

" *Jan.* 18.—Has a slight cough, which gives pain in the wound. ℞. Tinct. Opii Camph. ℥iss. Mucilaginis ℥iv. Mist. Scillae ad ℥viij. misce. Sig. ℥ss. quaque, 3tiâ hora. Skin cool; free from pain; pulse 88; the discharge from wound much increased, with less blood and serum.

" *Jan.* 20.—Continues well; stitches removed from wound; healthy discharge going on ; skin moist and cool ; bowels twice opened; pulse 80; and no pulsation has returned in tumour.

" *Jan.* 26.—The ligatures, with the exception of the one upon the external iliac, have come away; patient is looking well, and begins to take nourishment.

" *Feb.* 4.—Patient's recovery progressing rapidly; complains of slight numbness in left heel and sole of foot; the ligature upon the external iliac came away; it had, in all probability, been separated for some days previously. The tumour has begun to diminish in size; there has not been the slightest pulsation in it since the operation was performed.

" *Feb.* 20.—Is gaining strength; has been ordered generous diet, with wine and porter; begins to sit up in bed.

" *March* 2.—Some irritability of the neck of the bladder; passed the catheter twice. ℞. Aq. Acet. Ammon. ℥i. Spir. Æther. Nit. ʒii. Vin. Antim. ʒi. Aquæ Font. ℥ivss. Sig ℥ss. quâque 3tiâ hora.

" *March* 4.—Passes his urine freely now; his appetite not so good. ℞. Quinæ Sulph. Extr. Aloes ā ā ℈j. Extr. Gent. co. ℈ij. Con. Ros. q. s. fiant pil. xij. quarum una ter in dies sumend.

" Has risen from his bed for the first time since the operation, and moves about the ward by the aid of a crutch. The wound is almost entirely closed; very little discharge now coming away; tumour much diminished in size, and harder.

" *March* 7.—Has been taking rather too much exercise during the last three days. For the first time since the operation, a very slight thrill was felt in the tumour, which is somewhat enlarged again; ordered to resume and retain the horizontal posture.

"*March* 8.—No thrill in tumour, which has again diminished in size.

"*March* 28.—Has kept his bed since the 7th; a large quantity of healthy-looking pus is being discharged from the small opening in the wound.

"*May* 4.—Has left the hospital on leave of absence, for two weeks. Slight discharge still going on from the small opening in the wound; keeps water dressing to it, and wears a flannel bandage as a support to the abdomen.

"No thrill or pulsation of any kind (with the exception of what was referred to on March 7) has been felt in the tumour since the time of the operation. A most careful examination of the parts was instituted twice daily, and if any such thing had existed, its presence would certainly have been detected."

At the present date, the patient is walking about, somewhat weak and lame on the left side, with occasional slight discharge from a small fistulous aperture in the centre of the abdominal cicatrix; the tumour is almost gone, a thickness and fulness remaining in its site.

1. This is another example of aneurism of the lower extremity occurring in a plumber. That calling seems specially prone to such casualties; probably on account of the heavy lifts which have often to be borne, with a special strain on the limbs. He was a stout healthy-looking young man; and, saving his vocation, there seemed no other predisposing cause.

2. The incision required to be placed high. In consequence of the tumour bulging into the iliac space, and with a view to secure protection to the aneurismal cyst, the knife was entered almost where, under ordinary circumstances, it would have emerged, namely, nearly opposite to the anterior superior spinous process of the ilium. The incision had to slope backwards considerably in consequence, and implicated an unusal thickness of muscular substance. The disadvantages were—large extent of wound; unusual bleeding from arterial branches; injury to muscles, and consequent debility in the abdominal cicatrix; and removal of the line of access from the tract of the artery sought. These, however, were overborne by the securing of greater safety to the aneurismal cyst, and to the peritoneum, than if the wound had been lower and less.

It is worthy of note that, now when cicatrization is complete and the tumour has dwindled away, the line occupies very much of the usual locality; the superficial parts having descended to their ordinary level, after having been distended and pushed upwards by the tumour when at its height.

In the treatment of such a wound, it is very obvious how attention to position becomes most important, so as to relax the implicated muscular fibres; and how coughing, sneezing, or other exertion of the abdominal muscles should be avoided or restrained by every means in our power.

3. When my finger reached the external iliac, I found it following a course which I had before observed under somewhat similar circumstances—that is, bending downwards into the sacral space, instead of running straight along the psoas ridge; and so coming to lie deep, as if in the line of the internal iliac. With the point of the finger I was able to exert such traction on the parts as brought the vessel nearly up to its ordinary site, otherwise deligation would have been accomplished with no little difficulty. The cause of the bending, no doubt, was the bulging and pressure of the tumour below.

4. From the time of the operation to the 7th of March, a period of nearly two months, there was not the slightest pulsation or thrill in the tumour; neither did the swelling ever become hard; on the contrary, it continued throughout the whole cure, soft, elastic, and fluctuating; during the later portion of the period a hard external rim being felt, surrounding a soft centre of distinct fluctuation. The progress of diminution was also unusually tardy; and even yet the shrivelled remains of that outer rim are perceptible. This deviation from the ordinary course of events I can only explain by supposing, that at the time of operation the aneurism contained but little solid matter, and was almost entirely occupied by fluid blood, as its compressibility indeed showed; that the communication between the cavities of the cyst and artery was of small size; and that a cylindrical clot became suddenly impacted into that aperture of communication, so as to occlude it thoroughly. The aneurism then became reduced to a condition somewhat resembling that of an ordinary severe bruise; a hollow space, occupied by blood—partly coagulated circumferentially, fluid centrally, and amenable to absorption; the absorption, however, less brisk than in bruise,—inasmuch as the walls of the aneurism may naturally be supposed less gifted in that way, than the ordinary primal tissues constituting the boundaries of the "poche du sang" in bruise. The slight recurrence of thrill, with some increase of size in the retreating tumour, on the 7th of March, might be accounted for by supposing that the obstructing clot, or cork, not yet incorporated with the tissues in which it rested, had been jostled and disturbed by the movements of the patient, as he

hobbled through the ward, so as to admit a very tiny current of blood: and that, on resumption of rest and the horizontal posture, the plug, having again become adjusted, shut out all entrance of fresh blood.

At all events, here is an example of progressive and completed cure in a large aneurism, without consolidation of the contents, and without incorporation of all the component parts of the tumour.

5. Another case is added to those already in the records of surgery, of femoral aneurism successfully treated by ligature of the external iliac; an humble fact always of some value in practical statistics.

6. Let me direct attention to the use of aconite in very small doses, frequently repeated, as a valuable antiphlogistic. In my surgical experience this remedy is of very remarkable efficacy in arresting and subduing inflammatory invasion, after wound or other injury; telling almost at once upon the pulse and skin; and while very efficient, perfectly safe. My experience of it, as an antiphlogistic, extends from the date of its first introduction by my friend and former pupil, Professor Fleming of Cork, in 1845.

I have also found it of value as a simple sedative of the circulation; and, indeed, employed it as such, in the next case, before any inflammation could have had time to supervene.

2.—*Right Popliteal Aneurism—Ligature of the Superficial Femoral—Immediate Gangrene—Line of Demarcation, below the Knee—Amputation in the Thigh—Recovery. In the same patient, and subsequently, left Popliteal Aneurism—Ligature of the Superficial Femoral—No Gangrene or other accident directly connected with the operation; but Death by Diarrhœa.*

The report is again by Mr Bowen:—

"J. M'L. æt 38, was admitted into the hospital under Professor Miller, 22d March 1854: by occupation an iron-moulder.

"*History.*—He states, that five months previous to this time, on rising from bed one morning, he found his ankle swelled and throbbing, but went to his work as usual. The swelling continued; and it was just a week afterwards that he noticed for the first time, a pulsating tumour, of the size of a pigeon's egg, in the popliteal space of right leg. The tumour from this time continued to increase steadily in size, and the patient has invariably experienced severe exacerbations of pain in the limb on returning home after his day's work.

The nature of his occupation requires him generally to remain on his knees from five to six hours a-day, and he has been frequently in the habit of lifting heavy weights.

" *Signs on admission.*—An aneurism of the size of a large orange exists in the right popliteal space, and the patient is in a very unfavourable state. His pulse is quick, tongue foul, some dyspeptic symptoms, but no appreciable pulmonary lesions; he has a general scaly tertiary syphilitic eruption. The œdema of the limb is great, and the tumour large, and evidently rapidly increasing. The agony he suffers is very great.

" *March* 24.—Patient's general health having been attended to, and the bowels well cleared out; as no time was to be lost, Professor Miller proceeded to tie the superficial femoral. The operation was easily performed, and the femoral vein was never seen. A non-stimulating practice was enjoined, the limb being encased in wadding. Ordered—℞. Tinct. Aconiti gtt. v. Aquæ Puræ, ℥viij. M. Sig. ʒss. quaque 2ª hora.

" *Vesp.*—The temperature of the foot is very low; but the patient is in much better spirits since the operation. Ordered —R. Tinct. Hyoscm. ʒiss. Solut. M. Morp. gtt. xxx. Mist. Camph. ℥i. M. Sig. h.s.s.

" *March* 25.—The patient slept a little during the past night, at intervals of an hour; notwithstanding, this morning the dorsum of the foot presents the peculiar measly look so much dreaded as the precursor of gangrene. The coldness of the limb is increasing. The foot to be very gently compressed or rubbed (between the hands of assistants) for a few minutes at a time.

" *Vesp.*—Discoloration of the foot and ankle has taken place. and evident symptoms of gangrene are being established. Ordered to continue the opiate at night.

" *April* 23.—An eligible line of demarcation has formed below the knee: but there is a large abscess in the sural mass, from which a purulent discharge, dark coloured and putrid, has been issuing for some days. There is marked hectic, and the constitutional symptoms are becoming serious. Two large bed sores have formed over the hip of the affected side. The ligature came away on the 9th inst., and the wound is nearly cicatrized.

" Professor Miller proceeded to amputate low down in the thigh, owing to the unsatisfactory state of the tumour. In tying the femoral on the face of the stump, its coats were found very rigid and almost calcareous.

" *April* 27.—This is the fourth day since the removal of the limb; the flaps show no disposition to unite. A small popliteal aneurism in the other leg was detected to-day. The patient is kept in ignorance of it.

"*May* 18.—The stump is now healing, one of the bed sores nearly well. Ordered—℞. Quinæ Disulph. ℈ij. Acid. Sulp. Dil. ʒii. Syrup. Aurant. ℥ss. Aquæ. ℥vijss. M. Sig. ℥ss. ter in die sumend. Generous diet and stimulants to be given.

"*May* 20.—The left limb is beginning to swell; the popliteal tumour has rapidly enlarged, and the same excruciating pain is now complained of that was experienced with the other aneurism. The stump is nearly healed, but there is one large bed sore still open four or five inches over the sacrum. A consultation was held; and ligature of the superficial femoral having been determined on, the operation was proceeded with at once.

"*May* 27.—There has been no mottling of any part of the limb. A little lividity was observed on the dorsum of the foot two or three days after the operation; but this passed away, and everything seemed to be progressing favourably up to the 25th, two days ago, when diarrhœa set in with symptoms of collapse. Ordered stimulants and astringents. Pulse very weak, began to sink at noon; he never rallied, and died at 5 P.M.

"*Postmortem Examination.*—Owing to circumstances this was performed rather hurriedly. A few decolorized clots were found in the aorta; which vessel, with the exception of a few small spots of atheroma in its descending portion, was healthy, as were also both iliacs. The heart showed no appearance of disease."

The reporter has put down briefly the leading points of the case. The narrative will be made a little more full in the following remarks.

1. About three weeks before I saw the patient, he had applied to a medical man for aid on account of an eruption of the limbs with swelling. It was of the nature of Purpura, and was treated accordingly. The dark eruption having disappeared, the patient returned; still complaining of pain and swelling, especially in the right limb. The ham was then suspected and the aneurism found. In two days he was in the hospital; recently recovered from Purpura, thin, pale, quick pulse, loaded tongue, anxious countenance, a scaly syphilitic eruption studding the general surface, a large and rapidly enlarging aneurism occupying the whole ham, constant agony, sleepless nights, lower limb very much swollen, and circulation in the foot and ankle already much compromised, so far as the arterial pulsations then indicated.

2. Under these circumstances, the first question to be considered obviously was—"What line of practice is to be followed in regard to the tumour—ligature or compression?" Obviously, there was no room for two opinions. The answer was—ligature. With a tumour of such size and so acutely progressive, and with a lower circulation already so much embarassed, the application of pressure in the thigh—in the use of which we are unable, as yet, to avoid compression of the vein as well as of the artery—could have had but one result, and that not long deferred,—namely, the determination of gangrene in the entire limb.

3. The line of practice settled, another question emerged—"When shall the operation be performed?" The patient was in a most unsuitable condition, as has been described; and operation of any kind, under such circumstances, must be hazardous. In a few days, the tongue might be cleaned, the pulse calmed, the fever subdued, provided the usual means to such an end were not baffled by the increasing tumour and the unceasing and agonizing pain which it necessarily occasioned. That such would be the counteraction of antifebrile treatment was too plain; besides, the limb's circulation was becoming every hour more oppressed, and there was great risk too of sudden diffusion of the aneurism. Immediate operation, therefore, it was plain, afforded the only chance of relief, and it was accordingly performed without any delay.

4. It so happened that the steps of the operation proved remarkably easy; the vessel was readily and at once exposed, and in applying the ligature the vein was neither seen nor felt. In fact, the circumstances of the case happened to be such, as to enable the operator to tie the artery not only without difficulty, but also without fault.

5. Fully alive to the risk of gangrene, notwithstanding, I gave very strict orders as to the subsequent management of the limb. To that I believe nothing to be more hazardous than heat, stimulant friction, or any other means of exciting the vitality of the parts beyond what they are able to control or bear. Accordingly, I ordered that all such treatment should be abstained from, that the limb should be merely swathed in cotton wadding, that the man's ordinary diet should be continued, that aconite in the small doses should be given with a view to calm the heart's action, and that a hypnotic should be administered at bed-time.

6. The temperature of the limb fell immediately after the operation, and never rallied; *there was no reaction.* And when I saw the patient next day, I at once recognized on the sides and dorsum of the foot the peculiar measley mottled

appearance indicative of threatened gangrene. I directed the professional assistants to make gentle, very gentle, and brief friction on the parts, so as if possible to coax the superficial veins to resume the carriage of their contents. The limb was swathed thickly in the wadding, and placed close beside its fellow, so as to be benefited by its warmth. But all would not do. The gangrene slowly but steadily declared itself, with increasing coldness, want of sensation, lividity, phlyctænæ, &c.

7. What was now to be done? The ordinary practice is, in like circumstances, to amputate in the thigh, so soon as satisfied that gangrene has fairly set in. But there was an absence of aggravation in the constitutional symptoms; on the contrary, these were more favourable than before the operation; the local progress of the gangrene, too, was rather chronic than acute. And I resolved to wait.

8. Accordingly, a line of demarcation was observed gradually forming, encircling the limb about four inches above the ankle, sloping upwards in front till within about the same distance from the knee, and thus marking (what gangrene seldom does) a good line of flap in the event of nature's amputation being completed. Waiting for this line of separation deepening, and the patient remaining constitutionally well, an abscess formed in the sural mass; and this was evacuated by cutting in the dead parts, to avoid pain and loss of blood. The unhealthy purulent drain that followed, however, soon told upon the patient's strength, and amputation could be no longer delayed.

9. But where to amputate? Below the knee, much in the line of spontaneous separation, including part of the abscess in the flap? or in the lower part of the thigh? An examination of the aneurismal tumour determined this, by discovering that tumour still large, reddened on the surface, and distinctly fluctuating. The suspicion was, that suppuration had taken place within the cyst; and if a low amputation were even to prove successful, there would be risk from opening of the popliteal tumour in the progress of the abscess—risk by certain hectic, and probable bleeding too.

10. Amputation was therefore performed above the knee. Nothing unusual was observed in the course of the operation, excepting that the femoral tied crisp and gritty, from calcareous deposit in its coats. On examining the tumour, its contents were found altogether sanguineous, partly fluid and partly solid. The appearance of suppuration had been deceptive. The nerve was stretched and flattened very remarkably over the posterior aspect of the aneurism, which, indeed,

was made quite bilobular by its transit. And this very sufficiently accounted for the great pain endured previously to the deligation of the artery. The vein, too, was flattened and compressed as it passed over the swelling; but throughout its whole course in the amputated part, it was traced quite patulous, and free from all phlebitic sign.

11. The amputation was performed a month after the deligation of the femoral, and at the same time, of course, after the first invasion of gangrene. The ligature on the femoral had come away at the usual time; the wound had made a kindly progress, had ceased to discharge, and was almost completely cicatrized.

12. The prognosis of the amputation was necessarily most gloomy, the more especially as two large bed sores complicated the case sadly; one of them over the right trochanter being in extent sufficient to hold the fist, and deep enough to expose the tendon, in slough, of the glutæus maximus. Still he might struggle through, and he wanted nothing in the way of careful and skilful house management.

13. Some days after the operation, while the stump and other matters were progressing favourably, we were alarmed by discovering a pulsating tumour begun to form in the left ham. It was obviously another popliteal aneurism of recent development. The occurrence was carefully concealed from the patient, who all along had been mentally irritable and very anxious as to the result of his case. And my hope was, that when the stump was healed, the treatment by compression might be leisurely and cautiously applied to the new aneurism.

14. The growth of this, however, disappointed my expectations. It was as acute in its character as its predecessor in the opposite limb. For, on the 26th day after the amputation, when the stump was all but healed, and the patient's stamina holding out a fair hope of recovery, the swelling of the lower limb had within twenty-four hours become so great, the pain so excruciating, and the tumour so large, as to render it very plain that deligation of the artery could not be for an hour delayed. The existence of the tumour was then made known to the patient. He bore the communication well, and with great good sense made no objection to the operation, which accordingly was forthwith performed.

15. This time the artery required a little more care in its isolation; but the ligature was passed easily enough, and without any injury to the venous or other tissues. The same precautions were taken as formerly against stimulation of the limb, and gangrene was entirely escaped from. On the

second or third day a slight discoloration appeared on the dorsum of the foot; but it was mere venous turgescence, and soon passed away under gentle friction.

16. On the fifth day after the operation, however, while the thigh wound was looking well, and all gave hope of a prosperous issue, diarrhœa occurred; on the seventh day it became greatly and suddenly exacerbated, with coldness, lividity, and general collapse; despite of all our efforts, his weak frame could not be brought to rally—he sank rapidly and died.

17. From prejudice on the part of relatives, the *post mortem* examination was hurried and imperfect. It was certainly unexpected to find the larger arteries comparatively free from degeneration of the coats, while during life the aneurismal diathesis had so prominently affected the lower limbs, as evidenced by aneurism in both hams, and the crisp condition of the right femoral in the line of amputation. Had the patient's laborious avocations, with great, habitual, and especial strain upon the lower limbs, determined the arterial degeneration there, to the comparative exemption of the rest of the body? In other words, was the predisposing cause of aneurism in this instance of a local rather than of a constitutional kind? Or did the syphilis predispose generally, while the laborious avocation concentrated the evil mainly on the lower limbs? Be that as it may, the exciting cause of the first aneurism doubtless was some heavy strain of the part during the man's ordinary work, while that of the second might have been a sudden and rough movement of the other limb during amputation of the right thigh.

18. In *post mortem* examination, the condition of the tied left femoral was also ascertained. The ligature (on the seventh day after application) was found well advanced in separating; the vein clear; the artery plugged; and all things as they ought to be. The left popliteal tumour, too, was examined; like the other, but smaller; the nerve and vein flattened and stretched; the contents mainly solid. On its cardiac end a fusiform dilatation—*a third tumour*—had begun to form; or perhaps it may be considered as a sort of appendage to the main enlargement. A careful examination was also made of the right femoral artery at the site of deligation, more especially as to the state of the attendant vein, and all was found normal there.

19. It is natural to inquire how far this case tells either for or against the treatment of aneurism by ligature. It seems to say something on both sides of the question. *Against*, thus far:—as going to prove that sudden arrest of the arterial circulation, at the Hunterian point, is *of itself* capable of pro-

ducing gangrene, when the popliteal tumour is large, the circulation of the limb already embarrassed, and the general powers of the patient weak. In this case, the conclusion seems inevitable that the gangrene was determined entirely by the deligation of the femoral. The temperature sank at once, and *never rallied;* the gangrene, indeed, may be said to have begun and steadily and slowly advanced from the instant of the operation; and besides, the progress of the ligature—separating without casualty on the 15th day—and the subsequent dissection of the parts, shewed that no injury had been inflicted on any tissue other than the arterial—at least no injury of any such kind as could have at all influenced the invasion of gangrene. *But the case speaks also for the ligature,* inasmuch as even with the existence of so manifest a degeneration of the arterial tissue in both limbs, the ligature fairly applied did not induce secondary hemorrhage, or any other casualty of the wound; and the ligature was applied, be it remembered, at three separate points of the femoral artery—in the amputation, as well as in the two operations for aneurism.

20. I am far from casting blame on the ligature. No other treatment could have availed. From the first the chance of success was but a poor one; but such as it was, it depended wholly on the ligature; and this did all that it could be expected to do.

So did surgery. There is no room for blaming our art. The wonder is not so much that surgery should have failed to cure; we are called upon rather to be thankful that so near an approach to cure may by the ordinary practice of our art be obtained, even in a combination and sequence of circumstances so remarkably and obstinately unfavourable.

It will not do to reserve this, or any other operation, for favourable cases alone, to the exclusion of all in which the prospect of success is not fair, or almost certain. Wherever a chance exists and the patient desires to have the benefit of it, however slight that chance may be, it is at once the duty and the privilege of surgery to afford its aid; more especially when the operation, as in aneurism, does not happen to be formidable in itself, by shock or mutilation. In such difficult and all but desperate cases, the reputation of the surgeon and the credit of his art must both be set aside for the time, in favour of suffering humanity.

ART. II.—*Experiments on the Communicability of Cholera to the Lower Animals.*

By W. LAUDER LINDSAY, M.D., late Resident Physician of the Surgeon Square Cholera Hospital, Edinburgh.

(Continued from page 302.)

In addition to the animals already mentioned, the following dogs were employed as the subjects of experiment, viz.:—

Dog No. 5.—A male; black retriever; a large, powerful, healthy, active animal; weighing about 56 lb., and having the following measurements:—

Length of head	9	inches.
... body	34	...
Greatest girth of head	20	...
... thorax	31	...
... abdomen	24	...

Dog No. 6.—A male; mongrel Scotch terrier; 3 months old; lively, healthy, active; weight 8¾ lb.; measurements as follow:—

Length of head	5	inches.
... body	19	...
Greatest girth of head	11	...
... thorax	13	...
... abdomen	11	...

Dog No. 7.—A female; mongrel bull terrier; had been previously subjected to several experiments, with puerperal matter injected into the vagina, without effect; weight 18 lb.; measurements as follow:—

Length of head.........about	6½	inches.
... body	24	...
Girth of head	14	...
... thorax	19	...
... abdomen	15	...

Dog No. 8.—A male; young mongrel terrier; timorous, quiet; in very poor condition, (apparently starved); weight 7 lb. 11 oz.; measurements as follow:—

Length of head	6	inches.
... body	18	...
Girth of head	10½	...
... thorax	14	...
... abdomen	10½	...

The experiments were begun on 24th October, and continued, with frequent intermissions, according as patients were admitted and discharged, till 16th December. The substances on which the animals were fed during this period were chiefly the stools, vomit, and urine, passed or ejected in all stages of cholera; they were given in all states of freshness and putridity, concentration and dilution. Occasionally I used the blood and various other fluids and solids of fatal cases—removed at the *post mortem* examinations; these also were exhibited in different conditions of freshness, but generally as soon after death as convenient. When the opportunity occurred, I also fed them on the blood and solids of Dogs 2 and 3, which died of cholera, as has been already mentioned; so that all the media usually supposed to contain or convey the cholera poison were thus employed at different times, and to various extents. On one occasion only, did I inject into the cellular tissue, and then with negative results. In order duly to appreciate the effects of these substances on the canine and feline economy, it is necessary not only to give a brief sketch of their chemical and histological characters, but also to glance at the more prominent features—symptomalogical and pathological—of the cases that were thus indirectly made the media of experiment. Reserving comment, I proceed at once with these details, in a greatly abridged form :—

CASE I.—I. M., a woman, aged 55; *fatal*, in the "*consecutive fever*," three days after her seizure. Had a previous attack of cholera, which was nearly fatal, during the epidemic of 1848–9. Collapse and reaction were very mild; the fever very insidious and typhoid. The principal *pathological features* were—ecchymoses on the heart, stomach, intestines, the pelvic and lumbar cellular tissue and ovaries; bronchitis; fatty liver; lesions of the aortic and mitral valves and kidneys; absence of any cyanotic condition of the skin.

Collapse vomit resembled bran and water agitated together; was of a dark-brown colour; acid; slightly albuminous; of sp. gr. 1018; had little or no odour; scum consisted of a frothy, flocculent matter. Under the microscope, the vomit-sediment contained a quantity of mucus in the form of delicately striated hyaline bands; epithelium scales; a number of the so-called "cholera corpuscles," or "annular bodies;"[1] a few phosphates; fragments of muscular fibre, and of vegetable tissues; oil globules, and other food-dèbris.

---

[1] For a description of the forms which it has occurred to myself to observe in cholera, *vide* "Clinical notes on Cholera," Association Medical Journal, 14th April 1854.

The matter vomited, while *reaction* was passing into *fever*, consisted almost entirely of mucus, and contained a large quantity of pavement epithelium; a few starch granules; cells from the parenchyma, and fragments of the epidermis of various vegetables, and oil globules. It was acid, slightly albuminous, and gave a biliary reaction.

The *collapse stools* had the same characters as those of the diarrhœa, which in this, as in many other cases, preceded the cholera, viz.:—a pea-soup colour and consistence; a bran-like sediment; sp. gr. 1008; they were alkaline and albuminous, and gave no distinctly *biliary* reaction[1] with nitric acid, though they had a biliary colour. Sediment contained a considerable number of "annular bodies;" a few prismatic phosphates; starch cells; and fragments of muscular fibre, and other forms of food-dèbris.

The stools subsequently passed (during *reaction* and *fever*) were of a chocolate colour; and contained, under the microscope, blood corpuscles. On standing, an oily scum soon formed, consisting of oil globules, entangling a few phosphates. They very speedily became fetid.

Case II.—M. R., a woman, aged 37; *recovery*. Admitted in deep *collapse;* passed through all the phases of the disease in a marked form, and was discharged well in sixteen days. Prominent features of the collapse were vomiting and cramps; of the "consecutive fever," (or the typhoid stage), delirium and a tendency to coma; there was suppression of urine for three days after admission. During the convalescence, a scarlatinoid eruption appeared on the face, followed by desquamation of the cuticle as after scarlatina.

Collapse vomit whey-like; somewhat resembled the "rice-water" stools; acid; albuminous; had slight odour of whisky; sediment consisted chiefly of oatmeal, and, under the microscope, showed a large quantity of starch, oil globules, mucus, and mucus-corpuscles; muscular fibre; pavement epithelium; "annular bodies;" compound granular bodies, and various forms of vegetable dèbris.

Collapse stools had the usual characters of the "rice-water" evacuations of cholera; alkaline; albuminous; sp. gr. 1010; devoid of fetor; gave a pink reaction with heat and nitric acid. Flocculent sediment consisted of delicately striated hyaline bands of mucus, covered by mucous and other granular corpuscles, which, according as water and reagents were added, resemble pus, lymph, or white blood-corpuscles or free epithelial nuclei.

---

[1] For my views on the "presence of *bile* in the cholera evacuations," *vide* "Clinical notes on Cholera," Association Medical Journal, March 10, 1854.

CASE III.—H. H., a woman, æt. 44; *fatal* in *collapse* about eight hours after admission. The most prominent symptoms were cramps, vomiting, and purging; vox cholerica; and complete suppression of urine. Chief *pathological* features were—considerable livor of the body; a sodden condition of the extremities of the fingers; dryness of the muscles and viscera; intestinal and uterine hyperæmia "Psorenterie;" empty, contracted state of bladder; exudation of a glutinous mucus on the peritoneal surface of the intestines; ʒi serum in pericardium; distension of stomach, with thinning, softening, and exuviation of its mucous membrane along the greater curvature; cysts of Fallopian tubes; hemorrhagic extravasation in right ovary; absence of ecchymosis of any of the internal viscera.

Collapse stools had usual characters of "rice-water" evacuations, and contained, in addition, a quantity of pavement epithelium (probably from leucorrhœal matter), a few "annular bodies," and some food-dèbris. They were of sp. gr. 1008; slightly albuminous; and gave a greenish-yellow reaction with heat and nitric acid.

Collapse vomit a brownish-yellow, frothy fluid; acid; of sp. gr. 1005; sediment resembles bran, and contains phosphates, epithelium, oil globules, "annular bodies," and food-dèbris.

CASE IV.—A. W., a girl, æt. 7½; *recovery*. Admitted in advanced *collapse*—almost *in articulo mortis*—7½ hours after her seizure; passed through all the stages of the disease, and discharged well in twenty-one days. Collapse was very short, but severe; the typhoid stage tedious, but well marked. During convalescence, or towards the end of the typhoid stage, the "cholera-exanthem," in the form of an urticario-rubeoloid eruption, appeared all over the body; co-existent with its duration there was a considerable deposit of uric acid in the urine.

Collapse stool was "rice-watery," alkaline, albuminous, and gave a pink reaction with heat and nitric acid. Quantity ℥ii.

The next stool, passed shortly after, was of a light brownish-yellow colour; slightly fetid; alkaline; had a scanty, branny sediment. The third stool after admission resembled pea-soup in character and consistence; the microscopical difference consisted in the substitution of food-dèbris for the delicate colourless mucus of the collapse stools.

Collapse vomit was of a dirty grayish-brown colour; very turbid; acid; slightly albuminous; reaction with heat and nitric acid greenish-yellow. Under the microscope, the sediment contained a large quantity of pavement epithelium, and dark granular matter; no appreciable dèbris of food.

First urine passed after admission was sp. gr. 1015; acid; albuminous; loaded with urates; reaction with heat and nitric acid, deep orange-red; with nitric acid, cold, a pink. Sediment contained renal casts, transparent, granular, and oily; dumb-bell oxalates; epithelium; "annular bodies," as in the stools, and compound granular bodies; contained very little urea.

CASE V.—A. F., a man, æt. 48. *Fatal* in typhoid stage seven days after admission. Collapse and reaction comparatively mild; typhoid stage insidious and asthenic. Prominent symptoms were—vox cholerica, suppression of urine for two days, frequent but mild hiccup, great restlessness, insomnia, semicoma, low-muttering delirium, involuntary evacuations in bed, picking of the bed-clothes, black sordes on teeth, dry brown tongue, and extreme prostration. *Pathological* features were chiefly *negative*. Absence of marked ecchymosis; hyperæmia, or dryness of the viscera; emphysema and collapse of lungs, with broncho-pneumonia, recent and of a typhoid type; fatty kidneys; firm decolorized clots found in both sides of the heart; bladder contained two or three oz. urine, abounding in epithelium and renal casts; stomach and intestines contained a dark, greenish mucous fluid, having in a most intense degree the peculiar odour exhaled by the sweat and skin of cholera patients; its sediment consisted of mucus, softened mucous membrane and its elements (cylindrical epithelium, &c.), and food-dèbris.

Collapse vomit was of a dark chocolate colour; acid; sp. gr. 1014; had a scanty granular sediment of similar tint, and a frothy scum of a light pinkish tinge; reaction with heat and nitric acid greenish; scum contained a large quantity of *sarcinæ*, besides epithelium, starch, oil globules, and food-dèbris.

Stools passed during the *typhoid stage* were of a pea-soup character; alkaline, albuminous, fetid; of sp. gr. 1010; on standing, a scum collected on the surface, consisting of oil globules entangling a few phosphates.

Urine passed in typhoid stage was acid; sp. gr. 1012; albuminous; reaction with heat and nitric acid greenish-yellow. Sediment contained renal casts, octahedral oxalates and phosphates; and the evaporated residue consisted chiefly of common salt, in large, irregular cubes, and crystalline masses; plumose and stellate phosphates, globular and amorphous urates, and an oleo-resinous, viscid matter; contained a normal amount of *urea.*

CASE VI.—I. S., a girl, æt. 6½; *cholerine; recovery;* dismissed well on third day after admission; collapse mild, reaction complete; no typhoid stage.

Collapse vomit was of a light brownish-yellow colour; acid; slightly albuminous; sediment bran-like; reaction with heat and nitric acid greenish-yellow. Sediment contained a large quantity of echinus-like fatty bodies;[1] starch; epithelium perfect and shrunk; "annular bodies;" fragments of *acari* and vegetable tissues, phosphates, and oil globules.

CASE VII.—I. W., a woman, æt 38; *recovery;* admitted thirty-eight hours after seizure in deep *collapse;* passed through all the phases of cholera, and dismissed well on the eighteenth day. Prominent symptoms were—protracted and severe retching and vomiting; tendency to coma; delirium; involuntary evacuations in bed; suppression of urine; appearance of the so-called "cholera exanthem," as in Case IV., during the convalescence; and painful distension of the mammæ. She was nursing when seized, and her uterine functions had for some time been disordered.

Collapse stools possessed usual characters (mentioned in Case I.) of the "rice-water" evacuations, but contained a small proportion of "annular bodies" and food-dèbris; reaction of some specimens with heat and nitric acid pink, of others greenish-yellow.

Fever stools (*i.e.* passed during typhoid stage) were of a pea-soup character; alkaline; albuminous; sp. gr. 1010–15; with a dark-brown sediment on the surface; an oily scum collected on standing, containing a few pellets of concrete *fat,*[2] having a deep greenish-yellow tinge.

Collapse vomit first ejected was of the colour and consistence of whey; acid; sp. gr. 1008; slightly albuminous; reaction with heat and acid greenish-yellow. Branny sediment, contained pavement epithelium, *sarcina Goodsirii,* starch, "annular bodies," compound granular bodies, muscular fibre, potato cells, and other food-dèbris. On standing, an oily scum rapidly collected. Evaporated residue of the filtered fluid contained abundance of phosphates and common salt.

---

[1] For drawings of crystalline and other forms of fatty matter found in the vomit of cholera, *vide* paper by the author in "Medical Times, and Gazette," Aug. 5, 1854.

[2] For examples of the occurrence of fat and oil in cholera fæces, *vide* paper on the subject by the author in the Edinburgh Monthly Medical Journal, Aug. 1854.

Urine passed early in the *typhoid stage* was acid; albuminous; sp. gr. 1017; had a scanty, white, flocculent sediment, consisting chiefly of pus; but containing also renal casts; epithelium; dumb-bell, and octahedral oxalates; and compound granular bodies (the so-called "exudation corpuscles" of some authors); contained a very small amount of *urea;* reaction with heat and acid deep orange red; with acid alone pink. Evaporated residue contained a considerable quantity of phosphates, urates, and chloride of sodium.

CASE VIII.—A. S., a woman, aged 40. *Fatal* in *collapse* 18½ hours after admission. Prominent features were: severe cramps, involving not only the muscles of the feet and legs, but also those of the thigh, abdomen, and thorax; cyanotic condition of body; vox cholerica; and *post mortem* movements, accompanied by a rise in the temperature of the body. (The latter phenomena were comparatively common in the other fatal cases here noted.) Chief *pathological* conditions were: rigor mortis well marked; punctuate ecchymosis of heart and intestines; thickening, softening, and exuviation of the mucous membrane of the large intestines, the separated patches closely resembling in appearance flakes of dysenteric lymph; "Psorenterie" in ilium; muscles, pericardium and viscera dry; latter much collapsed; retrograde tubercle in lungs; recent bronchitis (or, more probably, a hyperæmic condition resembling it in its pathological phenomena); left ventricle of heart firmly contracted, and containing no blood; blood corpuscles normal. Contents of *small intestines*, when filtered, were alkaline, highly albuminous, sp. gr. 1015; reaction with nitric or muriatic acid, in the cold, pink; sediment consisted chiefly of mucus and the disintegrated elements of mucous membrane, but contained also "annular bodies" and blood corpuscles. Contents of *large intestines* were similar in character, but contained a larger amount of blood, and a slight admixture of semifluid fæces, of a dark brownish colour.

Collapse vomit was of a pinkish-red, acid, albuminous; reaction with heat and acid greenish-yellow, with nitric or muriatic acid, cold, pink. Sediment, which occupied three-fourths of the whole bulk of the fluid, consisted of undigested articles of food, chiefly potatoes, beef, and the elements of broth, on which she had dined shortly before admission. The microscope detected, in the finer part of the sediment, the parenchymatous cells of the potato, carrot, turnip, cabbage, and onion; fragments of the epidermis of these vegetables, and of barley grains; starch from bread, potatoes, &c.; isolated spirals and cells of the same vegetables; epithelium, both pavement and cylindrical; mucus and mucus-corpuscles; *sarcinæ;* fatty and oily matters. Fragments of several *acari*, and several large masses of *concrete fat*, were found in the oily scum. These fatty masses consisted of vesicles, each containing a radiating, crystalline mass of margaric acid.

Collapse stools had the ordinary "rice-water" characters, except that the sediment had a brownish tinge, and contained "annular bodies," starch, muscular fibre, and other food-dèbris; reaction with heat and acid pink. The second and third stools passed were fetid; resembled very thin gruel; alkaline and albuminous; reaction with heat and acid *nil.*

CASE IX.—J. W., a man, aged 34. *Recovery;* admitted in mild *collapse;* passed through all the stages of the disease, and dismissed well on the 18th day. The convalescence was marked by the appearance, chiefly on the face, of the "cholera-exanthem," in the form of a scarlatinoid eruption, followed by slight cuticular desquamation.

Fever urine was acid, non-albuminous; sp. gr. 1010; contained a small amount of urea; reaction with heat and acid pinkish-red; evaporated residue contained phosphates, both prismatic, plumose and stellate, and chloride of sodium.

CASE X.—M. F., a woman, aged 40. *Fatal* in consecutive fever on the second day. Admitted in deep *collapse* eleven hours after seizure. Collapse stage was short; reaction appeared sthenic and favourable; fever very typhoid, and marked by suppression of urine, great conjunctival injection, stupor and tendency to coma. Chief *pathological phenomena* were: blood contained in the heart and large vessels dark and grumous, showed no tendency to coagulation; alteration of the blood-corpuscles, the red disks being scarcely recognisable as such, but resembling pus, both before and after the addition of acetic acid;[1] bladder contracted, empty; bile healthy; *valvulæ conniventes* of duodenum and jejunum deeply stained with bile; ecchymosis on pericardium and heart, pleuræ, diaphragm, stomach, intestines, broad ligament of uterus and right ovary. Bronchitis, emphysema, and collapse; incipient Bright's disease of kidneys; liver slightly fatty; rugæ of stomach very prominent, and mucous membrane thickened, softened, and pulpy; exuviation of mucous membrane of large intestines in patches of various sizes, resembling in general appearance dysenteric ulcers.

---

[1] *Vide* paper in "Monthly Medical Journal" already referred to.

Contents of *small intestines* had the characters of reaction and fever stools; of pea-soup character; fetid; sp. gr. 1012–15; alkaline, albuminous; reaction with heat and acid orange-red. Sediment contained little cylindrical epithelium, but the intestinal villi were all nude.

Contents of *large intestines* were similar in character, but darker in colour, and of a distinctly bloody tinge.

Collapse stools "rice-watery."

Fever Stools of pea-soup character, having a branny sediment; alkaline, fetid, albuminous; sp. gr. 1012; reaction with heat and acid orange-red; the scum, which speedily collected on the surface, contained several masses of concrete *fat*, as in Case VIII., and a few phosphates.

Collapse vomit "rice-watery," alkaline; slightly albuminous; sp. gr. 1008; strong odour of whisky; reaction with heat and acid greenish-yellow. Microscopical character of flocculent sediment resembled that of the "rice-water" stools, with the addition of pavement epithelium, "annular bodies," and food-dèbris.

CASE XI.—J. M'G., a man, æt. 45; *fatal* in *collapse* eight hours after admission. Cramps of the muscles of the lower extremities and abdomen, and livor of the body, were marked symptoms. *Pathological* phenomena were: absence of ecchymosis and "psorenterie;" dryness of muscles, pericardium and viscera; general hyperæmia of viscera; contracted and empty state of bladder; abundance of apparently healthy bile in gall bladder; thickening and softening of mucous membrane of stomach and intestines; muscular substance of heart flabby, and of a dull brown colour; left ventricle firmly contracted, right ventricle flaccid and full of grumous blood; firmly intertwined among the *Columnæ carneæ* were a few decolorized clots.

Contents of *small* intestines consisted chiefly of mucous flocculi; had a chocolate or *café au lait* colour from bloody admixture; the fluid portion was slightly fetid; alkaline; densely albuminous, and sp. gr. 1010; a phosphatic scum rapidly formed on the surface on standing. The microscopical characters of the flocculi resembled those of the "rice-water" stools, with the addition of cylindrical epithelium, isolated and in groups connected at their apices by a continuous membrane. Reaction with heat and acid greenish-yellow.

Contents of *large* intestines are similar in character, but darker in colour, from containing a larger amount of blood; there were comparatively few cylindrical epithelium cells, but on the other hand, a considerable quantity of food-dèbris, annular and compound granular bodies; less albuminous; reaction with heat and acid brownish-red.

Collapse stools "rice watery;" sediment contained a few annular and compound granular bodies.

Collapse vomit of a pinkish colour, acid, non-albuminous; sp. gr. 1005-10; reaction with heat and acid, pink, which by prolongation of the heat was converted into greenish-yellow; frothy mucous scum contained a large quantity of *sarcinæ* and epithelium; sediment consists chiefly of undigested masses of potatoes and beef, and their disintegrated elements.

CASE XII.—Caroline Macaulay, a woman, æt. 34; *fatal* in the acme of the "*consecutive fever*," in three days. Admitted in collapse, three days after first seizure; was four months advanced in utero-gestation. Prominent symptoms were: contracted pupils, great conjunctival injection, restless insomnia, tendency to coma, delirium, involuntary evacuations in bed, suppression of urine, laborious respiration, and threatened abortion. Chief *pathological* conditions were: punctuate ecchymosis of heart; absence of marked hyperæmia; stomach and intestines apparently normal, liver slightly, and kidneys very fatty; blood normal.

Contents of intestines resembled the stools passed during life, but were darker in colour, having a bloody tinge.

Reaction and *fever stools* of pea-soup colour and consistence, alkaline, fetid, slightly albuminous; sp. gr. 1015; sediment presents the ordinary microscopic characters of the "rice-water" flocculi, with the addition of a quantity of food-dèbris of a bright greenish-yellow (biliary) tinge. The matters evacuated as the fatal termination approached were darker in colour, of more uniform consistence (no separation of a sediment), more fetid, and contained "annular bodies" and phosphates, in addition to a larger amount of food-dèbris.

Collapse vomits. First was of a grass-green colour, acid, non-albuminous; sp. gr. 1005; clear; sediment, which was scanty and flocculent, contained epithelium, starch, and vegetable dèbris, mucus, and oil globules. Decolorized by nitric acid.

Second and third were of an ale colour, neutral, devoid of odour; sp. gr. 1010; non-albuminous; turbid; reaction with heat and acid brownish-red.

Fourth was of a dark steel-gray colour, slightly acid, devoid of odour; sediment contained annular bodies, mucus-corpuscles, epithelium, and vegetable dèbris.

CASE XIII.—W. L., a man, æt. 42; *fatal* in *consecutive fever* in six days after admission in collapse. Reaction apparently sthenic and favourable; fever insidious and typhoid. Chief *pathological* conditions were: dryness of the muscles and viscera; ecchymosis of the pleura, heart, and intestines; general hyperæmia of the viscera; softening and thickening of the intestinal mucous membrane; red blood corpuscles normal, but there was an increase in the proportion of white corpuscles.

Collapse stools "rice-watery;" reaction with heat and acid pink, passing into greenish-yellow on prolongation of the heat.

Reaction and fever stools chocolate-coloured, alkaline, fetid, albuminous; reaction with heat and acid orange-red.

Collapse vomits acid, of very low density, non-albuminous, of various shades of whey-colour, greenish-yellow, brownish, reddish, and gray; reaction with heat and acid greenish-yellow; addition of acid caused considerable effervescence. Sediment contained mucus and mucus-corpuscles, epithelium, annular bodies; a few phosphates and crystals of chloride of sodium; fatty and oily matters to various extents. In some specimens there was a number of granular corpuscles having single or double nuclei, and otherwise resembling pus.

CASE XIV.—J. I., a boy, æt. 2. *Recovery* in ten days after admission. Attack followed protracted convalescence from rubeola. Collapse short; subsequent symptoms more rubeoloid than choleraic in character.

CASE XV.—A. B., a man, æt. 49. *Fatal* in four days in the consecutive *fever*, which was very insidious and typhoid. Chief *pathological* features were: softening and exuviation of the mucous membrane, and general arborescent hyperæmia of the intestines; partial ecchymosis of the intestines; first stage of typhoid pneumonia; dilatation and hypertrophy of the heart; lesion of the aortic valves; renal and hepatic epithelium slightly fatty.

CASE XVI.—J. B., a woman, æt. 53. *Fatal* in advanced *collapse* three hours and a half after admission. Chief *pathological* conditions were: absence of livor of the surface; marked dryness of the viscera, or ecchymosis in any part of the body; rosy hyperæmia of intestines; thickening and sodden condition of their mucous membrane; their contents "ricy" mucus, slightly tinged with blood; retroverted uterus; mucous polypi of *cervix uteri;* fatty liver.

Contents of *stomach* about ℥iii, of a dirty brownish-yellow colour, turbid, acid, albuminous; reaction (of filtered fluid) with heat and acid light greenish-yellow.

Contents of *large* intestines cream-coloured, having here and there a bloody tinge; alkaline, fetid, densely albuminous; reaction of filtered fluid portion with heat and acid orange-red. Sediment contained cylindrical epithelium, mucus, and mucous and other granular corpuscles.

Contents of *small* intestines similar in character; contained quantities of small shreds of softened mucous membrane.

I now proceed to the detail of the experiments with the solids and fluids of the patients just above referred to; beginning, as I have already stated, on Oct. 24th.

Oct. 24.—Dog No. 1 and the cat were introduced into the experiment room. Both were at first very shy; and, until impelled by hunger, they refused food; they had been starved on the previous day in order to insure some degree of appetite.

No patients in hospital.[1] Case III. had been *fatal* in *collapse* on the 9th; and Case I. in "*consecutive fever*," on the 11th curt. Case II. was dismissed recovered on the 19th curt.

In the morning gave the animals the collapse and reaction vomit, and in the afternoon the collapse and reaction stools of Case I.; the former were chiefly consumed by the cat, the latter by the dog.

Oct. 25.—*No* new cases admitted. The floor of the room was covered by a considerable quantity of fæces; those of the dog being semisolid, and of a dark brownish-green colour; those of the cat more fluid, of a grayish tint on the surface, and brownish-yellow below. Under the microscope they presented the characters of healthy fæces. Both animals were lively and well, and anxious to escape, especially the cat.

In the morning gave the fever vomit, and in the afternoon the fever stools of Case I. Both animals exhibited a better appetite, especially the dog.

Oct. 26.—*No* new cases. Both well and lively. Fæces slightly more fluid and abundant; floor covered with a quantity of urine. Effluvia from the evacuations had a most disagreeable odour. Room cleaned, and animals turned loose for an hour, but allowed no other food.

---

[1] By this I mean *no* cases of genuine cholera. There were occasionally admitted patients labouring under simple, but excessive, or suspicious diarrhœa, as well as cases simulating cholera in some of their features, *e. g.*, colic, cramps, vomiting, and purging, induced by intoxication and exposure to cold.

In the morning gave the collapse vomit, and in the afternoon the collapse stools of Case II.

Oct. 27.—Cases IV. and IX. admitted in collapse. Both well and lively. Fæces still more fluid, partly from admixture with urine, but not otherwise altered. Dog ate with avidity, and consumed proportionally a much larger quantity than the cat. The stench in the room necessitated its being cleaned and ventilated during the day. The animals were let loose on the ground floor of the hospital.

In the morning gave the collapse stools of Case III.

Oct. 28.—Cases V. and VII. admitted in collapse; Cases IV. and IX, in reaction, passing into fever. Animals well, very anxious to escape.

In the morning gave the collapse vomit of Case III.

Oct. 29.—4 cases under treatment; 1 convalescent from cholerine, and 3 in the "consecutive fever" of cholera.

In the morning gave the collapse stools of Case IV.

Oct. 30.—Case VII. admitted. Animals *in statu quo*, and fæces still very fluid, but, along with urine, comparatively normal.

Gave collapse vomit of Case IV.

Oct. 31.—Case VIII. admitted in collapse. Case V. dismissed recovered. Animals continue well.

Gave collapse vomit of Case VI.

Nov. 1.—Case VIII. fatal in collapse. Four "consecutive fever" cases remain under treatment. Animals well; room cleaned, and animals let loose for the greater part of the day.

Gave collapse vomit of Case V.

Nov. 2.—*No* new cases admitted. Animals well.

Gave collapse stools of Case V.

Nov. 3.—*No* new cases. No perceptible change on the animals; fæces still fluid, and urine abundant.

Gave collapse vomit of Case VII.

Nov. 4.—Case V. fatal in typhoid stage. Three fever[1] cases remain under treatment. Animals well.

Gave collapse vomit of Case VIII.

Nov. 5—*No* new cases. Animals well; fæces frequently differed in colour and consistence; their usual fluidity indicated a slight diarrhœa.

Gave about a pint of blood, removed from the heart and great vessels of Case VIII. at the *post mortem* examination on 2d curt. It was almost wholly consumed by the dog.

---

[1] In using the words or terms "fever," "consecutive fever," "rice-water," "serous stools," &c., I do not wish to indicate a belief in any particular theory or view of the nature of cholera, or any resemblance which I can trace in the stools. I employ them merely as the phrases ordinarily made use of to signify certain stages or phenomena of the disease.

Nov. 6.—*No* new cases. Both animals very quiet, and evidently ill. Appear to have had severe diarrhœa; fæces of dog tawny in colour and consistence; urine abundant.

In morning, gave contents of *small* intestines of Case VIII., removed at the *sectio cadaveris;* they were chiefly swallowed by the dog.

In afternoon, gave contents of *large* intestines of Case VIII. This was followed by a continuance of severe purging; the fæces of the dog were very fluid, but of a brownish-green (biliary) tint; the microscopical characters did not essentially differ from those of more solid and normal evacuations. The substances detected by the microscope usually consisted of the matters contained in the evacuations on which the animals were fed, many of them passing rapidly through the system of the dog and cat apparently little altered. There was no appearance of mucous flocculi in even the most fluid of the evacuations.

About 11 P.M. ℥ii. of the heart-blood of Case VIII., not defibrinated, and partially clotted—removed at the *sectio*—was *injected* by a common syringe into the cellular tissue of the thigh of each of the two animals. Immediately after the operation they scampered away apparently in good health.

Nov. 7.—*No* new cases; 3 cholera convalescents under treatment. Room cleaned and ventilated. In afternoon, dog No. 2 introduced; it was shy and timorous, refusing food, but withal lively and well.

Dog No. 1 and cat better; appeared in perfect health; the former gamboled very friskily, the latter as usual made repeated sly attempts to escape in search of more agreeable food, a warmer lodging, and a more pleasant companion. Floor covered with very liquid but dark coloured fæces, and with limpid urine; stench very strong.

Gave the urine of Cases IV., VII. and IX., concentrated nearly to dryness, mixed with milk and a little porridge.

Nov. 8.—*No* new cases. Milk and greater portion of the porridge had been consumed, but the urine-sediment had scarcely been touched. No appreciable difference in the animals. Gave no other food till the urine should be licked up. In the evening a little had been licked by the dogs, the cat having probably had the lion's share of the porridge and milk. By virtue of her claws, Grimalkin usually managed to have a selection of the viands, keeping the dogs in the background till she herself was served.

Nov. 9.—*No* new cases admitted. Diarrhœa continued in a mitigated form; animals almost *in statu quo*. Dog No. 2 had swallowed very little. Urine not consumed.

Gave the collapse stools of Case VIII., passed on 31st ult. and 1st curt.

Nov. 10.—*No* new cases. Dog No. 2 succeeded in making her escape; all animals apparently well.

Gave the filtered fluid portion of the fever stools of Case V., passed on 28th ult.

Nov. 11.—*No* new case. Animals *in statu quo*.

Gave about oj. of the fluid portion of the collapse stools of Case II., passed on 3d ult., evaporated to dryness, and mixed with some milk and porridge.

Nov. 12.—Case X. admitted. 3 convalescents still remain in hospital. Animals well.

Gave oj. of fluid portion of collapse vomit of Case II. evacuated on 3d and 4th ult., concentrated and mixed with milk.

Nov. 13.—Case XI. admitted, and *fatal* shortly after in collapse; 1 fever case and 3 convalescents remained under treatment. Animals still comparatively well; fæces still fluid.

Gave collapse stools of Case X.

Nov. 14.—Case X. *fatal* in consecutive fever; Case IX. dismissed well; 2 convalescents remain. Dog No. 1 made its escape yesterday afternoon, but was re-captured this morning.[1]

Gave collapse vomit of Case X.

Nov. 15.—*No* new cases.

Gave collapse stools of Case XI.

Nov. 16.—*No* new cases. Dog No. 2 re-captured and re-introduced; still sly and timorous. The semifluid, but still dark-coloured, fæces of the dog No. 1 were again examined by the microscope, and found to contain, in addition to vegetable dèbris, crystals of phosphates and chloride of sodium, grains of siliceous earth, and entozoon-ova (probably of an *ascaris*) in various stages of development.

Gave contents of the intestines of Case XI., removed at the *sectio* 2 days before.

Nov. 17.—Cases IV. and VII. discharged recovered. *No* cases remain in the hospital.

---

[1] When I do not specially mention the condition of the animals, or the characters of the fæces, it is to be understood that no appreciable or note-worthy alteration from the normal state was observed.

Gave about oj. of the heart-blood of Case XI., removed at the *sectio* 3 days before.

Nov. 18.—*No* new cases. Animals continued well. Fæces resembled soft soap in appearance, others resembled treacle, and were probably passed by dog No. 1, which appeared chiefly to have consumed the blood supplied yesterday. The microscope detected in the latter a considerable quantity of entozoon-ova, hairs, phosphates and chloride of sodium, vegetable dèbris, "annular bodies" such as occur in the cholera evacuations of man, and granular matter of a dark green colour. Most of the so-called "annular bodies," or "cholera corpuscles," appeared to suffer disintegration or great alteration by passing through the intestinal system of the dog and cat; others, however, were discharged unaltered, *e.g.* those I have for convenience sake denominated "gonidic."

Gave about ℥vi. of blood, taken at a *sectio* 2 days before from the heart and lungs of Case X.

In afternoon, gave contents of the intestines of the same case, also removed after death.

Nov. 19.—*No* new cases. Dog No. 3 introduced in good condition, quiet, shy; lay in a corner apart from the others; the cat also avoided canine society; but dogs 1 and 2 generally nestled together in some quiet corner; they all lived together, however, in comparative harmony.

Gave collapse vomit of Case XI., evacuated 6 days before.

Nov. 20.—*No* new cases. Microscopical examination of the fæces revealed no change. Animals all well.

Gave fever stools of Case X., passed 6 days before.

Nov. 21.—Case XII. admitted in collapse; no other cases in hospital. The body-clothes of this case were heaped up in a corner of the experiment room. Animals well; fæces still fluid, but otherwise normal.

In morning, gave about oj. of fluid portion of collapse vomit of Case VIII., concentrated by evaporation.

In afternoon, gave above ℥x. of the fluid portion of the collapse stools of Case XI, also concentrated by evaporation.

Nov. 22.—1 *fever* case in hospital (*i.e.*, a patient in the "consecutive fever," or typhoid *stage* of cholera). The animals were in the condition and positions already described.[1]

Gave reaction and fever stools of Case XII.

Nov. 23.—*No* new cases. Animals well.

---

[1] *Vide* page 289 *et seq.* of the Journal for the *special* experiments with the fomites, which ought to have been described at this stage of the *general* experiments.

Gave collapse vomit of Case XII.

Nov. 24.—Case XII.—*Fatal* in acme of consecutive fever. Body-clothes removed from experiment-room, and bed-clothes in which the patient had died, and which were saturated by her discharges, were substituted.[1]

Gave fever stools of Case XII.

Nov. 25.—*No* cases in hospital. Animals apparently in good health. Gave contents of the intestines of Case XII., removed at the *sectio* this afternoon. Room cleaned and ventilated during the day.

Nov. 26.—*No* new cases. Animals still well; confined all day. Gave about ℥x. blood, taken from the heart and great vessels of Case XII. at the *sectio* yesterday.

In afternoon, gave about oj. of the fluid portion of the fever stools of Case X., concentrated to one-tenth by evaporation.

Nov. 27.—Dog No. 2 died of cholera; remaining animals all apparently labouring under the disease in different degrees. Fæces, &c., as already described. As the matters supplied yesterday had not been consumed, I gave them no further food.

No new cases admitted.

Nov. 28.—Dog No. 3 died of cholera. Dog No. 1, and cat, still very ill. Cleaned and ventilated the room; turned loose the surviving animals, and gave them a supply of fresh milk and porridge.

Nov. 29.—Dog and cat still very much debilitated, and labouring under diarrhœa; allowed to run at large in the grounds surrounding the hospital, and fed on milk, porridge, bread, meat, &c. Dog No. 4 introduced.

Nov. 30.—*No* new cases. Animals all comparatively well and lively. Again confined them in the experiment-room, and gave them, as food, portions of the blood, fat, flesh, bones, and viscera of dogs Nos. 2 and 3, removed at the *post mortem* examinations on the 27th and 28th current; were chiefly consumed by dog No. 1. Dog No. 5 introduced.

Dec. 1.—*No* new cases. Dog No. 1 very lively; No. 4 trembling, quiet, and timorous; No. 5 lively, *whining and barking* from anxiety to escape. Cat very quiet; all well; none affected with diarrhœa. Most of the fæces were semisolid, and of a dark brownish-green colour; some were semifluid and tarry. Urine was abundant, and normal.

Experiments intermitted till 7th.

Dec. 6.—Cases XIII. and XIV. admitted in *collapse*. Animals in good health; had been running about in the hospital grounds daily, and fed on porridge, bread, &c. Dog No. 6 obtained.

---

[1] For characters of the fæces and urine of the animals, *vide* page 288.

Dec. 7.—*No* new cases. Dogs (not the cat) confined in experiment-room in the evening and during the night, and body-clothes of Cases XIII. and XIV. were thrown in a heap in a corner. Dogs Nos. 1 and 4 slept among these clothes, while Nos. 5 and 6 slept at door.

Dec. 8.—All well. Animals let loose during day, and fed on porridge and milk. Room ventilated and cleaned. Fæces and urine normal. Animals were shut up for the night in a damp, dark cellar of an untenanted house a few yards off.

Dec. 9.—*No* new cases. Animals well; confined for a few evenings in an under-ground room of the house above mentioned. In addition to some flesh, porridge, and milk, gave them collapse stools of Case XIII.

Dec. 10.—*No* new cases. All well. Stools had been consumed only partially, and that by dog No. 1. Dogs Nos. 7 and 8 introduced. Gave collapse vomit in the morning, and reaction and fever stools in the afternoon, of Case XIII.

Dec. 11.—CASE XV. admitted in collapse; two patients in consecutive fever stage remained under treatment. Animals well; fed on porridge, meat, and general kitchen refuse, and allowed to run about in the open air all day; confined in the room during night, the body-clothes of Case XV. being thrown into a corner thereof.

Dec. 12.—Case XIII. fatal in consecutive fever; two fever cases were still under treatment. Animals well; bed-clothes of Case XIII., in which he died, and soiled by his evacuations, were introduced into the room at night.

Dec. 13.—CASE XVI. admitted in collapse, and *fatal* 3½ hours after admission; 1 fever case, and 1 convalescent, remained under treatment. Animals well; body and bed clothes of Case XVI. introduced into room, along with the animals, at night.

Dec. 14.—*No* new cases. Animals well; running loose all day; No. 7 escaped; still feeding on porridge and milk, meat of various kinds, and other kitchen refuse.

Dec. 15.—Case XV. *fatal* in *consecutive fever;* no new cases in hospital. Animals well; bedding in which Case XV. died introduced at night.

Dec. 16.—Case XIV. dismissed recovered; no other cases in hospital. Animals well and lively. Floor covered with fæces; one specimen was perfectly formed, of the colour and consistence of the thicker plastic clay, and appeared as if it had been forcibly extruded through an unyielding sphincter;

a second specimen was also well formed, but of a brownish-yellow colour, soft, and easily broken down; a third was of the consistence of thick porridge, amorphous, of a deep brownish-yellow; it contained a considerable amount of potatoes and other vegetable matters, undigested or only partially so; a fourth was semifluid, amorphous, of a grayish-black colour, and mixed with sand and dust; a fifth was fluid, and of a dark grass-green colour. Their microscopic characters were characteristic of comparatively healthy fæces. Two specimens were agitated with water, and the fluid filtered; both had a greenish-yellow tint, no peculiar odour, and were alkaline and albuminous;—in one the reaction with heat and acid was brownish-red, which speedily changed into brownish-yellow, in the other it was pinkish,—thus showing the decidedly *biliary* nature of these reactions.

Dec. 17.—At this date the experiments were put a stop to by the cessation of the epidemic in Edinburgh. The surviving animals were therefore turned loose, and fed on the kitchen refuse. They continued to enjoy perfect health; some of them even appeared to have gained bulk and weight.

I cannot, in the present paper, make any remarks on the above experiments as bearing on the general question of the *contagion* of cholera; but I would simply beg, in the meanwhile, to call attention to the different effects of the fomites before and after the week between 21st and 28th November. Prior to this period, I endeavoured to concentrate in the animals all the predisposing causes of the disease as we know them in the human subject; subsequently I tried equally to disperse these, by giving the animals nutritious food, exposing them freely to the air, and giving them ample exercise.

CRICHTON ROYAL INSTITUTION, DUMFRIES,
April 1854.

ART. III.—*On the Immediate Cause of Sleep.* By JAMES CAPPIE, M.D., Edinburgh.

In investigating the immediate cause of sleep, it will be our object to determine some of the more important physiological conditions by which it is constituted. With its psychological aspect we have at present nothing to do. We have nothing new to advance either as to the nature of the connection between the mind and the material organization, or the mode in which the functions of the nervous tissues are performed. Regarding the brain as the organ of the mind, and assuming that every psychical operation must involve some change in the nervous tissue, we will simply attempt to determine some points in which the condition and relations of the encephalon differ in the state of complete repose from that of functional activity, and to a certain extent show how these are produced.

In prosecuting this inquiry it will be necessary to take into account the peculiar circumstances in which the brain is placed. To protect an organ of such delicate texture and important functions against physical injuries, it is completely enclosed in a firm, unyielding envelope of bone. Something more than mere protection, however, is secured by such a circumstance. Other peculiarities, especially in the relations of the brain to the circulation, and to the pressure of the atmosphere, are thereby created; and these, we believe, have considerable influence in modifying the action of the nervous substance, and are made subservient to the attainment of most important ends.

As is well known, the encephalic circulation contrasts remarkably with that of any other region of the body in the extreme uniformity of the amount of blood contained at any time in its vessels. We will not here enter into the controversy as to whether or not this amount is at all subject to change. We are far from asserting that it must remain invariably the same; but we yet think it sufficiently obvious that the extent to which it can vary, must, in ordinary circumstances, be extremely limited. As the cranium constitutes a cavity whose capacity cannot be altered, the absolute bulk of its contents must remain constantly the same. Its principal contents being the substance of the brain, the blood, and the cerebro-spinal fluid, no alteration in the amount of any one of these can take place without that of the others being at the same time affected. If the bulk of the nervous tissue be diminished, that of the fluids must be increased, and *vice versa;* or if, while the substance of the brain remains unaffected, the proportion of one of the fluids be increased, the other must be diminished to a corresponding extent. Now, as time must be an important element in effecting any change on the bulk of the nervous substance, or on that of the cerebro-spinal fluid, it may be safely asserted, that in short periods of time, however much its mode of distribution or rate of movement may alter, the absolute quantity of blood circulating within the cranium will not be liable to much variation.

The encephalon has a peculiar relation to the pressure of the atmosphere in being completely removed from its direct, while subjected to an important indirect, influence. The walls of the other cavities of the body are directly affected by that influence in an important manner—the thorax being made by its assistance to expand, and the abdominal parietes to adapt themselves to the minutest alteration that takes place in the bulk of the intestinal contents. The cranium, however, from the unyielding nature of its fabric, is enabled effectually to resist the weight of the air bearing on its surface. But still, the brain is not wholly removed from its influence. There is a communication through a fluid medium between the cavity of the cranium and the atmospheric pressure. The latter, bearing on the general surface of the body, will readily communicate its influence through the soft textures; the principal bloodvessels of the body will, in consequence, be more or less directly affected by it; and through those which enter and make their exit from the cranium, it will be made to bear on the encephalon. If a vacuum were suddenly to be formed in the cranium, its relation to the circulation would be analogous to that of an exhausted cupping-glass over the incisions of the scarificator; and from those vessels communicating with it, blood would immediately be propelled into the cavity.

But the force which, in this manner, would propel the blood into the encephalic vessels must tend to keep it there. The simple influence of gravitation will be insufficient to cause a single drop to leave the cranial cavity. Whatever be the position of the head, it will require a force acting within and from behind to cause the exit of any fluid from the vessels.

It may be said that, for the latter purpose, the force of the heart's contraction will be sufficient; as, for any fluid made to enter the cranial cavity through the arteries, an equal amount will be dislodged from the veins. It is to be remembered, however, that in the nervous as, in all the other tissues, the forces acting at the capillaries are as necessary to the transmission of blood through these vessels, as the influence of the heart's action. While nutrition is going on, a degree of force, varying in intensity according to the activity of the process, is expended in bearing onwards the current of the circulation in the capillaries. The mutual action which there takes place between the blood and the tissue is the condition, and an active cause of the movement of blood. The fluid in the encephalic veins is therefore acted on by two antagonistic forces. The pressure of the atmosphere, on the one hand, by affecting the vessels after they have left the cranium, tends to prevent its exit from the cavity, and to drive it backwards towards the capillaries; and, on the other hand, the nutritive operations in the substance of the brain constitute a constantly operating expulsive force.

Although the encephalic circulation has what may be called an individuality of its own, in being, to a great extent, independent of the amount of blood in the system at large, yet, within itself, variations in the balance of its distribution will be as likely to take place as in any other part of the body. Some interesting particulars result from this circumstance. No change will be able to take place in the amount distributed to one part without affecting the circulation in other portions of the encephalon. If one set of vessels receives more, others must receive less, than their usual proportion of blood. Thus, to use Dr Abercrombie's illustration, supposing that the whole amount of blood circulating through the brain is as ten, and that, in the healthy state of the organ, it is distributed between the arteries and veins as five to five, if the arteries are by some sudden impulse from the general circulation distended by a quantity as six, the veins will be able to contain a quantity only as four.

It must also follow, as a necessary consequence of any alteration in the distribution of blood within the cranium, that "pressure must be produced upon some part of the brain." Although the proper tissue of the organ appears to be incompressible, "the numerous vessels which traverse it will, under such circumstances, be differently acted on; the calibre of some will be diminished, and that of others increased, and the dilatation of the latter will necessarily induce the exercise of an unaccustomed degree of pressure on the nervous tissue with which they are in contact."—(*Bennett*).

What connection the above particulars have with the production of sleep will immediately appear. We would only further premise that the loss of consciousness which characterises that state may be occasioned by whatever exerts a certain amount of pressure on the substance of the brain. For example, it may be induced by the effusion of blood on its surface, by a depressed bone, or by any circumstance which presents such mechanical obstruction to the return of blood from the head, as to produce unusual engorgement of the encephalic veins.

In the sequel it will be our endeavour to show, that during the continuance of healthy sleep, the balance of the encephalic circulation is so affected that the blood accumulates in the venous system of vessels; that, therefore, an amount of pressure, proportioned to the extent to which engorgement of these vessels takes place, is exerted on the surface of the brain; and that this state of the circulation is the necessary result of the mode in which nutrition in the substance of the brain is then modified.

One of the most interesting laws affecting the general circulation, is that by which the amount of blood determined to particular portions of the system is made to depend upon the demand that exists for nutritive material. In a state of health, this demand is regulated to a considerable extent by the activity with which the function of the part is exercised. In the brain we have no exception to this general rule, and it is admitted that in an active state it requires and receives a larger amount of blood than when the exercise of its functions is less vigorous. Within a given period a greater quantity is made to circulate through its vessels in the one state than in the other. When any mental faculty is called into exercise, the brain is the subject of corresponding activity. Its functional powers are then aroused, and nutrition in its tissue must in such circumstances become very much modified. In what this modification consists we do not attempt to determine. It is enough for our purpose if it be admitted there is *excitement* in the process. The ultimate structures undergo changes more rapidly, and disintegration, especially, goes on with much greater energy. This excitement, again, reacts on the circulation; the operations connected with nutrition are at once the final and an immediate cause of the movement of blood in the vessels,—they occasion the demand for nutritive fluid, and they regulate the supply or amount transmitted through the capillaries. When they are modified in certain ways, the demand is rendered greater, and the attractive force is increased in intensity, and the blood is more rapidly transmitted through the vessels.

Not only, however, will the amount of blood transmitted *through* the brain in a given time be increased when it is in active exercise, but if we may judge from analogy and *a priori* reasoning, the quantity of that fluid *in* the vessels of the nervous substance must in the same circumstances be also augmented. Whenever the vital operations are made to take place more vigorously, and the blood is in consequence caused to move more rapidly, the internal pressure against the walls of the vessels will be greater, and this must tend to enlarge their calibre. It is thus when an organ of secretion, for example, is stimulated, the amount of blood in its vessels is increased so long as its functional excitement continues. In analogous conditions of the brain, the same phenomenon must be presented, unless the physical obstacles to its occurrence are too great to be overcome by the physiological action. This circumstance would cause the bulk of the brain to appear greater, and in those instances where its substance has been exposed by injury it has been seen to expand when its function was excited.

At first sight, however, it may appear to be really impossible that any increase of blood can take place in the brain; for how can its expansion take place while the cranium remains entire? But we only require to bear in mind the peculiar manner in which the veins are disposed on the surface of the organ, to perceive how a certain amount of room will be readily gained. These vessels will, in the circumstances we are speaking of, be subjected to an increased amount of pressure. The support they are constantly receiving will be made to bear with greater intensity. As they will in consequence become compressed, they will be unable to contain the same amount of blood, and this circumstance will permit a larger quantity to be contained in the nervous tissue of the brain itself.

If we have succeeded in showing that the amount of blood in the substance of the brain is increased when its functional activity has been excited, it, of course, follows that the reverse phenomenon—a comparatively diminished circulation in the nervous tissue, with an increased proportion of blood in the veins—must be presented during sleep. It must become our object to discover the mode in which this condition is produced. In the brain, nutrition and the circulation act and react on one another in a manner of which we have no similar example in any other part of the body. If congestion of the venous vessels be occasioned by a mechanical cause, the nervous tissue then ceases to perform its functions;

—loss of consciousness results. If we can show how this condition of the circulation can be produced by physiological means, we shall be enabled so far to determine the immediate cause of sleep.

If we could suppose the supply of blood to be at once cut off at the carotids, or its exit from the veins to be completely checked, and nutrition in the substance of the brain to be still kept up for a few moments, it is very evident how the circulation would become deranged. The current through the capillaries would still be kept up for a short time, but not a drop would be expelled from the cranial cavity. The blood would simply be transmitted into the veins; these vessels would, in consequence, become engorged, and cause the brain to be compressed into smaller bulk. Nearly this sequence of phenomena will probably take place when the brain ceases suddenly to perform its functions on account of deficient supply of blood, as in syncope. In this case the amount supplied by the arteries is not sufficient for the demand at the capillaries,—the forces acting at the latter tend to transmit onwards a greater quantity of blood than is furnished by the former. The result must be, that the fluid actually transmitted will at first be partly at the expense of the proportion which should be contained in the arteries,—more will be transmitted into the veins than is expelled from the cranium. The veins will therefore become distended with blood, and, reacting on the substance of the brain, they will check, by the pressure they exert, the operations which are the occasion of the demand.

In the production of natural sleep, however, the first phenomenon in the order of sequence must be a modified form of nutrition in the nervous tissue. Here, again, we do not pretend to determine exactly in what that change consists. It can only be in general terms, therefore, that we can describe the immediate process by which sleep is induced. Our knowledge of the actual changes which take place during the period of wakefulness is too limited to allow the steps by which this condition merges into that which accompanies the state of unconsciousness to be very minutely detailed.

We may say, however, that after a certain amount of functional exertion, the constitution of the nervous substance becomes so affected that, in the absence of extraordinary stimuli, it becomes unfit to subserve the exercise of intellect and volition, or the manifestation of consciousness. After a period, the function of the brain is exerted with greater difficulty the longer it is exercised. The destructive predominate so far over the reparative operations, or in some way the composition of the nervous tissue is so altered, that a period of complete repose is necessary to restore such a constitution that its functional powers may be exercised with former facility. In the transition from that modification of nutrition which accompanies or constitutes the exercise of function, to that condition in which the nutritive process is simply intended to repair and preserve the proper structure of the tissue, the vital operations become gradually less excited and the attraction for nutritive fluid grows less strong. The forces affecting the capillary circulation begin to act with less vigour. The pressure, or support, within the capillaries and arteries will therefore be weaker, the calibre of these vessels will more easily be made to diminish, and the whole brain to collapse if that organ be subjected to any amount of pressure on its surface. It is now to be remembered, that a certain amount of active force is necessary to expel the blood out of the veins. *This force will react on these vessels themselves if there be the least diminution in the amount of support they receive.* Or, what the result must be may be shown in another way. We have supposed it to be the constant tendency of the atmospheric pressure to produce congestion of the veins within the cranium, and the active capillary circulation to be an important means of counteracting its influence in this respect. The former must bear with greater effect as the latter becomes less excited and more unable to resist its influence. The quantity of blood in the substance of the brain will begin to be diminished, while that in the veins will be increased: the pressure from within or behind will be less, and that on the surface of the organ greater than before. As the ultimate structures undergo changes less rapidly, the balance of the circulation will become more altered, and the greater will be the pressure exerted by the distended venous vessels. When, at last, this pressure amounts to a certain degree of intensity, consciousness will be suspended.

These views receive confirmation, we believe, both from the circumstances usually attending prolonged sleeplessness, and from the nature of the influences which induce sleep; but on this part of the subject we will not dwell. In the former case we may have a strong psychical stimulus, as great anxiety or emotion; or the phenomena indicate unusual excitement in the nervous tissue, in consequence of a simple morbid activity, approaching to inflammatory action, as in mania, or from the irritation of a poison, as in delirium tremens.[1]

---

[1] See Dr Peddie "On the Pathology of Delirium Tremens; and its treatment without stimulants or opiates."

In regard to the influences which induce sleep, or tend to occasion death by coma, the most of them seem to favour the accumulation of blood in the veins, either by depressing the operations of nutrition in the substance of the brain, or by presenting physical obstacles to the return of blood from the head. Whatever interferes with the proper oxygenation of the blood must, obviously, act in the former manner. The combination of oxygen with the elements of nervous tissue is an essential condition of the functional activity of the latter. The oxygen acts as a direct stimulant on the brain; and if this organ has been for some time exposed to its action in a strong degree, any circumstance which merely diminishes its intensity, either by preventing the elimination of carbonic acid, or by diluting or rarifying the inspired air, will have a depressing effect on the nervous system. This may explain the influence which every one knows " coming off the fresh air" has in disposing to sleep. The anæsthetic properties of chloroform may perhaps be also explained, simply by that substance being to a great extent substituted for oxygen in the blood. The action of most narcotic poisons is more complicated; but their influence must also be by directly affecting the nutrition of the nervous tissue.

The influence of intense cold probably illustrates the other mode of producing sleep. The physical effect of that agent on the tissues presents such resistance to the exit of blood from the encephalic veins, that the nutritive operations cannot overcome it with sufficient facility to permit the function of the brain being exercised.

ART. IV.—*The Climate of Egypt.* By the Rev. THOMAS BARCLAY, D.D.

It is matter of surprise and regret, that while so many interesting works on the antiquities of Egypt have been published during the last half century, little or no precise information has been furnished respecting its climate. Its pyramids, its temples, and its tombs, have been minutely explored and elaborately described by travellers. The mysterious legends sculptured on its monuments have been deciphered and in part expounded. The names of its ancient Pharaohs, and the customs of its ancient inhabitants, have been brought to light by the learned interpreters of these sculptures, and even the chronology of many of the events which they record, has been ascertained with astronomical proof of its correctness. But while the "ancient manners" and the "modern customs" of Egypt have been minutely and faithfully described, nothing has been written respecting the hygienic peculiarities of that land of wonders; at least nothing sufficiently precise to guide the medical adviser in selecting a suitable residence for his patient, whose case requires a change of climate. The object of this communication is to draw attention to those characteristics of the Egyptian climate, by which an opinion may be formed of its curative influence in cases for which a removal to a milder and warmer atmosphere is usually recommended. The result of my own observation and experience, during five months spent in that country, is a thorough conviction, that there is no accessible part of the world so well adapted for the relief of most of that formidable class of diseases to which the respiratory organs are subject. In venturing to express this opinion, however, I am far from affirming that all Egypt, or any part of Egypt at all seasons, fulfils the conditions required in a climate suitable for such cases. On the contrary, it will be readily perceived that no part of the Delta is at any season adapted to patients who are subject to these affections, nor, it may be added, to those who are either dyspeptic or rheumatic; and from the beginning of May to the end of September, the heat in every part of Egypt is too great for a European constitution weakened by disease. But in Middle and Upper Egypt, from the beginning of October to the end of April, the invalid may breathe, under a bright and cloudless sky, an atmosphere at once of a warm and equable temperature, of perfect purity, and free from all excess of humidity. The climate of other regions may be equally distinguished by one or more of these properties, (though even that is doubtful,) but assuredly there is no other habitable part of the globe in which they are all combined in so great perfection.

The malady for which I sought relief in a southern climate, was chronic bronchitis in its most aggravated form. All the usual remedies, both external and internal, had been resorted to, and steadily persevered in, under the ablest medical advice; but with little temporary and no permanent benefit. I had tried with the same unfavourable result those places on the south coast of England, which are usually recommended to invalids. The symptoms obstinately resisted every remedial measure. The chronic character of the disease was frequently exchanged for attacks of a subacute form. These always commenced with inflammation of the pharynx, creeping insidiously down the glottis and trachea to the bronchial tubes, which became gorged with mucus throughout their whole extent; and on every spot on which the stethoscope could be planted over the lungs, the mucous *râle* was to be heard. Dyspnœa, accompanied with loud wheezing, was at all times distressing; but its nocturnal exacerbations, which invariably occurred after a short sleep, like fits of spasmodic asthma, were often so fearfully violent as to threaten suffocation. The digestive organs were deranged—I had no appetite for food—my frame was emaciated, and my strength prostrated.

After having been two years in this condition, I was induced, by the joint advice of Dr Christison, Dr Douglas Maclagan, and Mr Craig of Ratho, to try the effect of a temporary residence in Egypt. I was so enfeebled as to be unable to encounter the voyage till the month of November; and thus I lost two months of the season suitable for the residence of an invalid in that country. Yet the benefit which, by the blessing of Providence, I reaped from that delicious climate, was most signal; and far exceeded all that my most sanguine hopes had ventured to anticipate.

On the passage outward, I stopped five days at Malta, but found the heat so oppressive in the daytime, and the chills in the evening so severe, that I was glad to make my escape. The extreme humidity of the atmosphere in that island, notwithstanding its high temperature, must always render it, I apprehend, an unfit resort for a bronchitic patient; and the greatness of the diurnal range of the thermometer, at least in winter, makes it questionable how far it is an eligible residence for consumptive patients. It is believed that an inquiry into *results* will not tend to give a favourable idea of its sanative influence on that class of complaints. Of the climate of Alexandria also, I have reason, as I shall show afterwards, to speak unfavourably. In Cairo, however, a very different climate was found; and I had not been many days there, when I began to experience its effects in allaying the irritability of the respiratory mucous membrane. The coldest season there is the latter part of December and the early part of January; and though the temperature even then is equal to that of our best summer weather, yet the evenings are somewhat chill. The following observations, made with the Register Thermometer and Dollond's Hygrometer, show the temperature and the dryness of the atmosphere at Cairo, during the coldest fortnight of the year:—

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
Dec. 25.	59°	64°	5°	7°	...°	Bright sunshine.
„ 26.	58	63	5	7	...	Do.
„ 27.	58	62	4	5	...	Do.
„ 28.	57	60	3	...	5	Rain & blowing.
„ 29.	57	60	3	1	...	Showery.
„ 30.	58	60	2	...	4	Wind and rain.
„ 31.	59	64	5	5	...	Bright sunshine.
Jan. 1.	60	64	4	8	...	Do.
„ 2.	57	62	5	4	...	Cloudy.
„ 3.	58	63	5	0	0[1]	Do. and blowing.
„ 4.	58	64	6	5	...	Bright sunshine.
„ 5.	57	64	7	4	...	Cloudy.
„ 6.	58	61	3	...	4	Drizzling.
„ 7.	57	60	3	...	1	Cloudy & windy.

From these observations it will appear, that warm and equable as the winter temperature is at Cairo, the weather at that season is not free from frequent and sudden changes. It is in Upper Egypt that the invalid must seek entire exemption from these; and there he will not be disappointed.

While I was there, my register was kept on the Nile, and consequently it shows a lower temperature, at least in the night time, than would be denoted at a little distance from the river; while the dryness indicated by the hygrometer in the latter case, was many degrees greater than that registered on the river.

The following table contains the result of the observations noted between Thebes and Asouan, the uppermost town in Egypt, and from these an idea may be formed of the winter climate in that region :—

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
Jan. 17.	64°	70°	6°	7°	...°	Cloudless sky and bright sunshine every day; the firmament blazing with stars every night; no evening chills.
„ 18.	63	68	5	7	...	
„ 19.	66	70	4	5	...	
„ 20.	64	69	5	12	...	
„ 21.	64	71	7	7	...	
„ 22.	69	72	3	6	...	
„ 23.	68	72	4	10	...	

---

[1] The zero of the hygrometer corresponds to summer drought in Britain.

The benefit I derived from breathing the genial air of the Thebaid was very decided. The periodical night attacks, though still occurring, were less violent and of shorter duration; my breathing was greatly relieved; and my strength was so far recruited that I was able, without fatigue, to make daily excursions, sometimes of many miles, to the monuments with which this part of the valley of the Nile is studded.

At Thebes, which for several reasons should be made head quarters in Upper Egypt, the day temperature from the middle of January to the middle of February, ranges from 68° to 78°, and after the latter date, the heat becomes rather too great to be borne with comfort. The invalid should then commence his downward voyage, and by the time he reaches the latitude of Cairo, he will find the climate there nearly as delightful as that which he left at Thebes. My register during the first week of March at Cairo was as follows:—

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
March 1.	66°	72°	6°	9°	° ...	Gent. brze., sunny.
„ 2.	69	74	5	4	...	Do. do.
„ 3.	68	75	7	0	0	Fog.
„ 4.	68	76	8	6	...	Calm and sunny.
„ 5.	71	74	3	...	2	Haze.
„ 6.	69	72	3	6	...	Cloudy and sunny.
„ 7.	65	69	4	6	...	Drops of rain.

In a city like Cairo, with a dense population of more than two hundred thousand people, and with narrow streets walled in by lofty houses, and constantly watered to lay the dust, the air can neither be so pure nor so dry as in the desert. Accordingly, the medical gentlemen of Cairo are in the habit of sending almost all their convalescent patients to reside for some time in the adjoining desert, to enjoy the benefit of its invigorating air. In it there is no water, either running or stagnant, to produce humidity by evaporation, nor is there any decaying matter, either vegetable or animal, to taint the atmosphere with noxious exhalations. It is in the desert, therefore, that the qualities of warmth, equability, dryness, and purity, which are characteristic of the Egyptian climate in general, are to be found in the most perfect union. The soothing influence of the desert air on delicate organs of respiration, and its invigorating influence on a debilitated frame, can be appreciated by those only who, like myself, have experienced its marvellous effects. It is at once balmy and bracing; and the invalid, while breathing it, feels as if he were drinking in health at every pore. I quitted Cairo for the desert of Gheezeh, on the 12th of March, and took up my abode in the neighbourhood of the Pyramids; and there a sudden change came over me, as if by magic. The second night I passed in the desert was marked by sound and uninterrupted sleep, and the absence of the periodical fit of dyspnœa—the first occasion on which I had enjoyed the one, or had been exempted from the other, for more than two years. My appetite soon became *excessive;* both the flesh and the strength I had lost during my illness were restored; every symptom of my complaint disappeared; and at the end of a month, I returned to Cairo in perfect health. The following table contains the meteorological register kept by me while I lived in the desert.

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
	°	°	°	°		
Mar. 13.	63	69	6	10	...	High wind.
„ 14.	63	70	7	12	...	Bright sunshine.
„ 15.	63	68	5	12	...	Do.
„ 16.	63	68	5	13	...	Do.
„ 17.	63	67	4	13	...	Do.
„ 18.	63	70	7	12	...	Do.
„ 19.	63	73	10	14	...	Do.
„ 20.	67	72	5	17	...	Do.
„ 21.	63	70	7	18	...	Do.
„ 22.	63	71	8	18	...	Do.
„ 23.	66	76	10	19	...	Do.
„ 24.	71	76	5	16	...	Cloudy.
„ 25.	67	71	4	10	...	Do.
„ 26.	63	71	8	13	...	Bright sunshine.
„ 27.	63	71	8	12	...	Do.
„ 28.	64	73	9	11	...	Do.
„ 29.	63	74	11	15	...	Do.
„ 30.	63	71	8	12	...	Do.
„ 31.	66	71	5	12	...	Cloudy.
April 1.	67	81	14	23	...	Khamséen.
„ 2.	67	72	5	10	...	Cloudy and blowing.
„ 3.	65	72	7	11	...	Bright sunshine.
„ 4.	68	71	3	12	...	Do.
„ 5.	65	71	6	16	...	Do.
„ 6.	65	75	10	18	...	Do.
„ 7.	64	70	6	3	...	Rain.
„ 8.	65	71	6	10	...	Bright sunshine.

The only other place in Egypt whose meteorology it seems necessary to notice is Alexandria. When it is borne in mind that this city is surrounded on three sides by the Mediterranean Sea, and that on the land side it is enveloped by the Lake Mareotis, a moist atmosphere may be expected; and accordingly, the degree of humidity indicated in the following extract from my register is excessive. In this respect the state of the atmosphere in Alexandria, during the early part of May, will be found to contrast remarkably with that of Cairo and its vicinity during the latter part of April; while the greater equability of temperature in the former city, both from day to day and during the twenty-four hours, is no less observable:—

Place & date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
Cairo.	°	°	°	°	°	
April 19.	68	70	2	6	...	A gentle breath of wind and bright sunshine by day. Always calm towards evening and during the night.
„ 20.	67	70	3	5	...	
„ 21.	66	72	6	5	...	
„ 22.	67	72	5	4	...	
„ 23.	69	74	5	9	...	
„ 24.	70	77	7	9	...	
„ 25.	75	79	4	5	...	
„ 26.	72	75	3	4	...	
Alexandria.						
April 28.	70	73	3	...	10	Sunshine and calm.
„ 29.	70	73	3	...	13	Do. do.
„ 30.	71	73	2	...	12	Do. do.
May 1.	72	74	2	...	15	Do. do.
„ 2.	72	74	2	...	14	Do. do.
„ 3.	73	75	2	...	15	Do. do.
„ 4.	72	74	2	...	14	Do. do.
„ 5.	73	76	3	...	15	Do. do.

How far Alexandria, during the months of March and April, may be a more suitable residence than Madeira or Italy, for those whose complaints require a climate at once warm, equable, *and moist*, I leave it to gentlemen of the medical profession to judge; but I do think myself fully warranted to denounce it as a most unsuitable place for a bronchitic patient. During all the time I was there I felt as if inhaling steam; my breathing was excessively affected, and my whole system was languid and relaxed. These effects, however, by the time I had been twenty-four hours at sea, were completely dispelled, leaving no doubt whatever as to their cause.

I may add, that dyspepsia is very prevalent among the European residents in Alexandria; and I was informed that cases of pulmonary consumption, though not common, do occasionally occur among the natives of the whole northern sea-board of Egypt; but the inhabitants of Middle and Upper Egypt, as far as I could learn, are entirely exempted from that fatal disease. The prevailing maladies throughout all Egypt are dysentery and ophthalmia,—both induced, it is believed, by exposure of the heated frame to currents of cold air.

These observations on the subject of Egyptian climate would be very incomplete if I failed to notice its influence in arresting hæmoptysis. Several instances of its efficacy in that respect were mentioned to me, and one very decided case fell under my own observation. A. B., a middle-aged gentleman, of a clear and florid complexion, had been for years afflicted with this complaint to an alarming extent. He had spent a winter in Italy without experiencing the smallest mitigation of his ailment. He had next been sent to Madeira, and there the malady was very greatly aggravated. He was at Malta when I went to Egypt; but finding no relief there, he came to Cairo in the end of December, and took up his abode at the hotel at which I lodged. The effect of the change of climate was immediate. The spitting of blood ceased at once, nor did it ever recur during his stay at Cairo, which was prolonged till the end of April. He then went to the south of Spain, and remained there till the month of June, when he returned to England, apparently in perfect health, and fully resolved to spend the whole of the following winter in Egypt with the view of confirming his cure.

The transition from the climate of Egypt to that of Britain is too violent to be hazarded by one whose respiratory organs are in a delicate or dubious condition, especially as the season at which it is necessary to quit the one country is far from being genial in the other. I therefore stopped in Spain on my homeward passage, and spent the latter part of May and the greater part of June in that country and in Portugal. Having heard that the air of Malaga was remarkable for its mildness, I repaired thither, and was both delighted and surprised to find in Europe a climate scarcely inferior in any respect to that of the latitude of Cairo. The register I kept while there, and from which I subjoin an extract, shows a temperature which was probably only a few degrees lower than that of Cairo at the same date, while its equability was greater than I had noted anywhere except at Alexandria; and what is still more remarkable, the dryness of its atmosphere exceeds that of Cairo, and contrasts surprisingly with the humidity with which the air of Alexandria is loaded.

Date.	Lowest temperature by night.	Highest temperature by day.	Diurnal range.	Degrees dry.	Degrees damp.	State of weather.
May 20.	70°	72°	2°	9°	...	Bright sunshine.
„ 21.	70	73	3	12	...	Do.
„ 22.	71	73	2	12	...	Do.
„ 23.	69	72	3	11	...	Rain.
„ 24.	67	70	3	10	...	Do.
„ 25.	66	69	3	10	...	Do.
„ 26.	68	70	2	14	...	Bright sunshine.
„ 27.	69	72	3	16	...	Do.
„ 28.	69	73	4	15	...	Do.
„ 29.	69	73	4	15	...	Do.
„ 30.	69	72	3	16	...	Do.

The atmospheric conditions indicated by this table are doubtless to be referred, partly to the geological structure and the physical conformation of the country around Malaga, and partly to the latitude in which it is situated. I am persuaded that, in a therapeutic point of view, the climate in this part of Andalusia is deserving of more attention than it seems hitherto to have received. Those who take it for granted that the climate of Italy must be the mildest and warmest in Europe, seem to forget that Malaga is 248 miles farther south than Naples, 318 miles farther south than Rome, and 518 miles farther south than Venice; and those who have not adverted to the fact, will probably be surprised to find that it is 5 miles farther south than Algiers. Both Gibraltar and Cadiz are somewhat south of Malaga, but both have a great diurnal range of temperature, and are nearly equal to Alexandria in the humidity of their atmosphere; the one standing on an almost insulated rock in the Mediterranean Sea, and the other on a narrow spur of land projecting into the Atlantic Ocean.

In the beginning of June I moved northward to Lisbon; but on its climate, and that of delightful Cintra, no information is needed. In the end of June, 1853, I returned to

Scotland; and though I was threatened soon after I reached home with a recurrence of bronchitic symptoms, these *now* yielded readily to medical treatment. I am most thankful to be able to add, that during the last twelvemonth I have enjoyed excellent health, nor have I been affected, during that time, by any of the changes of weather to which our variable climate is subject.

CURRIE MANSE,
September 1, 1854.

ART. V.—*Suggestions in regard to the Performance of Post-mortem Examinations.* By W. T. GAIRDNER, M.D., one of the Ordinary Physicians of the Royal Infirmary of Edinburgh, and lately Superintendent of Morbid Anatomy in that Institution.

(Continued from p. 321.)

In the following remarks upon the mode of performing the details of a *post-mortem* examination, my object is chiefly to furnish practitioners with a certain number of directions, which may serve to prevent the omission of important particulars without being too complex or minute for general use. It is to be observed that medico-legal examinations require attention to certain special circumstances, which, as they are usually very fully treated in the numerous works on legal medicine,[1] I shall not think it necessary in general to notice.

In private houses, the mere presence of a corpse, and, still more, the performance of a *post-mortem* examination, is always the source of a certain degree of discomfort and inconvenience, which, however, may be greatly diminished by a few precautions on the part of the medical man. The decomposition of the body is often very much hastened by the incautious burning of fires in winter, or gas in summer, in rooms adjoining that in which it lies, or by the unnecessary heating of the passages, and thus of the whole house. It is worth while to prevent this, by instructions either to the relatives or to the servants, immediately after the death; which instructions may be made to include all the necessary arrangements for free ventilation and fumigation of the apartment in which the body lies. In the case of eminent persons, the medical attendant may sometimes have it in his power to render an important service to the public, by suggesting the employment of an artist to preserve a plaster mask of the face as a guide to the sculptor and painter. It occurred to me, on one occasion, to fall under the censure of a distinguished phrenological writer for neglecting to secure a cast of the entire head of a man well known to the world; and, as I believe the students of this so-called science are not without a suspicion of a conspiracy on the part of the medical profession to deprive them of the means of pursuing their investigations, I think it right to warn my professional brethren of the reproach which they may unwittingly incur.

---

[1] In a little work published by the London Medical Society of Observation, entitled "What to observe at the Bedside and after Death in Medical Cases," the reader will find very minute directions of the kind here alluded to. See also Beck's Medical Jurisprudence; Orfila, Médecine Légale; Devergie, Médecine Légale; Bock, Pathologische Anatomie; Engel, Pathologisch-Anatomische Propädeutik.

The arrangements for a dissection in a private house should include the following precautions:—The time should be, if possible, during clear daylight, but after the visits of the day. The table on which the body is to be placed should be of good size, steady, and of a convenient height; but it is often a good plan to dispense with a table altogether, and to procure from an upholsterer a plain deal board of sufficient length, with two proper supporters. Abundance of water, cold and warm; several basins, and one or two larger receptacles, such as the stoneware foot-baths now in general use; one or two sponges; towels; a little chloride of lime and sulphuric acid, should be provided, if possible, at the house in which the dissection is to take place.

Preliminary Observations.—The body, having been removed from the bed, is to be properly placed and undressed. The operator should then appoint some one to take notes for him, and this person should commence his duties by inscribing at the top of a sheet, and in separate lines, the *date of dissection*, the *number of hours since* death, the *state of the atmosphere*, and the presence or absence of *external signs of decomposition*, viz., the putrefactive odour, the greenish discoloration of the abdomen, and the sugillation or soaking of blood into the textures from the great veins in the neck and thighs. The *post-mortem* lividity of dependent parts must be carefully distinguished from this last phenomenon; but the amount of lividity, and the parts chiefly affected, may also be here noted, as also the degree of *rigor mortis*. The *name, sex,* and *age* (apparent and real) of the individual should then be noted, and it is often convenient at this point to obtain some particulars of the fatal illness and general history, although the recording of these facts may sometimes be postponed with advantage.

External Measurements.—These must be made with greater or less minuteness according to circumstances. The *stature*, as furnishing an important element of comparison with the weight and size of organs, should rarely be omitted. To this I commonly add the circumference of the head and chest, and the breadth of the body at the *humeri* and *trochanters*, together with the length of the trunk as measured from the jugular fossa to the *os pubis*. When the identification of the body is concerned, these measurements should be very exact; and others of equal importance should be added, particularly of the hands, feet, and diameters of the head. It would even be desirable, in important cases of this kind, to procure casts of those parts of the body most likely to be recognised or identified. To those who have time for the study, much instruction is to be derived from the comparison of the different thoracic diameters in cases of disease, as compared with those in health. Such measurements may easily be made with the large callipers, described in a former paper.

Weights.—The trouble involved in weighing a dead body, is the only objection to the constant employment of this particular as a standard of comparison. When it can be procured, it is a datum of great value. As there is a manifest advantage in keeping all the statements of weight of the different organs in one place, where they may be compared without the necessity of reading over the general report, I have been accustomed either to leave room for them at this point, or to place them in the margin of the report, as well as in the detailed description of the organs.

External Appearances.—The degree of development of the body: the relative thickness of the bones, muscles, fat, and integuments, should be stated in general terms; the conformation of the head, thorax, abdomen, and limbs, should also be adverted to. In cases of doubtful personal identity, scarcely any remark is too minute; the position of a mole or nævus; the exact form and peculiarities of the features; the number and position of the teeth; the state of the nails; the colour, distribution, and character of the hair; the condition of the genital organs and mammæ, should receive minute attention.

The external morbid peculiarities should next be studied. The evidences of anasarca should be looked for in the lower limbs, face, and back; of emphysema, in the neck and thoracic parietes; of eruptions, ecchymoses, or wounds on the general surface, and at the orifices of all the mucous canals.[1] Syphilis will leave its traces on the genitals, in the groins, on the tibiæ, &c.; scrofula in the cervical glands, in the eyes, in the upper lip; rickets in the limbs, pelvis, back, and thorax; chronic hydrocephalus in the enlarged head and expanded fontanelle; rheumatism and gout in the distorted small joints; phthisis in the chest and general organism; cholera or dysentery in the contracted and blue finger ends, in the retreating eye-balls, and occasionally blood-shot conjunctivæ; age and decay in the state of the hair and teeth, and in the *arcus senilis* of the cornea. From the study of these particulars, much may be learned in the dead as in the living, and they derive additional interest from their comparison with the state of the internal organs.

In cases where contusions or other marks of external injury are found, it is proper to investigate thoroughly, by dissection, all the parts of the body where these have been discovered, before proceeding to the examination of the cavities. For the facts to be noted in regard to wounds and contusions, the works above mentioned on medical jurisprudence may be consulted.

In now proceeding to lay open the cavities of the body, the operator must be guided in his order of procedure to a certain extent by the facts known in the individual case. It is commonly most convenient to open the thorax and abdomen together, extending the incisions, if necessary, for the neck and pelvis, and leaving the head to be examined afterwards. Where the spine is to be opened, however, it will be found preferable to proceed first with the head and spine, and afterwards with the thoracic and abdominal cavities.

To open the thorax and abdomen, make an incision from the jugular fossa to the pubic bone, in the mesial line; note the thickness of the integuments, the amount and consistence of the fat, the colour of the muscles. Dissect back these to about an inch beyond the costal cartilages; cut the latter close to the ribs, and, in doing so, note the degree of ossification, if any (take care in this step not to wound the internal organs, and, for this purpose, place the fore-finger firmly on the back of the knife). The first costal cartilage is to be cut obliquely outwards, proceeding from below; the sterno-clavicular articulation obliquely outwards, proceeding from above. Next raise the sternum very carefully, and separate it from the subjacent parts; if there be any aneurism or tumour adherent to the bone, examine well its relations, and leave a greater or less portion of the sternum to be removed along with the thoracic viscera, and sometimes also the larynx and trachea.

---

[1] The insidious form of murder, perpetrated in numerous instances of late years, by wounding the interior of the vagina, should never be forgotten. I have likewise known death follow wounds of the rectum and of the back of the pharynx, which would be very apt to escape a superficial or a too hasty observer.

ANTERIOR MEDIASTINUM AND THYMUS GLAND.—In removing the sternum, the condition of the anterior mediastinum and root of the neck necessarily comes under review; note, therefore, at this point, the existence of any morbid structures, and when any remains of the *thymus gland* exist, let them be carefully examined. Such remains are not unfrequently to be discovered even in adults. I observed them repeatedly during the cholera epidemic of 1848–49, in young persons cut off in the midst of high health. I have elsewhere (Monthly Journal, vol. ix., p. 1086) noticed a case of anæmic and hemorrhagic disease, with enlargement of the spleen and thymus gland, where the latter organ, in a man twenty years of age, was "so much enlarged as to fill the entire mediastinum, resting on the diaphragm below, and passing into the neck above. The gland was eleven inches long, and, on an average, three or four inches broad. The microscope showed that the essential glandular elements were present in large quantity." This case is, so far as I know, a unique one;[1] but minor degrees of the abnormal persistance of the thymus gland are not, I believe, exceedingly rare; they are very apt, however, to escape attention, and to pass for mere thickenings of the cellular tissue.

The next care of the operator should be to make a *general survey of the thoracic and abdominal viscera,* as they are now exposed to view. Let him first direct attention to their relative position. In perfectly normal subjects the lungs fall back so much that only their edges are seen; they may, however, be readily inflated from the trachea, so as to meet in front under the sternum. The anterior inferior border of the upper lobe in either lung should be a slightly curved line crossing obliquely from the lower part of the axilla to a point on a level with the lower part of the pericardium. In cases of tubercular disease, when the upper lobe is contracted in volume and adherent, the position of this oblique line may be so much altered, that its termination may be opposite the third, second, or even the first rib in front. On the other hand, in certain cases of collapse of the back parts of the lung with emphysema, the upper lobe may appear to occupy the whole anterior wall of the chest. When an entire lung, or even an entirê upper lobe, is permanently contracted, the heart is often carried from its natural position towards the diseased side, and the opposite lung, with its pleura, will be found to have passed in front beyond the middle line of the sternum. The usual relations of the heart are so well described in many elementary works, that I may pass them over here. The liver, in the epigastric region, is generally found beyond the margins of the ribs for a space of one or two inches; elsewhere, it is usually covered, but there are many exceptions to this rule, from alterations in the position of the organ consistent with health. The uppermost point of the liver, and, consequently, of the diaphragmatic concave on the right side, is usually on a level with the nipple; on the left side, the diaphragm does not ascend quite so high. The gall-bladder scarcely projects beyond the margin of the liver, unless it be unduly distended, or unless the liver be much diminished in size. The spleen is entirely concealed beneath the hypochondrium. The stomach shows a triangular portion, amounting to one-third or two-thirds (according to its state of distension) of the anterior wall. The rest of the abdomen presents to view the great omentum covering the transverse colon and small intestines; the cæcum, more or less distended, projects from the right iliac fossa. The bladder is usually just visible above the pubis. The uterus is invisible, unless during pregnancy.

---

[1] In the Museum of Guy's Hospital, No. 1559^{60}, is the nearest approach to it which I have seen. The gland is about four inches long. The preparation is from a girl aged eleven years, who had repeated hemorrhages and purpura, with enlarged spleen and disease of kidneys.

Before concluding the general survey, the character and amount of any fluid which may exist in the serous cavities should be noted, as the handling of parts during the dissection very soon renders it turbid. The details of the examination should next be proceeded with.

PERICARDIUM.—The pericardium is occasionally entirely devoid of fluid, and so dry that its anterior surface is semitransparent and parchment-like. More commonly it contains several drachms of fluid; as much as two or three ounces may exist without giving rise to any appearance of distension, and without being connected with any distinct morbid process. The serous layer of the pericardium should be everywhere quite transparent, and of almost impalpable thickness. Adhesions (morbid but insignificant) are very often met with at the base between the great vessels and the auricles; thickenings without adhesion (white patches) are likewise of great frequency and of little clinical importance; their seats being the parts above mentioned, and also the whole surface of the auricles, the course of both coronary arteries, and the anterior surface of the right ventricle.

The existence of pure serum, or of serum with small transparent flocculi, in the pericardium (when in considerable amount), is usually a presumption of general dropsy. When pus or lymph in large quantity is found, pericarditis is commonly present. The tinging of these elements with blood-colour is not infrequent as a *post-mortem* phenomenon; the blood having transuded from the interior of the heart and great vessels; in these cases (as Dr Jenner has correctly remarked) the microscope shows that the blood-coloured fluid is devoid of blood-corpuscles, thus distinguishing it from a hemorrhagic extravasation. When blood in substance exists in the pericardium, it is necessary to look with care for its source, in the manner to be immediately mentioned. The remarks in this paragraph apply to all the serous membranes of the thorax and abdomen, as well as to the pericardium.

Blood in Pericardium, &c.—When pericardial or pleural hemorrhage gives proof that a bloodvessel has been ruptured, the utmost care is sometimes necessary in order to discover the site of the rupture. If a cursory inspection fails, it is necessary to test successively the aorta, and the cavities of the heart, by injecting them with water. The aorta (the most probable source) may be advantageously filled from the carotid artery. If this fails to show the rupture, and the aorta appear normal, cut across its ascending portion immediately above the pericardium, and pass a tube connected with a syringe carefully past the sigmoid valves into the left ventricle of the heart; distend the left side of the heart with water. If there is still no appearance of an opening, distend the right auricle and ventricle from the jugular or brachiocephalic vein. If the source of the blood is still undiscovered, it is probably some very minute branch of the coronary artery, which has been partially closed by fibrine. The best way to discover the exact vessel which is at fault is as follows:—remove the heart with the pericardium entire; tie a tube into the cut aorta; ligature all the other vessels leading to or from the heart; place the organ in water, and inject from the aorta with *air*, using if necessary a little more force than formerly; the ruptured vessel will probably give way, and bubbles of air will reveal its position.

Air in Pericardium, &c.—When air exists in considerable quantity in the pericardium or pleura (not being the product of decomposition or the result of an external wound), it probably proceeds either from the lung or the intestine, and a fistulous communication must be sought for. If this be not at once apparent, the following is the best method to detect it. Before the organs have been disturbed, open the trachea and insert a tube or blowpipe. Then fill carefully the pericardium or pleura (as the case may be) with water, and blow air into the lungs, watching at the same time for the rising of bubbles in the fluid. If the existence of a fistula is thus made clear, remove the fluid gradually, repeating the inflation from time to time, until the exact site of the abnormal opening is disclosed.

If the intestine be the source of the air, it will probably have more or less of a fæcal odour, and adhesions will be found among the organs below the diaphragm, of sufficiently definite character to reveal the parts affected. The whole of the portions thus brought into suspicion are then to be detached with the diaphragm and thoracic organs, and water or air can then be injected from the intestine in such a manner as to test the existence of a fistula, and to discover its exact site.

HEART.—If the great vessels issuing from the heart be free from enlargement, the examination of that organ may conveniently be made at this stage ; but if there is the slightest appearance of aneurism in the aorta or arterial trunks, it is desirable to preserve the relations of the parts until a careful dissection has been made. If this cannot be done without disturbing the viscera, the entire contents of the neck and thorax should be removed *en masse*, and laid aside after being roughly examined. In ordinary cases, however, I commonly remove the heart by carefully dividing the great vessels within the pericardium. In doing this the character of the blood and particularly the amount of fluid and coagulum should be observed.

For full information on the weight, measurements, and form of the heart, I must refer the reader to the very numerous special monographs on this subject, and especially to the works of Bouillaud, Bizot, John Reid, and Peacock. In the work of Hasse (translated by the Sydenham Society), and the recent treatises of Drs Walshe and Stokes, as well as in the Descriptive Anatomy of Quain and Sharpey, most of the requisite information will be found in a condensed form. The normal weight of the adult heart is usually between 8 and 12 oz., in healthy subjects, but varies extremely in disease. Its form is conical or pyramidal, the apex being in the state of health formed by the left ventricle. In dilatation the organ becomes to a marked extent more gibbous, approaching a spherical form, and frequently having for its apex the ricle. Much importance has been attached to the of the thickness of the walls, and the size of the c cavities, and very complex classifications of heart-ses have resulted from the study of these relations. But most all these classifications are, in my opinion, vitiated by over-refinement, if not by positive error. The size of the cavities is, in normal as well as hypertrophied hearts, so much subject to physiological variations, that to use observations on this point as fixed pathological data is not conducive to accuracy. The thickness of the walls is equally subject to variation, being in the full or distended heart not much more than half what it is in the contracted organ. In no part of the description of a dissection is it more necessary to avoid the use of questionable phraseology than in this. Let all exact observations and measurements, therefore, be accompanied by a clear description of the organ and its cavities, conveying to the well-informed and skilful reader a distinct idea as to the degree of contraction or relaxation of the organ. For this purpose, examine and note the following particulars, in order:

(*a.*) *The general form and size when in situ.*—In particular, the form of the apex; the position of the ventricles with respect to one another (does the right project, and the left retire, more than natural?); the relative size of the auricles; the relative size of the aorta and pulmonary artery.

(*b.*) *The degree of resistance of the ventricles when compressed by the fingers.*—The left ventricle should present somewhat of the feeling of a solid mass; in most chronic diseases, and in all cases of death by hemorrhage or emaciation, this may be considered as its normal condition. In acute diseases, especially fevers; in plethoric subjects with weak circulation; and in many forms of disease of the lungs and heart, the left ventricle loses this resistent property, and is found after death more or less flaccid and bag-like.

(*c.*) *The amount of blood in the heart, and in its right and left auricular and ventricular cavities respectively.*—The heart which has both its auricles and the veins leading to them distended with blood is not necessarily diseased; but if, in addition to this, the ventricles are distended, the apex more or less rounded, and the bases of the *columnæ carneæ* widely separated, the organ is abnormal, inasmuch as the natural heart always contracts on its contents, and expels them in considerable measure from the ventricles after death. In particular, the distension of the left ventricle with blood in the dead body, is pathognomonic of a morbidly weakened circulation, and always coincides with disorder, either of the mechanism, or of the fibre of the heart. It is never, as is often erroneously assumed, a result merely of death by syncope;

nor is the emptiness of the left ventricle some time after death in any degree a proof of death by asphyxia.[1]

(*d.*) *The general form and apparent capacity of the ventricles.* After the heart has been opened (for the method of opening, see hereafter), *the position of the* septum ventriculorum (does it arch to the right or left?); the *position of the columnæ carneæ*, especially in the left ventricle (nearly approximated or widely apart?).

(*e.*) *The average and greatest thickness of the ventricular walls* (not including *columnæ carneæ*), and the *thickness of the* septum ventriculorum.

(*f.*) *The condition of the endocardium generally, and in each cavity specially. The condition of the lining membrane of the commencement of the aorta and pulmonary artery.*—Note the existence of thickening, blood-staining, opacity, atheroma (examining if necessary with the microscope), adherent lymph, polypi, &c.

(*g.*) *The competency of the valves, and condition of the valvular orifices.* For the method of making this examination see below.

(*h.*) *The state of the Foramen Ovale.*—For the most part this is absolutely closed in the adult; but the persistence of a small oblique opening, of size sufficient to admit a crow-quill or even a goose-quill, is not to be accounted morbid, inasmuch as this is frequently met with where there has been no trace of disease.

(*i.*) *The state of the muscular fibres of the heart in either ventricle*, as to softness, flaccidity, friability; and, finally, as to any morbid deposit ascertainable by the naked eye or the microscope.

(*k.*) *The condition of the* coronary arteries, as to atheroma, calcification, or obstruction.

(*l.*) *The weight of the entire heart,* devoid of coagula and fluid blood.

Method of proceeding in making the above examination.—After noting the particulars *a*, *b*, and *c*, the heart is to be carefully emptied of clots, and the auricles are first to be laid freely open, by perpendicular incisions from before backwards. The *foramen ovale* may then be examined. By now pouring

---

[1] It would be out of place here to enter into a physiological dissertation; but I am bound to state, as a result of experience, that the opinions given in courts of justice as to the cause of death, founded on the state of fulness of the right and left cavities of the heart, appear to me to rest, in most instances, upon a very unstable foundation. Syncope, no doubt, frequently concurs with a full left ventricle, and asphyxia with an empty one; but the reverse is also not uncommon; and the dogmas of authors upon this subject are certainly among the most assailable of those with which legal acumen delights to perplex a jury.

water into the ventricles from the auricles, the auriculo-ventricular valves will be floated up towards these openings, when their flexibility, freedom from disease, and capability to close the openings, may be roughly tested by the experienced eye. There is, however, no absolutely correct method of ascertaining the competency of the auriculo-ventricular valves. When the left ventricle is in a state of extreme contraction, the closure of the mitral valve is usually nearly complete, and it may even be made to resist the pressure of water injected into the ventricle; but this perfect action of the valve disappears so soon as the tonic contraction of the fibres is overcome. The tricuspid is never found to act so completely (in the dead body) as the mitral; not (as some have supposed) because the apparatus is normally imperfect,[1] but because the contraction of the right ventricle after death being incomplete, the valve is not in the position for accurate closure.[2] After this examination of the orifices from above, the ventricles are to be laid open by incisions parallel, and as close as possible to, the septum; these incisions, beginning at the apex, are to pass upwards towards the *conus arteriosus* of either ventricle, but with care not to injure the semilunar valves; the competency of these is then to be tested by water poured into the two arteries from above. Care must be taken that no clots are present, and that the valvular orifices are not deformed by the position in which the heart is held. I have repeatedly seen both of these circumstances deceive inexperienced observers. The opening of the heart is then to be completed by continuing the anterior ventricular incisions through the pulmonary artery and aorta; this should be done very carefully, so as to cut *between*, and not *through*, the valvular segments. The auriculo-ventricular and semilunar valves, and the whole endocardium, should then be carefully examined, and the other necessary observations made. In noting the size of the orifices, the graduated balls (formerly described) are to be employed; in measuring the thickness of the ventricles, the small callipers will be found to give the most accurate results. In the absence of these instruments, the auriculo-ventricular orifices may be roughly gauged by means of the fingers; and the thickness of the ventricles may be accurately estimated by new incisions transversely to their axis; but these must be made exactly perpendicular to the ventricular wall. Finally, the coronary arteries are to be divided with the scissors, and the state of the muscular fibre of the heart is to be carefully scrutinized; sectious being made for this purpose in the right and left ventricles, the septum, and the columnæ carneæ.

---

[1] Adams—Dublin Hospital Reports, vol. iv.; Wilkinson King, on the Safety-valve function of the Tricuspid valve, Guy's Hospital Reports, 1st Series, vols. iv. and xii. John Hunter held a similar opinion.

[2] See a short abstract of my views on this point, in the Proceedings of the Med.-Chir. Society of Edinburgh, Association Medical Journal, May 19, 1854, and Monthly Journal, for July 1854.

The description of the Heart is now finished. There remains that of the

BLOOD, as contained in the heart and great vessels. The following points require attention :—The relative amount of coagulum and of fluid blood, the degree of fluidity of the latter, the firmness of the former, the extent to which they are decolorized, the firmness of their adhesion to the ventricle, the degree to which the form of the vessels and valves is impressed upon the coagula, the degree to which staining of the endocardium and endangium has taken place, and the existence or non-existence of marks of decomposition in the blood and blood-corpuscles. Finally, a microscopic examination should be made with care—the amount of red and white corpuscles,[1] the character of both, and the presence or absence of foreign matters, should be, as far as possible, ascertained.

PLEURÆ.—The membrane of the pleura is, in the normal state, so delicate and transparent, that the separate air-vesicles of the lung can be readily traced through it, as well as the lobules, pigmentary matter, &c. The pleuræ may contain no appreciable amount of fluid; but three, four, or even six ounces of clear or blood-stained serum in either or both can scarcely be considered a proof of positive disease. Adhesions are exceedingly frequent; indeed in persons dying from disease their absence is quite exceptional. Their most frequent sites are the apex, upper and back part of the lung, as well as between the lobes; they are exceedingly frequent in connection with tubercles, notwithstanding which the existence of tubercles in the pleura itself is rather uncommon. As cavities in the lung are very frequently sealed up by the formation of adhesions, or with the intervention of only a thin layer of lung, it is necessary to remove an adherent lung in such cases with considerable circumspection if its state is to be correctly understood. Adhesions in the pleura may have every degree of toughness, from that of soft and recent lymph to that of firm cellular tissue, and from the consistence of the latter to that of fibro-cartilage. Sometimes too, but rather rarely, they become the seat of extensive calcification. In all these cases it is requisite, in removing the lung, to separate the costal pleura from the ribs rather than run the risk of injuring the organ.

---

[1] It is important to observe, with relation to this point, that only a very rough estimate of the amount of corpuscles can be formed in the dead body, whether by chemical or microscopical examination. This arises from the fact that during coagulation the white corpuscles become in a great measure incorporated with the clot, and inseparable from it.

The method of pursuing the examination in cases where blood or air is found within the cavity of the pleura is described above in connection with the *pericardium.*

LUNGS.—The method of removing the lungs for examination when they are non-adherent, is so simple as to need no description. When firm adhesions exist, it is often extremely difficult to avoid lacerating the lungs. The best method has been mentioned above.

Weight of Lungs, &c.—On removing the lungs they should be immediately and separately weighed before being cut into or handled. The weight of the lung depends, to a much greater extent than that of any other organ, upon the necessarily fluctuating amount of the fluids which it contains. It is, accordingly, impossible to assign any very definite standard for the weight of the healthy adult lung. The observations of Dr John Reid give an average of 45 oz. for the male, and 32 oz. for the female, as the weight of the two lungs together, the right predominating over the left to the extent of two or three ounces. Other observations give results slightly different, but I am inclined to think that no average hitherto presented gives a correct idea of the weight of the strictly normal lung. In fact, all collective observations made in hospitals on this point must necessarily be vitiated by the almost invariable presence of morbid accumulations in the lungs of those who die slowly. These errors, too, are all on one side; and, accordingly, I have repeatedly had occasion to observe that the weight of the strictly normal adult lung has been very far below the above averages. I have found the two lungs together, when devoid of all morbid effusions, to weigh in many cases less than 20 oz.; in one case (an adult robust female, who perished by acute disease of the brain, and in whom the lungs were free from adhesions, pale, and with the vesicles of perfectly uniform size, the bronchi pale, and containing no fluid), the right lung weighed 7½, the left only 6½ oz. On the other hand, I have found a single lung under the influence of pneumonic disease to weigh between 60 and 70 oz.

Form and Size, &c. of Lungs.—The healthy lung has the shape of a cone rounded at the top, the base of the cone being oblique, and one side scooped out so as to receive the heart and great vessels. The edges, anterior and inferior, are sharp and well defined, presenting in the normal state no abrupt hollows and convexities such as are often observed in disease. The air-filled vesicles of the lung are perceptible to the very edge with the naked eye, or at least with a simple lens of a few diameters' linear power. These vesicles should be nearly uniform in size throughout the anterior part of the lung; as a general rule they are smaller at the root of the lung than at the edges, though this difference can scarcely be considered strictly normal. The same may be said of Magendie's observation as to the enlargement of the air-vesicles of the lung as life advances.[1] It is frequently correct as an observation in individual cases, but cannot be considered as indicating a normal lung. Any abrupt subsidence of the tissue of the lung, any corrugation or depression of the pleura, indicates disease. A rounded state of the sharp edge, accompanied by sudden subsidence and increased sharpness at certain points, with air-vesicles alternately enlarged and invisible, indicates emphysematous disease, with primary atrophic degeneration of the deeper air-vesicles towards the centre and root of the lung.[2] Deep furrows, with induration and calcareous matter at the apex of the lung, commonly indicate chronic tubercular disease. Diminished size of the lower lobe as compared with the upper usually proceeds from bronchitis; the opposite condition is most commonly the result of tubercle. It is rarely that the middle lobe of the right lung is materially altered in volume. I have, however, seen it diminished to a remarkable degree. Individual lobules or groups of lobules of the lung not unfrequently become affected either with increased or diminished size of their vesicles; they are then seen to stand out or sink in from the general surface, and to be distinctly marked out by the interlobular septa.

Absolute increase in size of an entire lung only occurs when the opposite organ has been contracted in volume by disease; the healthy lung then after a time becomes truly hypertrophied, and its edge crosses in front beyond the middle line. *Apparent* increase of size is not uncommon; hepatization of the lung, on the one hand, or extensive emphysema, on the other, may so destroy the natural resiliency of the organ that it may appear to fill up the chest more than usual, and on removal may contrast strongly with the appearance of a sound lung. But it is very certain that in such cases the diseased organ never appreciably exceeds the size of a sound lung when fully inflated. There is therefore no real hypertrophy.

---

[1] Journal de Physiologie, tom. i.

[2] See the author's "Pathological Anatomy of Bronchitis," or the British and Foreign Med.-Chir. Review for April 1853, p. 452.

Absolute diminution in size of an entire lung is not uncommon from pleurisy, and also from very chronic and extensive tubercular disease.

Inflation as a test of pulmonary disease.—Where a portion of lung is simply impervious to air from closing of the vesicles, without any disorganizing disease or effusion into the tissue, it is found to be susceptible of inflation from the bronchi; and under these circumstances, resumes more or less slowly its normal characters. Where inflation does not take effect, more complex disease must be looked for.

Specific gravity of the lung.—It is rarely, and only as a result of very extreme disease, that the entire lung which has once breathed is so heavy as to sink in water. The knowledge of this fact led to the celebrated medico-legal "hydrostatic" test of living birth; a test of much practical value, although not susceptible of the general application originally claimed for it. Whether in the child or the adult, it may be said that the lung which sinks in water is almost absolutely devoid of air; for a small quantity of air in the vesicles is sufficient to reduce the specific gravity of the organ to that of water. The same rule may be applied to small portions of the lung. But although the sinking of a lung, or of a separated portion, in water is an unquestionable proof of disease (except, indeed, in the unborn or still-born fœtus), this circumstance throws absolutely no light upon the kind of disease which exists, inasmuch as almost all the organic affections of the lung consolidate its tissue. Still less does the floating of a portion of lung necessarily presume its sound condition; inasmuch as a lung may be extremely diseased, and yet be scarcely increased, nay, decidedly diminished in specific gravity. The information, therefore, communicated by the hydrostatic test, although valuable, is of the most vague and general character.

Crepitation when pressed between the fingers, or when cut by a knife, is another of the characters of the air-filled lung to which much importance has been attached. It is to be observed, however, that the lung tissue, when perfectly devoid of fluid accumulation in the air-cells, crepitates very little, or not at all; on the other hand, no crepitation is elicited from air-vesicles which are perfectly devoid of air. The *perfectly* normal lung, therefore, like the emphysematous lung, scarcely crepitates; and the hepatized or collapsed lung is also non-crepitating; indeed, crepitation is most distinct in lungs which are the seat of œdematous accumulations, and in which the air-vesicles contain a mixture of fluid and air. With these limitations, crepitation will be found to have nearly the same application as the hydrostatic test.

Consistence and resistance of the lung to the touch. These qualities must be estimated with relation to the normal organ, and only by a person familiar with the latter. Thus the lung may be more turgid and dense (resistant) than natural, as in hepatization, or more collapsed and flaccid, as in carnification from pleuritic effusion; and in connection with either of these conditions it may be unduly indurated and tough, or abnormally soft and friable. I have repeatedly seen much importance attached in the description of disease to the dimpling of the external surface on pressure with the finger; but this in reality only proves that the organ is not preternaturally resistant, and that it is more or less filled with air or fluid.

Observations to be made on section of the lung.—The colour; the amount of black-pigment; the amount of fluid to be squeezed or scraped from the section; the characters of that fluid on the blade of the knife, on glass, under the microscope; the apparent size of the air-vesicles; their contents; do granulations of fibrine exist, giving the "granular aspect" described by Laennec in the hepatized lung? Notice whether the granulations are large or small as compared with their usual size in hepatization; observe also whether they break down on being removed from the tissue, or continue to be visible on the blade of the knife. Finally, notice the existence of any other morbid deposit or growth in the pulmonary tissue; the existence and position of abscess, gangrene, &c.; the position, relations, and character of cavities and their contents, or of encysted matter.

BRONCHI AND BLOODVESSELS OF LUNG.—These are to be traced as far as necessary by means of fine scissors, with the help of probes and directors. In the bronchi, the quantity and characters of contained fluid are to be noted: the existence of cylindrical coagula or other obstructive bodies, the existence of contraction or dilatation; the character of the pulmonary tissue in the neighbourhood of such portions; the mode of communication of the bronchi with pulmonary excavations; the connection of their lining membrane with that of the latter; the condition of the bronchial lining membrane as to thickness, vascularity, &c.

In the pulmonary artery and its branches, the existence of atheroma, and of any contraction, obliteration, or obstruction, is to be noted.

In the pulmonary veins, and their branches, the existence of thickenings, fibrinous or purulent plugs, or other obstructive matters.

The capillaries can be examined only after injection.

BRONCHIAL GLANDS AND LYMPHATIC SYSTEM OF LUNGS.—The lymphatic vessels of the lungs may be traced in general by the distribution of the black pulmonary pigment: they collect into larger trunks in the surface of the lung, keeping for the most part in the interlobular spaces; they enter the bronchial glands and carry with them thither the pigment with which they are loaded. Their special pathological conditions are little known. The bronchial glands are subject to enlargement, and with this to many pathological alterations; sometimes they become the seat of morbid deposits, which are ultimately dried up into calcareous concretions, or discharged into the bronchial tubes, through fistulous openings between their cartilages. Occasionally the bronchial glands may be so much enlarged as to compress the bronchi or the lung, and thus to lead to secondary lesions of those parts.

(*To be continued.*)

ART. VI.—*Summary of the Analogous Remedial Effects hitherto observed in the Non-oxygenated Essential Oils, namely, the Oils of Turpentine, Copaiba, Cubebs, Juniper, Savine, &c., with a Commentary.* By WILLIAM SELLER, M.D., F.R.S.E., Fellow of the Royal College of Physicians of Edinburgh.

The substances popularly termed turpentines owe the principal part of their medicinal effects to the presence of an essential oil. This essential oil, though existing under a somewhat modified form in different turpentines, is sufficiently represented by the well-known essential oil of turpentine of English commerce. That the turpentines owe their medicinal efficacy to the essential oil of turpentine is now so generally acknowledged, that, in proportion as the oil of turpentine has gained ground as a safe remedial agent, the several turpentines have justly fallen into comparative disuse. The oil of turpentine is a non-oxygenated essential oil, that is, it is a compound of hydrogen and carbon. The other non-oxygenated essential oils, some of which seem to be absolutely identical in composition with oil of turpentine, are, the oil of copaiba, the oil of cubebs, the oil of black pepper, the oil of juniper, the oil of savine, the oil of the rind of the lemon, the oil of the rind of the orange, the oil of neroli (that is, the oil of the flowers of the orange), the oil of the leaves of the orange tree, the oil of elemi, and the oil of storax.

To point out some remarkable medicinal effects in these oils, with their several analogies, as suggestive of important practical conclusions in therapeutics, is the purpose of this paper.

These oils, and the substances in which they exist, all rank as stimulants of an active character. It cannot be disputed that their physiological effects are unequivocally stimulant, and yet some of their most striking remedial uses are afforded in diseases with the character of which, according to every common rule, they should be altogether incompatible.

The disease in which the largest number of these stimulants has been used is gonorrhœa. The turpentines in former times were largely in use for the cure of acute gonorrhœa, notwithstanding that speculative authorities were continually striving to prohibit their employment, except in the chronic forms. Oil of turpentine has not come into use as a popular remedy in gonorrhœa, but it is employed often enough by some practitioners, to leave no doubt of its efficacy both in the acute and chronic forms of that disease. In gleet, combined with sesquioxide of iron (the old precipitated carbonate), it is an approved remedy.

The utility of the balsam of copaiba, in the most acute forms of gonorrhœa, can no longer be disputed. It seems certain that the balsam owes its efficacy to the essential oil which it contains. The European practitioners, who first learned the use of this balsam from the natives of South America, employed it in the acute stage of the disease. Speculative views of its irritant consequences led to the disuse of this mode of treatment, which, however, became revived in France about the beginning of this century, and at a somewhat later period in this country. It is vain to urge that the first stage of gonorrhœa is often a mild disease, even in first attacks, and that it is only in such circumstances that copaiba fails to do mischief in that stage. It is beyond doubt that our best authorities for the efficacy of copaiba in acute gonorrhœa have been quite alive to the fact that there is much variety in the degree of acuteness of the commencing stage, whether it belong to a first or a subsequent attack. One author of credit, writing at an early period of this century, says, " My experience has taught me to pursue entirely a different course. Commencing with the medicine on the very accession of the disease, I am utterly regardless of all the appearances of inflammation, such as *ardor urinæ, chordee, &c.* No remedy, indeed, is better calculated to relieve these very symptoms than the copaiba itself."[1]

It would be easy to cite numerous other authorities to the same effect. The following comprehensive passage, however, from Trousseau, will more briefly serve the purpose of this summary. "To demonstrate how far *a priori* views—rules resting upon the mere speculations of an imperfect physiology—in short, the superficial rationalism of the age—is destructive to the progress of therapeutics, we have only to cast our eyes on the ideas which physicians in past times have imposed on themselves as to the action of copaiba in gonorrhœa, and on the urgent directions which they address to us not to employ this remedy unless in chronic atonic discharges from the urethra. We have but to consider with what precision of terms they proscribe its use in the acute and inflammatory period of this affection, founded, as they believed, on the hazard, or rather on the certain danger, of exasperating all the symptoms, and producing a host of evils by the premature employment of this treatment. They reserve it, in a very moderate dose, for the period when nothing remains but slight mucous discharge of very light colour, or for what is termed a gleet or chronic gonorrhœa. What, then, has since happened? How many trials, arising, doubtless, for the most part, from accidental circumstances, and from an empiricism, rash and censurable in itself, have brought to light, that, in the great majority of cases, the exhibition of copaiba in very large doses, and at the most violent onset of the most acute gonorrhœas, without any preparatory soothing antiphlogistic treatment, has not only not aggravated the disease, but, on the contrary, has energetically subdued it, and without any inconvenience, has freed the patients from suffering in a shorter time than any other method of treatment."[2] In short, notwithstanding the exceptions taken by writers on surgery and materia medica against the use of copaiba when the symptoms are highly acute, it is a therapeutic fact that copaiba subdues the inflammatory stage of gonorrhœa.

There is no reason to doubt that the balsam owes all its curative properties to the presence of the essential oil. The oil of copaiba is now officinal in the Edinburgh Pharmacopœia. It constitutes from 40 to 45 per cent. of the balsam.

The reputation which cubebs has obtained for the cure of gonorrhœa in all its stages, is now universally regarded as dependent on the essential oil which it contains. The use of cubebs in gonorrhœa was derived, between thirty and forty years ago, from India, where its efficacy against the disease appears to have been long familiar to the native inhabitants. It was previously known in our works of materia medica as a stimulant. But so long as its use was governed by what is called rationalism in practice, acute gonorrhœa was one of the last diseases in which it could have been prescribed. It owes its introduction into the list of remedies against the acute form of this disease to observation unequivocally empirical. The oil is now officinal in the Edinburgh pharmacopœia.

---

[1] Chapman's "Discourses on the Elements of Therapeutics and Materia Medica," i. 261.

[2] Trousseau and Pidoux, Traité de Therapeutique, ii. p. 592–3. Paris, 1851.

Of the other oils allied in chemical composition to the oils already spoken of, the only two which can be said to indicate analogous effects in the cure of gonorrhœa are the oil of black pepper and the oil of juniper.

The oil of black pepper is not officinal. In many respects it resembles the oil of cubebs. In dyspeptic complaints the oil has been used as a stimulant, but never, as far as appears up to this time, in gonorrhœa as an isolated substance. But the black pepper itself, as a substitute for cubebs, has been made use of with success in the several forms of gonorrhœa.

As to the oil of juniper, it is certain that it is long since it came to be employed with advantage in that disease.

The remaining oils of this group have probably never been tried in this disease. Yet it will not be surprising if the oils of savine, of the rind of the orange, of the rind of the lemon, of neroli (that is, of orange flowers), of orange leaves, of elemi and storax, or some of them, be found to partake of the same virtues in gonorrhœa as the five analogous oils which have been successfully put to the proof.

While, however, it is surprising enough that stimulants, like the five oils hitherto spoken of, should prove curative agents in the acute forms of gonorrhœa, it is not to be forgotten that this disease, even in its most active state, is but a limited inflammation, and that a great deal of the suffering attendant on many cases is owing to the extreme excitability of the adjacent parts, being merely symptomatic, and not the direct result of an extensively inflamed tissue. It should be added, that even the constitutional disturbance occasionally attendant on a first attack of gonorrhœa in excitable persons is not of the nature of the usual fever symptomatic of inflammation, but rather a species of febrile irritation, partaking in no small degree of the character of hectic. There is far, then, from sufficient ground here to run into the opposite extreme with the hasty conclusion that stimulants in general are not forbidden in acute inflammation. All that flows strictly from the facts stated is that certain peculiar stimulants exercise a more direct influence on the cure than common antiphlogistic treatment, the disease being a limited acute mucous inflammation of an organ unconnected with the organic life of the individual.

The analogous curative action of several of the essential oils of this group, particularly those of turpentine, copaiba, cubebs, and juniper, is well illustrated by their long-recognised common effect in catarrh of the bladder; but that effect need not be dwelt on at present, since it is one which might be expected to flow from their ordinary physiological properties.

There are other mucous discharges in which several of the same group of essential oils have obtained a reputation, as in chronic catarrh, chronic dysentery, and chronic diarrhœa. The turpentines and copaiba have long been employed in such affections with unequivocal advantage. The utility of the confection of black pepper (the substitute for Ward's paste) in fistula, piles, and ulcers about the rectum, belongs to this head. Mr Pereira quotes the opinion of Sir Benjamin Brodie on this medicine. Sir Benjamin thinks that severe cases of piles are sometimes cured by it. It requires to be continued for a long time. In some cases the confection has been introduced into the rectum with advantage. An old remedy for diarrhœa, when astringents have failed, is a scruple of black pepper, with a moderate dose of syrup of poppies, administered twice a-day.

The local action of the oil of turpentine in burns is somewhat analogous to the effect of oil of copaiba and oil of cubebs in acute gonorrhœa. The oil, when applied to the burn, undoubtedly soothes the pain and irritation under which the patient is suffering; and, what is singular, if the oil be allowed to touch the adjacent sound skin, the patient often complains loudly of the uneasiness so produced. No account has hitherto appeared of analogous effects in any of the other oils of this group on burns, but the beneficial influence reported of the oil of lemons in some inflammatory affections of the eye presents a striking analogy. This practice was first tried by the very simple expedient of squeezing into the eye little drops of oil from the rind of a lemon. Worlitz, to whom the idea seems to have first occurred, insists on its good effect in rheumatic, catarrhal, and scrofulous inflammations of the eye, in pannus and pterygium, and in opacity and some other consequences of inflammation of the cornea.[1] A like practice has since been adopted by Mr Foote of the London Ophthalmic Hospital, who employs the oil of lemons in the same way as the vinum opii is made use of. He thinks it preferable to vinum opii in all cases where a stimulant is required. He admits, however, that in a few cases it causes excessive pain. It accords with the plan of this paper to place in juxtaposition with these statements what has been said of the employment of oil of turpentine in some inflammatory diseases of the eye, both in the form of collyrium, and also, as it would appear, with greater success as an internal remedy. The chief authority for the external application of oil of turpentine to the eye is Laugier; but it must be confessed that he has hardly succeeded in pointing out the precise cases of ophthalmia and blepharophthalmia in which the remedy so employed is of much service. As to the internal use of oil of turpentine in inflammatory diseases of the eye, there is a host of authorities. Carmichael of Dublin was probably the first to recommend the internal use of oil of turpentine in chronic inflammations of the iris and of the choroid. Guthrie subsequently added his testimony to its utility in such diseases. Flarer of Pavia afterwards took up the subject. From his account, as reported by his pupil Trinchinetti, it appears that oil of turpentine succeeded after the other usual remedies had failed in cases of primitive iritis, both acute and chronic in their character, but particularly in chronic iritis, consequent on operations on the eye. According to the same author, this remedy abates the local pain, calms constitutional disorder, diminishes ocular congestion and lacrymation, while it favours the absorption of pus or blood existing between the layers of the cornea, or in the anterior chamber of the aqueous humour. Flarer recommends the addition of bicarbonate of soda when gastric disorder arises during its use.

---

[1] See Pereira, Materia Medica, vol. ii., p. 1239.

The curative effects of oil of turpentine in rheumatic and neuralgic affections, being observed chiefly in those of a subacute or chronic character, present no feature at variance with what might be expected from its physiological action. In the rest of the group, the analogies to this action hitherto observed are not numerous. Of savine, which plainly owes its medical properties to the presence of its essential oil, Chapman speaks highly as a remedy in chronic rheumatism, when accompanied with a languid circulation in the extreme vessels. It is hardly necessary to remark how desirable it is to ascertain how far such remedial uses of oil of turpentine belong to other members of the group which have the advantage of being more easily administered.

Another extensive field of inquiry is, how far the allied oils agree with oil of turpentine in its alleged curative effects in epilepsy, chorea, hysteria, croup, colic, and jaundice.

A proposal to use the orange-flower water, the virtues of which must depend on the presence of the oil of neroli, in any serious case, owing to its analogy with oil of turpentine, would be apt to raise a smile in this country at the present day. Dr Christison, however, says of the orange-flower water, "the distilled water of the flowers is probably a more active remedy than is commonly thought in this country. In France, it is held in estimation as a calmative and anodyne, in the slighter forms of functional disease of the nervous system; and I have known it to produce sleep in the milder forms of hysteria, when taken in the dose of one or two ounces." The oils derived from the leaves, and from the rind of the fruit of the orange, have the same chemical properties and composition as the oil of the flowers; and when we consider the antispasmodic virtues ascribed to the leaves by so many high authorities in the last century, perhaps, on a review of the whole circumstances, it may not be deemed unwise to assent to the probability of these oils making a nearer approach to an analogy in antispasmodic effect with the oil of turpentine, than has been of late thought credible.

The history of the rise of the reputation of orange-tree leaves will be found detailed by Murray in his "Apparatus Medicaminum." It is not improbable that, among the causes of the decline of their reputation in disorders of the nervous system, one of the chief was the frequent substitution of the leaves of other plants by fraudulent druggists. Trousseau, whose work stands so high among the most recent writings on therapeutics, speaks with a slight degree of doubt on this medicine. He says,—"I have seen it succeed in convulsive cough; it is still believed to exercise some influence on the frequency and the intensity of epileptic attacks, the dose being half a drachm."

One of the most singular coincidences in remedial effect between oils belonging to this group is, that the oil of lemons in doses of from one to two drachms is regarded, like the oil of turpentine, as a good anthelminthic in tape-worm. It is not unlikely that some others of these oils may participate in the same property. The oil of savine, though not in use as an anthelminthic, is acknowledged to possess that property.

Nothing analogous to the alleged utility of oil of turpentine in puerperal peritonitis, passive hemorrhages and erysipelas, has been noticed in the other oils of this group, yet it would be an omission to pass by these alleged virtues without notice in a Paper professing to detail the remarkable remedial effects of the group to which oil of turpentine belongs. In the last stage of fevers, with tendency to coma, whether these be idiopathic, like our common typhus, or symptomatic, like those accompanying a low state of inflammatory action, there can be no doubt that oil of turpentine often exercises a very beneficial effect. Whoever has seen the hopeless-looking cases which in every epidemic of typhus recover in numbers under the frequent use of turpentine-enemata and blistering of the head, will hardly doubt that the use of oil of turpentine may often prove serviceable in attacks of inflammation of a low character, accompanied with fever, tending to a typhus-type. But the question still remains, how near may the disease approach to a simple uncomplicated attack of internal inflammation without being subject to aggravation by such treatment? This is the point which demands examination by those to whom favourable opportunities for that purpose may be presented. Trousseau has examined with much pains the evidence afforded by English authorities in behalf of the claims of this remedy in puerperal peritonitis, and comes to the conclusion that that evidence is insufficient. The following passage, however, will show that he has accorded no favour to the remedy, while the end of the quotation points to a subject well deserving of our consideration on this side of the channel; for the frequency of this remark in France is very discouraging in therapeutics, whether its source be ascribed to a jealousy, on the part of the French, of remedies originating in England, to a difference of constitution in the inhabitants of the two countries, or to overhasty conclusions on our part as to the effects of treatment in diseases. "What confidence should be placed in the labours of those English physicians who strive to prove the signal efficacy of the essential oil of turpentine in puerperal peritonitis? We state the question in these terms, that in the sequel may appear our distrust in the value and solidity of these labours, and to prevent ourselves from conceding to them anything which it shall not be rigorously impossible to refuse. It is between them and us a pure question of diagnosis; and we should declare beforehand that the motives to our doubts result from the insufficiency of the particulars required to characterize the malady; also from the marvellous and incredible effects related, as well as from the experience we have had of being hardly ever able to verify upon our patients what the English physicians observe in their hospitals."[1]

But an important question is suggested by the contrariety so manifestly existing between the ascertained medical uses of some of the members of the group of bodies under consideration and their physiological effects, namely, whether in the cultivation of therapeutics so great an influence as undoubtedly operates at present should be permitted to physiological effects, and the therapeutic orders founded on these, in determining what agents should be selected for the treatment of such and such diseases.

---

[1] Trousseau et Pidoux, op. cit. ii. 564.

It is quite true that in the early ages of medicine every medicinal substance was naturally regarded as a specific against some particular disease, and that many errors became introduced under this mode of proceeding, not because it was in itself erroneous, but because it was carried out by a system of imperfect observation. Ever since remedies became reduced to a tolerably exact system of therapeutic orders, it has been common to consider the remedial effect of any substance in the cure of disease as a particular instance of the general effect belonging to that order—purgative, emetic, tonic, or alterative, as the case may be—under which the substance in question stands. There can be no doubt that, as a general rule in the treatment of diseases, this view of the matter is correct; but if it be adopted exclusively, it carries with it the total abolition of the idea of specifics. Now, a specific signifies nothing more than a substance which cures a disease by a kind of action on the living body which we cannot yet explain. There never was a better example of a specific than sulphur in the cure of scabies. It has ceased to be a specific, because we now know that it cures scabies by poisoning the animalcule on which that disease depends; and this fact in the history of medicine clearly proves that, if not now, there was once room for an order of specifics in therapeutics. There still remains one specific which resists being confounded with any therapeutic order, namely, quinine in the cure of marsh diseases.

But a very serious question here arises, whether we have abolished specifics to this great extent by really penetrating the *modus operandi* of most substances in the cure of disease, or only by assuming that we have attained a kind of knowledge of physiology of which we are still ignorant. The facts detailed in the first part of this paper unequivocally show that we are still in the latter predicament. To speak of specifics is undoubtedly to confess ignorance; but our case is not mended if it be discovered that the ground on which we have discarded them is but an assumed knowledge. We cannot explain why the oils of turpentine, copaiba, and cubebs, cure acute gonorrhœa; and doubtless there are many other cases in which the *modus operandi* of remedies is equally obscure though assumed to be known.

There is nothing absurd in the supposition that any one substance may have a special action on the living frame, as well as a general action in common with a number of other substances. On the contrary, it is almost certain that every substance has a definite special action.

If such views are not actually opposite to the truth, very probably there is room, by close observation, for the discovery of many special properties applicable to the treatment of diseases in the substances grouped under one general therapeutic order, even when the general property which is the ground of their association into one group is to appearance at variance with the presence of the special property. That the existence of a common medicinal agency in a group of substances does not preclude the presence of special properties in the several individual substances concerned, is too obvious to require any comment. How many special properties, for example, belong to the individual substances composing the group of purgatives!

One general maxim then flows from such considerations; namely, that the special properties of each individual substance should be carefully investigated, while nothing should be held less in accordance with the philosophy of therapeutics than to rest content with having referred a medicinal substance to a certain physiological order. To take an example not the less instructive because it is from a science essentially beyond the pale of the economy of living nature—how little progress would chemistry have made, had chemists confined themselves to referring the several bodies with which that science is concerned to such groups as acids, alkalies, earths, metallic oxides, and the like? Is there anything better known than that the progress of chemistry became rapid from the moment when it was felt that nothing but the minute study of the properties of each individual gas, liquid and solid, could avail to advance the certainty of chemical operations. Every one acquainted with practical chemistry, knows how apt an unrehearsed experiment of the best known and most simple kind is to fail; and what is the application of remedies in a disease but an experiment, not in simple chemistry, but in vital chemistry, in which last form of chemistry the accidents determining to an unexpected result are multiplied beyond measure? Why do chemical experiments so often fail? For the simple reason that the hasty chemical experimenter trusts too much to leading general rules, forgetting to take into account all the more minute circumstances which may at the moment disturb the expected operation of these rules. He is too much of a rationalist in chemistry and too little empirical; that is, too little of a practical experimentalist; too little observant of and attentive to the special properties of the substances concerned; and the lesser agencies which may interfere with the result. Chemistry, nevertheless, is very much less complex, very much less dependent on the individualities of the subjects and objects concerned in such experiments, than the department of therapeutics. Every attempted operation in therapeutics involves innumerable causes of failure. These causes of failure are to be best avoided not by studying, however desirable that may be on other grounds, the general laws affecting the several classes to which the patient as an individual, or the remedial agent as an individual, belongs; but by carefully imitating every circumstance, even the minutest, when possible, which was present on the occasion when the result was that which is desired a second time. How different is this kind of foresight from what is termed the exercise of rational medicine! Every circumstance to be dealt with is individual; the individuality of the patient, the individuality of the medicinal agent. To secure the result, the mere extra trouble of doing things which are unnecessary is not to be grudged. The votary of rationalism continually risks the failure of the experiment by the desire to discover at once the minimum of the steps absolutely requisite.

Let us not, however, carry this argument for the experimental too far, or beyond its legitimate limits. Men ignorant of the science on which the art they practise rests are less fit to conduct experiments in which any of the circumstances are new and therefore unknown, than those who, with less practical experience, are masters of the science. To be perfect in practice, a man must know all the science applicable to the process which he conducts, as well as all the influences of all the accidental circumstances which may interfere with his success. To improve the practice of medicine, a man should be acquainted with all the medical science of the day, at the same time that he is an exact observer of such a kind as makes him a sure experimentalist. Many a man of science, in chemistry for example, has no turn for that exactness which ensures success at once in each experiment. Such a man has often the genius to conceive the conditions of a new experiment, while by the omission of slight particulars he perpetually fails of success through many repeated efforts, and at last, it may be, only blunders into the right path by accident. But to fail in one therapeutic experiment may be destructive to the patient. In the treatment of disease there is no room for rehearsing. It is no diminution of a speculative chemist's reputation how many times he fails before he obtains success in each new experiment. It is only so much glass broken—so much material thrown away; but in a therapeutic experiment such as that in which a patient is to be saved or lost, the man who can perform the experiment with the least risk of failure, be he a man of original genius or a mere copyist, must be preferred. Let the case be changed to a surgical operation, and the illustration is complete. A dexterous surgeon is not made by reasonings, but by the knowledge of particulars.

If then the progress of chemistry afford us a proper lesson as to the mode in which therapeutics should be cultivated, nothing is more certain than that the special effects of each remedial agent should be studied to the utmost possible extent, and that the arrangement of the articles of the materia medica into what are termed therapeutic orders should be regarded as merely a first step in the progress of therapeutical knowledge — one subservient merely to the convenience of the student in his first advances.

It is unfortunate for the progress of therapeutics that so many difficulties interpose to prevent the acquisition of a more certain knowledge of the effects of medicines on the animal economy, whether in health or disease. There is a continued influx of new remedies. A few of these, doubtless, turn out to be even of signal efficacy. But there is the great attendant evil that it interferes with the application of the energies of the medical profession to the attainment of a more extended knowledge of the real operation of those already in established use. What adds to this difficulty is another evil, not always unmixed with good, namely, the continual revival, in all the freshness of novelty, of remedies which had become obsolete in a former age. Foxglove and colchicum are remarkable examples of this kind of fortune in articles of the materia medica, and many others might be referred to.

These, however, are evils which cannot be put down. They must be submitted to. But that should not hinder those who have opportunities from endeavouring to add to our exact knowledge of the effects even of common substances on the animal economy. This is a kind of study in which young men should not fail to engage. They could not be better employed than in applying their microscopes and their skill in chemical analysis to the improvement of our acquaintance with the effects of remedies on the blood and on the secretions in diseases and in health.

Among other difficulties, one perhaps greater than all the rest is, that the very language of therapeutics has not attained that exactness which would prevent those who cultivate the subject from perpetual misunderstandings. This difficulty, it is true, is common to all the subjects of knowledge to the conclusions of which such tests as the fixed symbols of number and quantity cannot be applied. Were such a thing possible—to introduce a new definite nomenclature into therapeutics would be a vast stride in advance. Some improvement at least on this point need not be altogether despaired of; but therapeutics proper have been too much neglected of late, the attention of writers on medicines being so largely absorbed by the chemistry and natural history of the substances of which they treat.

The rarity of discussions on pure therapeutics, at least in this country, will perhaps excuse the few additional observations which follow.

Even some of the terms made use of in the previous parts of this Paper do not convey to all the same ideas. The term specific, which most surely ought to have no other signification than that assigned to it above, namely, that it is a curative agent whose mode of action is still unknown, figures in the minds of many as some monstrous superstition of ancient times. A specific is a remedy which generally, but does not invariably, cure. When a remedy is described as possessed of the power of curing a disease, it cannot justly be inferred that there are no circumstances under which the disease will not yield to its effect. The idea of a therapeutic agent being capable of curing a disease without the possibility of failure, is so totally foreign to all proper notions of medicine, that it must be rejected from the definition of a specific. According to some authorities, a specific is a therapeutic agent which cures without any physiological action, or without any apparent physiological action. As to the first case, how can it be told, in such a system of minute parts as the animal frame is composed of, when physiological action has or has not taken place? And in the second case, how can it be discovered whether, in cases where evident physiological action has occurred, whether that action be that on which the specific depends for its effect? Sulphur was once a specific in scabies, yet it exerted evident physiological action in several ways, on none of which, as we now know, did its specific effect depend. In short, as said before, the type of a specific is what sulphur was to scabies before the discovery of the itch-acarus; and if this were accepted as a universal understanding, it would be a certain step of advance in precision of therapeutic nomenclature.

Again, there is not much of distinct understanding as to what constitutes the difference between rationalism in medicine and the only form of the empirical—namely, empirico-rationalism—which still holds its ground in modern times. The empirico-rationalist admits the knowledge of all that can justly be called law, principle, or natural history, in medicine, to be essential to the professional character of a physician; but when he is challenged to treat diseases on rational principles, he finds the thing, according to his ideas, to be impossible, except in a comparatively small number of cases. The empirico-rationalist treats the major part of diseases on the ground that such and such treatment succeeded in the hands of certain well-known authorities, and he consequently expects to succeed by following their example as closely as the circumstances will permit. He frequently acts on rules, but these rules are for the most part empirical; that is to say, they are rules of art, technical rules—not rules deduced from a strict principle or law. It is true he does not—like some of the ancient empirics—reject physiology. He adopts, as said above, all that can be called science in medicine, naming himself therefore not an empiric, but an empirico-rationalist, and contending that the distinction between the empirical and the rational obtains no place now except in the department of the treatment of diseases, and reserving to himself the full liberty of treating diseases rationally as often as it is permitted. For example, he prescribes magnesia for the cure of heartburn, because he knows that heartburn depends most commonly on the presence of an acid acrimony in the stomach. It often happens, however, that when the empirico-rationalist demands of his brother physician, who is hourly announcing himself as a pure rationalist, whether he proceeds on any other plan in the treatment of diseases than by imitating the practice of those whom he has known to obtain the best success, his opponent probably admits that he has no other plan, saying that he proclaims himself a rationalist because he is continually employing the microscope to detect the simple elements of disease; so that he altogether ignores the sense in which his opponent pronounces the views he entertains to be empirical. Such is the prevailing confusion of terms in therapeutics Every man should be a rationalist as far as rationalism can be justly carried; but continually to insist on treating diseases rationally, is to mislead the unwary into mistaking reasonings for reason—words for things.

The language of surgery, as respects the cure of wounds, is more definite than that of pure medicine as respects the cure of diseases. It is undeniable that, as wounds heal spontaneously, so diseases often become spontaneously cured. It is equally true that there are some states of the general health in which wounds do not heal until, by remedial means, such or such a state of health has been corrected. It is equally true, that diseases which become cured spontaneously under the ordinary circumstances of the constitution, fail of this spontaneous cure when certain derangements of the general health coexist, unless remedial means are taken to correct the existing aberration. Moreover, when diseases require a certain treatment for their cure, that treatment only succeds when there is no unusual coexisting derangement of the constitutional health, at least until that coexisting unusual derangement has been made to yield by an appropriate treatment.

When then a disease disappears under the use of a remedy, or when it fails to disappear on one occasion under the use of a remedy during the employment of which, either alone or in combination with some other remedy, on another occasion it had passed away; there are several questions to be settled before the treatment made use of can have its proper therapeutic place assigned to it. The substance employed may have been wholly inert, while the disease may have ceased spontaneously, or under the accompanying regimen, as is seen to occur every day—imposing upon the unwary a belief in the efficiency of the most inert treatment. The substance employed may have removed some latent derangement of health, so as to permit the prominent disease to pass away spontaneously, or to yield easily to the use of a subsequent remedy. How many disappointments must occur daily, so long as the state of therapeutics fails to enable us to solve such questions in the treatment of diseases!

One of the best illustrations that can be given of a latent derangement of health, such as may prevent the spontaneous resolution of whatever diseases have arisen during its continuance, and the effect of ordinary remedies on those which require treatment, is the scorbutic tendency. This scorbutic tendency can no longer be regarded as a mere hidden mystery, a mere fanciful impediment to the restoration of health in diseases, even by those who pin their faith most strictly to ocular proof. By scorbutic tendency is here designated not some obscure constitutional diathesis, but an actual, though perhaps temporary, approach to the dreaded scurvy—the sea-scurvy of former times. Ever since the rise of the potato-disease, there has been the most unequivocal evidence that, to produce at once among thousands of our population the most perfect form of that disease which was once thought to be almost peculiar to the sea service, nothing is required but the kind of insufficient sustenance afforded by an erroneous diet. And taking this evidence, together with our ample knowledge of the frequent vagaries of invalids in all stations of society as to their diet, who will doubt that among the causes of the failure of treatment in chronic diseases, even among the luxurious, must often be a tendency to scurvy, and that not slight but often so far gone as to require only to be suspected to be discovered.

Of the great efficacy of lemon juice in scurvy no reasonable person entertains any doubt, even while it is acknowledged that it was at one time overrated. Lemon juice, after the recommendation of Dr G. O. Rees, has had considerable apparent success in rheumatic diseases. It would be worth while for those who follow out this practice to examine if, in the cases in which it is successful, there be not a scorbutic tendency present.

The defective elimination of certain ordinary constituents of the urinary secretion, even when it does not amount to the gouty habit, is very probably another distinct example of latent derangement of health sufficient to prevent, during its continuance, the spontaneous cure of diseases, and the ordinary effect of remedies on such as require treatment. This subject is still somewhat obscure, notwithstanding the great advances made in it of late; yet enough is understood of it to afford an illustration of what is at present under consideration. The good effects of colchicum in the treatment of many chronic diseases can no longer be doubted, notwithstanding that this is one of the substances which many eminent French authorities hold up as an example of a medicine from which they in vain try to obtain the curative properties ascribed to it in England. It is however certain that colchicum usually increases the amount of urea and uric acid thrown off by the kidney in a given time.[1] By this effect, then, it is reasonable to conclude, it restores the system to that state of health in which some diseases disappear spontaneously, and others become susceptible of cure by a treatment efficient for their removal in the ordinary general state of the animal economy.

There are some grounds to anticipate that the group of essential oils, the singular remedial effects of which have given rise to these somewhat unduly lengthened reflections, may be found to operate by the elimination or modification of the constituents of the urinary secretion. It is already known that the oil of turpentine, like colchicum, augments the separation of uric acid from the blood, and that the oil of copaiba renders the urine coagulable by nitric acid.

---

[1] For an excellent Memoir on Colchicum, by Dr J. M'Gregor Maclagan, see Edinburgh Monthly Journal of Medical Science, December 1851, and January 1852.

PART II.

CRITICAL ANALYSIS.

ART. I.—*On the Structure and Use of the Spleen.* By HENRY GRAY, F.R.S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum of St George's Hospital. 8vo, pp. 380. London: John W. Parker and Son. 1854.

The structure and functions of the spleen, and of the ductless glands, are studies of unusual difficulty, and for ages have remained unsolved problems. The researches concerning them by the older anatomists proved singularly barren of positive results; the statements and descriptions of authors were inextricably confused and contradictory; and to most minds the information acquired or attainable seemed so imperfect and worthless, that at last the subject began to be abandoned as impracticable, and was fast sinking into neglect and obscurity. Of late, however, a new interest has been created, and a spirit of inquiry aroused, in consequence of the impulse which the discoveries of Schwann and his followers have given to anatomical and physiological science. By the cell theory the processes of nutrition have been explained with a clearness, simplicity, and force of demonstration, never before approached; the structure of true glands has been revealed in its minutest elements by the aid of the higher powers of the microscope; their function has been shown to depend on a uniform process of cell-growth and decay; and the chemical changes which occur in them have been brought within the scope of accurate analysis. In this advanced condition of our knowledge of the true glands, it was reasonable that the progress of inquiry should be next directed to the more difficult structures, the spleen and the blood glands. Accordingly, numerous observers simultaneously, or in quick succession, have contributed their labours; every means of modern research has been applied to the investigation; and within a few recent years, so much valuable information has been accumulated in regard to the structure, function, chemical composition, nay, even diseases of the spleen, and of the blood in connection with it, that we cannot neglect the present opportunity of calling the attention of our readers to these rapidly advancing and important inquiries.

Mr Gray's treatise on the spleen was composed for and obtained the Astley Cooper prize for 1853—a triennial endowment munificently founded by the great practical surgeon for the encouragement of scientific inquiry. The work well deserves its honourable success, and forms an excellent sequel to Mr Simon's masterly essay on the thymus gland. We do not, indeed, find in it, nor could we expect, any great discovery, or any startling novelty in fact or theory; nor has any new path of investigation been opened up; but there is, to a remarkable extent, completeness and faithfulness of research. The author has, with the assistance of some able colleagues, gone over every department of the subject—anatomy, chemistry, physiology; the observations of previous writers have been revised, confirmed or corrected, and generally extended by some important additions; while some parts, as the embryology and comparative anatomy, are almost entirely new. There is throughout that independence and originality which result from an author having particularly examined, and candidly reflected on every fact which is recorded; and the whole execution of the work is such as to establish Mr Gray's reputation as an accomplished scientific anatomist.

In the remarks we have to make we propose to follow the divisions of the subject which we find in Mr Gray's book; passing lightly over some parts, and referring more fully to others, in proportion as the importance of the subject, or the peculiar observations or views of the author, may require. These sections are—Part I. Historical Introduction. Part II. Development. Part. III. Structure, including the chemical analyses of the Spleen and Splenic Blood. Part IV. Comparative Anatomy. Part V. Physiological Conclusions.

PART I. The Historical Introduction is particularly necessary for understanding a subject so little generally known or studied, but fortunately its important points may be summed up in a few sentences. Before the discovery of the circulation of the blood, the notions entertained in regard to the spleen were entirely theoretical and fanciful; it was generally thought to attract the black or melancholic humours of the body, an opinion from which the popular signification of "the spleen," and "splenetic," in our own language is derived. But after anatomical science was really established, the researches are easily distinguished as of two kinds. First, the descriptive or larger anatomy was fully investigated, particularly by Malpighi, Ruysch (the great injector of bloodvessels), Eller, De la Sône, Heusinger, &c. This was followed up by the minute or histological anatomy, which inquired into the ultimate elements of the spleen, either in conjunction with the descriptive anatomy, or sometimes more partially with the purpose of observing the physiology rather than the structure. These histological inquiries, to which indeed Leeuwenhoek's observations belong, was properly begun by Hewson, and has only of late years (and especially since Schwann's discoveries) reached its full extent in the researches chiefly of Müller, Oesterlen, Kölliker, Ecker, Schaffner, and Günsburg in Germany; Bourgery in France; Spring and Poelmann in Belgium; Home, Evans, Handfield Jones, Simon, Sanders, Wharton Jones, Huxley, and Gray in Britain.

Among the physiological opinions there are two that merit attention; the one originated by Malpighi, that the spleen pours some secretion directly into the venous blood; the other that of Hewson, that, in common with the other ductless glands, and the lymphatic system, the spleen forms the red particles of the blood. The association, indeed, of the spleen and lymphatic system in effecting the proper constitution of the blood, has been adopted by many physiologists, in particular by Tiedemann and Gmelin, and recently by Funke and Bennett, who conceive that the spleen forms the white particles of the blood.

A peculiar mechanical function has also been generally assigned to the spleen, and was greatly insisted on by Stukeley; viz., that the spleen, by its large and distensible veins and (supposed) cells, served as a diverticulum or reservoir for the superabundant blood of the circulation,—a use which was more fully illustrated by Dr Dobson's experiments on the increase of size of the spleen during the process of digestion. A very curious circumstance brought out in the historical accounts is the facility and frequency with which the spleen has been excised in animals, and the entire absence of any injurious effects from its removal. It would appear that at one time this experiment was so common, that in some university town (Leyden, we think) it was almost impossible to find a dog which the zeal of the student had left in possession of its spleen; and we are even told that in olden times the operation of excision of the spleen was successfully performed on man for the cure of hypochondriasis!

In a historical summary it is hardly necessary to mention the chemical part of the inquiry; for although begun by Malpighi, it has only yielded important results in our own day through the labours of Beclard, Scherer, Funke, and Lehmann. The pathological inquiries, especially connected with the interesting subject of Leucocythemia, do not come strictly within Mr Gray's range of observations; we may perhaps cursorily allude to them in the sequel.

Mr Gray's historical analysis, which is arranged in the order of time, is similar both in matter and form to previous accounts. The earlier history shows considerable erudition; but we should greatly have preferred, if Mr Gray had added instead of it, as he was well prepared to do, an analysis of recent investigations, previous to which his historical introduction somewhat abruptly stops. Before leaving this part of our subject, however, we must notice a material error which appears in the account given of Malpighi's description. At p. 15 we find it stated: Malpighi "supposed that the septa (*i. e.*, partitions between the venous cells) communicated directly with the veins;" and further on, "that the glandular bodies at times burst, discharging their fluid contents, which were poured into the membranous septa, and from thence conveyed into the veins." Now it is difficult to imagine fluid contents poured into a membranous septum; and Malpighi will be found, we think, to make an entirely different statement, viz., that the cells, *i. e.*, cavities or spaces, not septa or walls, received the fluid contents of the glandulæ, and communicated directly with the veins.

PART II. *Development of the Spleen.*—Mr Gray's observations on the development of the spleen before birth are new and original, and were first made known to anatomists in his paper in the Philosophical Transactions for 1852, "On the development of the ductless glands in the chick." He has shown that the spleen does not originate in connection with the pancreas from the intestinal tube, as Arnold and Bischoff had asserted, but arises independently in a fold of the "intestinal laminæ." It will be remembered that Professor Goodsir described the supra-renal, thymus, and thyroid bodies (and in his lectures he added also the spleen), as being formed from a mass of blastema which extended along the spine of the embryo from the Wolffian bodies to the base of the cranium, and constituted in fact the remains of the blastoderma, which still retained its primitive function of nutrition by the action of nucleated cell particles. Goodsir's account coincides therefore with that of Mr Gray so far as regards the independent origin of these ductless glands. And although Mr Gray, in the paper above alluded to, states that he could not find a continuous mass of blastema such as Goodsir describes, still Mr Goodsir's great generalization in regard to the origin and function of these bodies is very important to keep in view in the present condition of our knowledge in regard to them.

Mr Gray has also made the important observation of a distinct process of cell-formation and destruction taking place in the spleen-pulp of the embryo in connection with the evolution of the bloodvessels; and he found that the Malpighian vesicles were not developed till fœtal life was just completed—the period at which the nutrition of the body begins to be derived from without.

Indeed, the connection of the spleen with the nutritive processes, on which recent observers in Britain especially have always laid great weight, is a point of primary importance and interest. In the sequel, particularly in the chemical part of the inquiry, this will more fully appear. In the chapter on the development of the spleen after birth, some leading particulars are ascertained regarding it. It is shown by 160 observations of the weight of the spleen at different periods of life, that this organ is of considerable size, and consequently, in active function during the whole of life, that it reaches its maximum during adult age, and decreases in old age. And by a series of experiments on rabbits, it is proved that the size and weight vary according to the nutrition of the body, being greatly increased in highly fed, and diminished in starved animals; and further, that the spleen is enlarged during the process of digestion, being at its greatest size (in rabbits) about ten or twelve hours after feeding, *i. e.*, towards the completion of the process of digestion, when new material has been added to the blood. But in starved animals this increase of size during digestion did not take place. This intimate connection of the size of the spleen with nutrition not only points to its probable function, but explains especially those great variations in the size and weight of the spleen which are so commonly met with in examinations of the human body.

PART. III. *Structure of the Spleen.*—In this department of the subject we cannot, of course, enter into details; but must necessarily limit ourselves to new or controverted observations. And, first, in regard to the fibrous framework of the organ, which consists of three portions, the proper fibrous coat, the capsulæ or sheaths of the bloodvessels, derived from the fibrous coat, and the trabeculæ or fibrous filaments which, arising from the capsulæ and from the attached surface of the fibrous coat, forms a close anastomozing network through the substance of the spleen. It has long been a question among anatomists whether these textures are muscular or not. To the naked eye, no doubt, they appear to be composed of fibrous tissue, and they are never seen to contract like the intestines or the bladder; yet evidence both histological and physiological has been adduced to show their muscularity. Not to mention the older anatomists, we find that Kölliker, who has investigated the minute structure of involuntary muscle more fully than any other anatomist, has particularly described spindle-shaped cells, which he considers characteristic of involuntary muscle, in the fibrous coat and trabeculæ. Now, it is quite certain that the fibrous tunic, capsulæ, and larger trabeculae chiefly consist of common and elastic fibrous tissue, and spindle cells occur mixed with these only in certain animals (pig, sheep, &c.) In the minute or microscopical trabeculæ, on the contrary, these spindle-fibre cells are exceedingly common, and often compose the entire trabeculæ. Mr Gray gives a careful account of the different kinds of these spindle-fibre cells, and describes their formation from those curiously shaped cells (see Fig. 17) first noticed by Günsburg, and which are very abundant in certain, especially softened, spleens. From various considerations, and especially from their mode of development, Mr Gray concludes, in opposition to Kölliker, that these spindle-shaped cells are not muscular; and he consequently maintains that these membranous textures in the spleen are merely elastic, and not contractile. This conclusion appears to us highly probable: there is deficiency of satisfactory proof that these fibre cells are necessarily muscular, and the absence of these cells in the fibrous tissue of the spleen in many animals constitutes a decided argument against any *functional* necessity for a contractile texture. At the same time, we admit that these fibre cells in their various forms, which Mr Gray looks upon as a mere variety of ordinary fibrous tissue, do not appear as yet to be sufficiently understood, either in their nature or their use. In regard to the physiological proof on this question, Wagner first succeeded in producing slight contractions in the tunic of the spleen by galvanism; and the experiment was successfully repeated by Kölliker and Ecker. This proof would be decisive if confirmed and free from fallacy; but we find that Mr Gray has not been able to see distinct or well-marked contractions in experiments performed expressly for the purpose; and we know of many physiologists who have been equally disappointed. The question at present, therefore, remains in this position, that the presence of these spindle-fibre cells is fully recognised, but their muscular function is doubted or denied.

The bloodvessels are well described: the arrangement of the arteries, their sheaths and branches, the connection of their minute sub-divisions with the Malpighian bodies, and their final distribution in tufts of capillaries, are points on which anatomists are now agreed. Mr Gray has confirmed the curious observation, first made by Assolant, of the isolated distribution of the vascular branches, so that each main trunk supplies only that region of the spleen in which its branches ramify, and forms no anastomosis with the neighbouring branches. The description of the veins is also excellent. Their large size in ruminants, the thinness of their coats, and their modes of distribution, are clearly set forth, and well illustrated in the plates (Fig. 23). The observations most novel and worthy of remark, however, are on the origins of the veins, which Mr Gray describes to take place in three ways:—*1st,* By continuity with the capillaries given off by the arteries. *2d,* By intercellular spaces. This, we are informed is the most common mode of origin; and by it Mr Gray accounts both for the easy extravasation of artificial injections, and for the source of the disintegrating blood corpuscles which are found in the spleen pulp. *3d,* The veins rarely arise from distant cæcal pouches. The question of the anomalous vascular communication by intercellular spaces has long been one of the most difficult in regard to the spleen; nor are the doubts regarding it entirely dispelled by our author's observations, which we would gladly find confirmed, and the mode in which this peculiar circulation is carried on, determined by some of our anatomists who have attained peculiar skill in the art of minute injection.

With the exception of the pouches before-mentioned, Mr Gray denies the presence of venous cells in the spleen—a doctrine which is now nearly exploded. He has traced, however, an important relation of the venous radicles to the Malpighian bodies, each of which is described as inclosed by an imperfect capsule formed of the small primary veins—an arrangement which gives great additional probability to the view that the splenic venous blood is the vehicle which conveys away the secretion of the spleen.

Certainly the most elaborate part of Mr Gray's researches, and that which most advances our knowledge, is the inquiry into the constitution of the splenic venous blood. The chemical analyses in particular, by the able assistance, we are informed, of Dr H. M. Noad, are most complete and conclusive, in showing the relation of the spleen to the process of nutrition. The amount of blood in the spleen is found to be greatest in well fed, and smallest in starved, animals; and in well fed animals, the quantity is increased during digestion. The introduction of water also, or the direct infusion of blood into the circulation, increases the mass of blood contained in the spleen, and any cause obstructing the portal system, has a similar effect. On microscopic examination, the quality of splenic venous blood presents peculiarities of a remarkable kind. The white corpuscles especially are in excess, the red particles are altered in appearance, and free pigment granules are observed. The excess of white corpuscles first clearly pointed out in healthy blood by Funke, had been for some time previously a source of great interest in pathology, in the condition of the blood named Leukæmia or Leucocythemia. The blood discs contained in cells, similar to those in the spleen pulp, are also occasionally met with in the splenic venous blood. Mr Gray could not detect, however, any differences in the microscopical characters of the blood at the different periods of digestion.

Mr Gray's numerous and careful chemical analyses seem to set at rest the discrepancies in the researches of Beclard and Funke. His results have established that the splenic venous blood, when compared with the arterial and venous blood, whether of the systemic or portal veins, presents constantly the following differences in its constituents: *1st*, It contains less solid matter, the crassamentum is diminished, and the serum increased; *2nd*, It exhibits a great diminution in the blood globules, especially in well fed animals, and when digestion is completed; *3d*, There is a constant increase of the albumen, which however varies very much in amount, being generally most abundant when the blood corpuscles are much diminished, except at the completion of digestion; and in well fed animals, it is increased before digestion and during its early stages, while, on the contrary, in ill fed or starved animals the albumen is not at all increased during digestion; *4th*, The amount of fibrine is always increased, but varies also, the blood of ill fed animals yielding more than that of well fed; and when the blood particles are greatly diminished in number, the fibrine seems to be proportionally increased. In analysis, however, we presume that the colourless corpuscles must be included along with the fibrine ; *5th*, The residue of the serum, on evaporation, has a reddish brown colour, which is probably owing to ; *6th*, The increase in the amount of iron. These analyses prove most satisfactorily the differences of splenic venous blood, from all other blood, and they afford every probability that the splenic vein is, so to speak, the duct of the spleen conveying some nutritive material, such as albumen, &c. into the portal circulation. There is one investigation which we miss in this elaborate inquiry ; it is the very remarkable fact of the crystallization which was shown by Funke to take place in splenic venous blood, and which has been since detected in other blood. This curious subject deserved some study, from its bearing on the properties of splenic venous blood.

In the structure of the pulp, there is still further evidence of the probable nutritive function of the spleen. This soft reddish substance, which composes the chief part of the spleen, and fills up the interstices of the trabecular net-work, is found to consist principally of nuclei (corpuscles), granular matter, and plasma, with nucleated and granular vesicles in small quantity, affording an indication of a continuous process of cell-development and decay. By chemical re-agents, these organized particles are shown to consist of some proteine or nutritive compound, and while they occur abundantly in animals in a high state of nutrition, they are entirely absent, according to Mr Gray, from the spleens of starved animals.

Another theory of the function of the spleen, which has especially prevailed in Germany, has been founded on certain coloured particles which are met with in the spleen pulp of most animals. These yellow or reddish pigment particles, of the nature of blood pigment or hæmatin, were first noticed by Dr Handfield Jones ; but their formation from the disintegration of blood corpuscles within cells was first described by Kölliker and Ecker, who, in consequence, look upon the spleen as the special organ for the destruction of the red blood particles, while some other physiologists, as Gerlach and Schaffner, reading the facts in the reverse way, consider the spleen to be a gland where the red blood particles are formed, as Hewson long ago supposed. These yellow pigment particles are often remarkably abundant, particularly in the horse ; and their origin from the disintegration of blood discs is well made out. But according to all recent observers, the metamorphosis takes place mostly in mere heaps of blood corpuscles, and only very rarely in blood discs contained within cells (Remak, Virchow). Many observers, indeed, have failed to detect the blood-holding cells altogether, and in many animals they have not as yet been seen by any one. Mr Gray has observed them particularly in the horse and ass. The disintegration of blood into pigment, whether it takes place without or within cells, therefore, is an admitted fact; and Mr Gray imagines that it may be occasioned by extravasation occurring from the intercellular spaces by which the veins originate; and he coincides with Kölliker, in so far as to regard these changes as "peculiar to the spleen in the higher vertebrata, and as consequently representing a part of the function which the organ normally performs." Mr Gray, however, and other observers, have not found this disintegration of the blood to occur in greater amount during the digestive process, as Landes and Kölliker's experiments attempted to show, than at any otherperiod, and in certain animals, this disintegration is not to be observed at all. In fact, it is difficult, with our present notions, to believe extravasation of blood to be a normal process anywhere, or to imagine destruction of the blood as the function of any gland. The evidence is not conclusive, that these "changes may not be either post-mortem or pathological," or if they be admitted to be normal, it appears to us a process whose purpose is at present utterly unknown. Besides, similar changes have been observed in other parts, especially in pathological extravasations in the brain. We doubt, therefore, if physiologists are prepared fully to admit the disintegration of blood particles as an important and normal function of the spleen.[1]

The chemical analyses of the spleen pulp, a subject to which Scherer's researches have lent much interest, were performed by Mr Gray on a considerable scale, and with the utmost care. Although obtained by the same method, his results are entirely at variance with those of the German chemist. He failed entirely to find either uric acid or hypoxanthin (a body discovered by Scherer), and, with regard to the other acids, the formic, acetic, butyric, valerianic, &c., it appears very doubtful whether they exist preformed in the spleen, or are formed by combinations occurring during the process of analysis. It is considered highly probable, however, that the lactic and phosphoric acids, which are found in considerable amount, really exist in that form in the spleen pulp, and give it its peculiar acid reaction. The chemistry of the pulp, therefore, requires some further elucidation. Is it not possible that the action of these acids may have some effect in producing those disintegrating changes in the blood corpuscles?

---

[1] We beg to refer, on this subject, to two able papers, by Dr. J. Scott Sanderson, in the *Monthly Medical Journal* for 1851, where a series of experiments and observations on the cells containing blood corpuscles, and the disintegration of the blood discs, both in the spleen and in other organs, is recorded. We do not find these papers mentioned in Mr Gray's bibliography.

The Malpighian bodies, or "splenic corpuscles," as Mr Gray names them (not very happily we think), are the last part of the structural description which we need to review. Their universal presence in mammalia, and their variable size and number, are facts now become notorious, since the attention of anatomists has been more carefully directed to them. They are soft, round or oval, white bodies, composed of a delicate capsule, which contains fluid and corpuscles. Mr Gray describes the capsule as composed of an outer fibrous, and inner homogeneous membrane, similar to the basement membrane of glands. This account agrees so far with that of Sanders, and is opposed to the late observations of Wharton Jones and Huxley, who deny the existence of this capsule altogether, except in the sheep. The Malpighian bodies are in close relation with the arterial sheaths, and with a capillary and venous plexus (see fig. 42), to which we have already alluded. The minute arterial branches to which these bodies are attached, are described "as proceeding over their outer surface; others more rarely pass through their centre or part of their circumference;" this latter observation coinciding with those recently made by Sanders and Huxley, by whom this relation has been found greatly more frequent than would appear from Mr Gray's account. Mr Gray does not notice capillaries in the interior of the Malpighian bodies, as Kölliker and others have observed. The contents of these bodies are granules, plasma, and nuclei, more rarely nucleated cells, —all of an albuminous nature, and showing evidence of cell-growth. The nucleated cells have appeared in what we have seen to be much more numerous than Mr Gray describes; and we do not think the evidence of decay in the nuclear particles here to be so evident as in the pulp.

A valuable contribution to the physiology of the spleen on a point long cursorily noticed will be found in Mr Gray's experiments on the effect of diet and nutrition on the size and development of the Malpighian bodies. We refer our readers to these in detail; they almost demonstrate a nutritive function in the spleen. The general results are: that these bodies attain their maximum of size in animals in high condition, and are scarcely observable in starved animals: they are largest during and especially towards the completion of the process of digestion, appearing to store up certain albuminous ingredients in their interior; and they are greatest also in animals fed on an appropriate mixed, or an albuminous diet. The introduction of fluids into the system also increases their size and the fluidity of their contents.

Mr Gray is convinced of the glandular nature of these bodies, and of their analogy with the vesicles of other ductless glands.

To conclude this part, we have only to observe, that neither the lymphatics nor the lymph present any peculiarity which would entitle these vessels to be considered excretory ducts of the spleen; they are not connected with the Malpighian bodies, as certain very recent observers have supposed. The excellent dissections of the nerves do not present us with any topic of sufficient general interest for remark.

Part IV. *Comparative Anatomy.*—This subject has not even yet been satisfactorily exhausted, but our limits will only allow a notice of the results of Mr Gray's numerous dissections. They are:—1. The presence of a spleen in all vertebrata. 2. Its small size, and the absence of the Malpighian bodies in fishes and reptiles; its larger size, and the presence of these bodies in mammals and birds. 3. Changes of disintegrating blood corpuscles in the pulp of nearly all classes. 4. The large number of white corpuscles in the splenic venous blood. 5. The presence of peculiar coloured masses in the spleens of certain fishes. These masses or vesicles are sometimes attached to the bloodvessels, and sometimes are independent of them; they appear somewhat analogous to the Malpighian bodies in other animals, and they were considered by Kölliker to contain blood corpuscles, for the purpose probably of disintegration; but Mr Gray doubts the accuracy of this opinion, and states that they contain "a peculiar colouring matter formed as a secretion in distinct nucleated cells." 6. There appears evidence of the spleen acting as a diverticulum for blood, especially in diving animals (with the exception, however, of the cetacea). 7. The general conclusion is, that the spleen's function is not for "specific but for general purposes, serving to regulate, under many varied and opposite conditions, the quantity and also the quality of the blood."

Part V. *Physiology.*—We have already anticipated this part, which is, in fact, a summary of the results of the preceding observations. The spleen, in its influence on the quantity and quality of the blood, may be considered to act in four ways:—1. It acts as a reservoir or diverticulum for blood, both when its quantity is increased in the whole body, or only in the portal system. In most animals this use must, it appears to us, be of secondary importance, since the volume to which the spleen may be distended without injury cannot lodge any great quantity of blood. 2. The spleen alters the quality of the blood in a very marked manner, diminishing the red blood globules, increasing the albumen, the white corpuscles, the fibrine, and the iron,—facts which have been clearly and fully proved by Mr Gray's analyses. 3. The spleen seems to serve as a sinking fund for nutrition by storing up the albuminous compounds, as the varying size of the Malpighian bodies in different states of nutrition sufficiently indicates. 4. It accomplishes the disintegration of the red blood discs,—but on the importance of this last function our doubts have been already expressed.

With regard to those conclusions, which obviously depend for their proof on the observations recorded in the rest of the work, it would be out of place here to enter into any full discussion. The prominent and important point appears to be, that the spleen is connected with the process of nutrition, and with the constitution of the blood,—a fact not valuable in physiology only, but highly illustrative of pathology, where we find diseased states of the blood accompanied by lesions of the spleen (as in the softened spleen in fevers, tubercles in the spleen, in phthisis, and the waxy spleen in connection with the waxy liver and kidney); and, on the other hand, morbid condition of the blood produced apparently by disease of the spleen, as has been shown in so interesting a manner in the white cell blood (Leukæmia or Leucocythemia). The connection of the spleen with the blood and with nutrition is, we think, the leading idea in the inquiry, and that which the progress of research will further and more amply illustrate.

The spleen, however, has no absolutely essential or exclusive part to play in nutrition; for if any one so overrated its importance as to assert this, the curious fact of the excision of the spleen from animals, which have been none the worse, nay have been in better condition after its removal, would refute any such exaggeration. If this experiment throws no positive light upon its function, it negatives any false idea of its importance. At the same time, we should not infer from these experiments that the spleen has little or no use, but only that its function is not exclusive or always necessary; and the absence of notable effect from its removal merely opens up the difficult question of the vicarious action of parts in the body—a subject full of interest, but little known.

We have only a few words to say on the accessories of the work before us. The *getting up* of it is excellent; the printing clear and good; the plates generally true to nature and illustrative. A copious index of contents is prefixed, and a bibliography added to the book. The latter is full and copious; but shows a great want of arrangement; the order, apparently intended to be chronological, is very frequently violated; for example, we find Clopton Havers, London, 1691, next to Dr Otto Funke, Heidelberg, 1851!

In the preceding pages we have given a tolerably full analytical sketch of Mr Gray's treatise; we have extracted what appeared to be important or well ascertained; we have endeavoured, while indicating its difficulty and deficiencies, to give an idea of the extent and value of the knowledge which has been acquired on the subject. To Mr Gray the profession is indebted for having brought together, and consolidated as it were into one structure, the various researches which had been made by different classes of observers, and for having cleared up the whole inquiry by his careful descriptions and especially by his excellent physiological experiments and chemical analyses. Mr Gray's work is written in a clear and precise style, and will have a great effect in diffusing correct ideas on the subject of the spleen; we acknowledge with pleasure the great information we have derived from its perusal, and we add our tribute of admiration and of praise for his talent and perseverance in the successful prosecution of a difficult, complicated, and laborious investigation.

ART. II.—*Practical Observations on Aural Surgery, and the Nature and Treatment of Diseases of the Ear.* By WILLIAM R. WILDE, F.R.C.S.I., Surgeon to St Mark's Hospital, Honorary Member of the Royal Medical Society of Stockholm, &c., &c. London, 1853. 8vo, pp. 506.

The want of a good practical work upon diseases of the ear, and their treatment, has long been felt, and this want has only been rendered more apparent by the productions of Curtis, and others of a like stamp. We hailed the publication of the work before us with great pleasure, as we were well aware that its author, a man of intelligence and independent spirit, was from his opportunities well qualified for the task; but we think Mr Wilde need not have been at so great pains to inform us as to the good reasons why he is fitted to write a book "for the information of practitioners and students in medicine, upon the history, symptoms, causes, mode of treatment, and results of the most frequent and remarkable diseases of the ear."

The ignorance of diseases of the ear, even at the present day, both among practitioners and students, in which they are content to remain, honestly acknowledging that they know nothing about them, is a very great evil, bringing discredit upon the profession, and, to say the least, deafness upon their patients. It has conduced to the support of empiricism in the profession, quackery out of it, and indifference and prejudice among the public at large. It is at first sight difficult to explain this degraded position of a most interesting branch of surgery, especially when we look at the great strides towards perfection which have been made of late years in ophthalmic surgery and medicine. Mr Wilde gives us the following sarcastic, but not untrue, exposition of the melancholy fact:—

> "The difficulties which beset the student in acquiring a knowledge of the anatomy of the ear—owing to the exceeding minuteness of the organ itself, the great difficulty of dissecting it, from its depth, the complexity of its structure, and the small hard bone in which it is placed, as well as the number of crabbed names attached to its different parts, all of which have made it a sort of anatomical crux, which no one wishes to remember longer than the day after he has passed his examination—have conduced not a little to strengthen the belief in the doctrine promulgated by his instructors, that little or nothing could be done to reach the diseases of so delicate and intricate an organ."

The very name of specialist is an abomination in the eyes and ears of the profession, and diseases of the ear have long been ceded as the special domain of quackery and imposture —another strong reason to make every one stand aloof; but let us remember that in this position they will remain so long as the profession pass them by, and do not lend a helping hand to place aural pathology and surgery in a position from which they shall be able, with a sure footing, to resist the attacks of all unlicensed and licensed vagabonds. Mr Wilde has done this to a great extent, and for this our best thanks are due to him.

Before giving our readers a brief analysis of the work before us, we think it right to quote the author's own explanation of its nature.

> "Detached portions of it," says he, (Clinical Lectures and Cases observed at St Mark's Hospital), "have already appeared in the periodicals of this country; all these, together with much additional information gleaned since their publication, are embodied in this book,

which does not profess to be a complete system of aural surgery, giving a full description of all the diseases of the ear which have been recorded by authors; but is intended to supply the reader with a practical treatise on the most frequent and urgent affections of the organ of hearing, and those that I myself am best acquainted with. It may, therefore, be regarded *somewhat in the light of a monograph, a form of publication peculiar to this school*, and one generally containing more useful and practical information than either a large systematic work or a manual."

The italics are our own; the meaning of the passage so marked, we do not profess to understand; a monograph the book is in no respect; and the assumption of that form of publication as peculiar to the Dublin school, is an assertion which we feel confident is peculiar to Mr Wilde. If we might state our impression of the book thus early, we would say, that it consists of a great deal that is most excellent, some that is unnecessary, interspersed with some highly appropriate abuse of quackery, and here and there as we think some very irrelevant observations; but we can readily pass over all this, in consideration of the great merits of matter and style which make this book both readable and instructive to an eminent degree. Unlike many writers of the present day, Mr Wilde ascribes to his readers an average amount of intelligence, and consequently does not tease and torment them by putting the same facts and objects before them in fifty different forms.

The book consists of eight chapters, each of which, if space permit, we propose briefly to analyse.

The first chapter consists of introductory remarks on the subject of aural surgery generally, into which we have already dipped, and really think them, with the exception of three or four paragraphs, of no practical importance, and little consonant with the plan he has given us of the work.

The latter part of the chapter, consisting of 44 pages, is devoted to a very excellent history of aural surgery and review of the works upon the subject. The chief novelty consists in the amusing review he gives of the later writers on aural surgery, which he carries down even to contemporary authors, on some of whom he is exceedingly satirical. The early part of his history has necessarily a great resemblance, in order of succession and in the facts detailed, to the same part of the work of Kramer. We are fully convinced of the accuracy of both narratives, and we have therefore determined to give a short sketch of the early history as helping to elucidate the progress which aural surgery has since made.

The inspection of the external auditory canal, and the acknowledgment of specific or independent forms of aural disease, seems to have originated as early as the time of Celsus; the notices quoted from Hippocrates, as believed by many to indicate his knowledge of diseases of the ear, as of what not else! were simply referred to, as symptomatic of other affections. The treatment adopted by Celsus was as then fashionable complicated in its measures, as it must have been unfruitful in good results; and Galen, though he seems to have been better acquainted with the inflammatory affections of the organ of hearing, did more harm than good by his writings on this subject, for he recommended all manner of nostrums, gathered from every source, which unfortunately were perpetuated with many additions to the end of the fifteenth century.[1]

In the works of the Arabians, and in some of the later Greek physicians, several notices of diseases of the ear and their treatment occur; and in speaking of these Mr Wilde mentions a curious and interesting fact, "That the works of Hali Abbas, Mesue, and Dioscorides, were translated into Gaelic, by several distinguished Irish physicians, *from* the beginning of the fourteenth to the end of the sixteenth century."

The advance made in the knowledge of the anatomy of the ear, about the end of the fifteenth century, added a meaningless nomenclature to this sufficiently intricate subject; and while it opened up the field of inquiry into aural pathology, it seems to have availed but little in forwarding the diagnosis and treatment of diseases of the ear. Even in 1584 so little had been advanced in this direction, that we find the first special work upon the ear to consist of a mere compilation from the works of the Greeks, Romans, and Arabians, with the addition of remedies worthy of the age. About the same time, or rather earlier, the attention of philanthropists began to be turned to the melancholy condition of deaf-mutes, and attempts were made to reclaim them from the degraded position which previously had been assigned them. It was Jerome Cardan of Pavia who first urged the fact that the deaf-mute could be taught to read and write, and claimed an interest in his behalf. We regret that we must pass over the interesting history of this department of the subject, and refer the reader to Mr Wilde's most excellent appendix upon deaf-dumbness for full details. Fabricius Hildanus seems to have been the first to employ a speculum auris, and thus to have struck upon a new means of accurately investigating diseases of the ear.

The first work upon our subject, which appeared in this country, was the translation of the work of Duverney, published at Paris in 1683. This work Mr Wilde very justly shields from the odium which has been cast upon it with such unsparing hand by Kramer and others, and insists that though M. Duverney was the means of perpetuating some error, still we owe much to him, as he first fully introduced the subject to practitioners in this country, and we cannot join in the censure of Kramer and say, "Duverney the anatomist is always to be admired, but never the physician."

---

[1] See "Gabelhover's Boocke of Physicke."

The next point of interest in this somewhat disjointed history is the proposal to introduce instruments into the Eustachian tube. M. Guyot, a postmaster at Versailles, seems to have made a communication on this subject to the French Academy in 1724. That he proposed this means of investigation, we can see no reason to doubt, but that he ever actually employed his method of introduction of an instrument by the mouth is more than improbable. Whether Cleland, a surgeon in the English army, was acquainted with this proposal is a matter of no importance, as to him is undoubtedly due the credit of the recommendation to introduce a silver instrument through the nostril into the Eustachian tube, so as to investigate the condition of the middle ear by throwing into it a stream of water. This valuable addition to the means of diagnosis, in use up to this period, was eagerly adopted by numbers of surgeons both in this country and abroad, who either overlooked, or did not think it necessary to acknowledge, Mr Cleland's claim to the invention.

Mr Wilde, passing over a number of writers, French and German, of little importance, as being mere copyists of what others had written before them, mentions next a work published in this city (with the existence of which we were previously unacquainted), by a Dr Peter Degravers, and who, though of little celebrity as a writer on diseases of the ear, curiously enough speaks of having incised the membrana tympani; in fact, performed the operation which now-a-days Sir A. Cooper usually gets the credit of having first proposed.

The end of the eighteenth century was given up to the ingenuity and effrontery of quacks, who then, as now, found it an open and most fruitful source of revenue; and Mr Wilde, in the bitterness of his spirit, in which we quite sympathise, finds it necessary to occupy two whole pages in a clever tirade against the quacks and quackery of the present day; then, quieting down after this little sally, returns to his history as though nothing had intervened to disturb his equanimity. He details at some length the history of the investigation into the effects of perforation of the membrana tympani upon the lower animals, and the effect of destruction of it as found in man, upon the function of hearing, and of Sir A. Cooper's proposal to perforate the membrane in cases of deafness dependant upon obstruction of the Eustachian tube, accompanied with thickening and consequent want of vibration of the membrana tympani.

We cannot follow Mr Wilde any further in his history, as we feel we should be reviewing works as open to our readers as to ourselves; and we would say in passing, that we are very doubtful of the propriety of one author criticising, as a matter of history, the works of others who are either his contemporaries, or who have but lately passed from the field of their labours. If he considers them wrong in any point, the text of his work—not an historical review—is the place to combat their opinions. Such a proceeding is very similar to *posting* a man, and cannot fail to excite a rancorous feeling not to be easily assuaged.

Chapter Second is devoted to a consideration of the means of diagnosis, and the application of remedies in diseases of the ear, prefaced by an explanation of the plan which Mr Wilde proposes to pursue in the chapters which follow. This arrangement is, to divide the subject into three great heads, diseases of the external, middle, and internal ear, attaching to each section a brief description of the anatomy of the part to be considered, followed by the pathology, symptomatology, and treatment, of those diseases with which he is best acquainted. His more particular arrangement of diseases under these three heads is detailed in the Third Chapter, of which we shall afterwards have to speak.

We had hoped that all introductory remarks were at an end; but instead of immediately entering upon his subject, Mr Wilde refers to papers formerly published by him on diseases of the ear, then indulges in some observations upon quackery, then in a description of the manner in which he teaches his subject clinically, and further, how he thinks himself thoroughly worthy of credence from the very accurate way in which he has noted all his cases. These observations in themselves, and in the right place, are all very excellent; but we must really once for all object to his swelling the pages of his book with matter which, while it is of no practical importance, is a mere repetition of what he has said before. But let us to the subject itself. In order to arrive at a just appreciation of aural disease, the same method of inquiry is to be employed as in our investigation of disease elsewhere, viz.:—1st, By directing our attention to the history of the case; 2d, To the physical; 3d, To the rational symptoms. It is to the second of these, and the means to be had recourse to in observing them, that Mr Wilde gives particular attention. He, however, has some excellent observations on the frequency with which "tinnitus aurium" may be traced to local causes, to which we must beg particular attention.

> "From its (tinnitus) not being present where the membrana tympani has been in whole or in part removed, and that hearing is not quite destroyed, and from its cessation after artificial perforation on the one hand; and, as we have all experienced its occasional presence in influenza, sore throat, or simple catarrh, until by pressing air through the Eustachian tube into the tympanal cavity, we instantaneously get rid of it, on the other; I am firmly of opinion that one cause of tinnitus is non-vibratability of the membrana tympani; . . . and possibly a similar state of the fenestra rotunda, may be another."

And this, too, by way of warning.

> "Physicians are too much in the habit of treating patients for this symptom alone, without having made a careful examination of the ears. I have seen persons who had been subjected to a severe course of treatment, consisting of cupping and leeching, low diet, blistering the nape of the neck, purgation, and salivation, or the administration of blue pill, and James's powder for noises in the ears and head, until it was eventually discovered that all these symptoms proceeded from a comparatively thin cake of hardened wax pressing upon the drum."

The means employed in making a physical examination of the ear, consist; 1st, Of simple inspection of the external parts of the organ of hearing, including the mastoid, preauricular region, and external ear; 2d, Inspection of the meatus and membrana tympani by the speculum; 3d, Auscultation, to determine the condition of the middle ear; and percussion (useful in a very few cases) over the mastoid cells, to determine whether or not they contain air; and lastly, Catheterism of the Eustachian tube, to ascertain the condition of that canal and the middle ear.

We have not time to consider and describe such an examination in detail, and, indeed, we think it unnecessary, as with the exception of percussion of the mastoid cells, the other modes of investigation have been known and employed for many years.

Mr Wilde has most justly the credit of introducing to the notice of the profession the conical tubular specula which bear his name, but which he attributes to Dr Gruber of Vienna. His instruments, which from experience we can most strongly recommend, as those through which a satisfactory view of the membrana tympani can most easily be obtained, he thus describes :—

"These instruments consist of conical silver tubes of different calibers. . . Each tube or speculum being an inch and a half long, $\frac{5}{8}$ths of an inch wide at the greater aperture, and varying from two to four lines in the clear at the smaller extremity. Larger and smaller sizes will be occasionally necessary; but for the greater convenience of carrying in the pocket, I generally order a set of three, formed so as to fit into one another. They should be made as light as possible, highly polished, both inside and out, with a stout rim or burr round the larger margin, and the smaller aperture well rounded off, so as not to irritate the ear in entering."

Mr Wilde very naturally prefers his own form of specula to any other, but he is by no means prejudiced in their favour.

"I believe," says he, "it is a well managed light (natural, not artificial), a practised eye, and delicate manipulation, that, more than any peculiar form of instrument, assists the practitioner effectually to explore the external auditory passage or the middle ear, and which enables him to make an accurate diagnosis."

Mr Wilde discountenances the employment of lamps or artificial light of any kind, preferring a bright day-light when it can be had; no doubt, there are days in this northern climate of ours, when employment of artificial light is necessary; and in such a case nothing is better than the light of a good clear gas flame, concentrated by means of the ordinary condensing lenses, employed in working with the dissecting or compound microscope, fitted to a sliding rod, with a universal joint. This was the apparatus employed long ago by Cleland, and is in every respect more useful and less cumbrous than the costly apparatus consisting of condensers and reflectors, introduced of late years by different aurists and mechanicians.

If the patient is unable to inflate the cavity of the tympanum through the Eustachian tube, and there is any reason to suspect obstruction of that canal from the accumulation of fluid, the Eustachian catheter is under certain circumstances a most invaluable adjunct to assist us in diagnosis; the utility of this instrument is now very generally admitted, though, for some time, a very strong prejudice existed against it, from the indiscriminate use made of it by some practitioners. There is now, we think, little difference of opinion as to the propriety of alone employing an inflexible silver instrument, and that its introduction should be effected by passing it along the floor of the nostril. The form of instrument generally employed, and that which Mr Wilde recommends, is the same as that figured by Kramer. He also employs the frontal fillet to retain it in position, and the air-press of the ordinary construction, by means of which a stream of air is forced through the catheter along the obstructed Eustachian tube. M. Gairal, as a substitute for the press in common use, long ago recommended the employment of an elastic air-tight bag, by means of which he injected the stream of air. From the force required to effect this, the beak of the catheter was almost sure to become displaced; it seems to us, however, that an elastic caoutchouc bag, such as those lately introduced by Messrs Varnont and Galant of Paris,[1] might easily be adapted to answer this purpose; and in this way the employment of the frontal fillet might be advantageously dispensed with, while the requisite amount of force would be attained.

Mr Wilde's opinion, in regard to the instrumental examination of the Eustachian tube, so entirely coincides with our own, that we make the following quotation:—

> "We should bear this rule ever in remembrance before exploring the middle ear; it is one which Sir A. Cooper observed to me many years ago, and I have ever since acted upon it. Whenever the patient is himself able to inflate the tympanum, never use any artificial means to do so; it is unnecessary, and may be injurious. Let me to this aphorism add another, which surgeons would do well to remember. Where there is reason to believe that the cavity of the drum is inflamed, carefully abstain from all poking with catheters, or any attempt to introduce foreign substances into that delicately organized portion of the animal machine."

If the reader is anxious for information, or curious in the matter of aural instruments and their use, he will find a very full account of them in Mr Wilde's work, and a description of several very ingenious inventions of his own well suited to the purposes for which they are intended.

Chapter Third is occupied with the statistics and the nosology of diseases of the ear.

One point aimed at by Mr Wilde, in bringing forward his statistics, is to prove that Nervous Deafness is not so common as he formerly, and most people generally, believe; and that, on the other hand, inflammatory affections of the ear of one type or another form the great mass of cases. It is remarkable that Kramer, in his work on the Ear, published in 1845, in his synopsis of 2000 cases, has recorded 1028, upwards of one-half, as cases of nervous deafness. Mr Wilde handles this statistical statement pretty severely, but not unjustly. He remarks, that on first commencing the study of ear diseases, he was under the belief, still maintained by

---

[1] These bags, which are about the size of a child's glove, can be inflated by the mouth to contain fully a cubic foot of air, are exceedingly elastic; and on the removal of the retaining pressure by a stop-cock or otherwise, contract with great rapidity to their original size.

Kramer and others, that where he had no positive evidence of disease, either in the meatus, membrana tympani, or cavity of the tympanum, the case was one referable to the class of nervous deafness; and during that time he noted 85 cases out of 706. Since then, he has been struck with the fact, that cases of deafness, with *perfectly* healthy membrana tympani, were comparatively few; in fact, that he met with only 18 cases in 1679; and, at the same time, that the cases of thickening and opacity of the membrana tympani, taken no notice of in the first-mentioned period, were as many as 219 in the second; and that the number of cases of chronic inflammation of the membrane had increased from 82 to 314. These facts, placed alongside Kramer's description of the appearances noted by him as characteristic in nervous deafness, make it evident that he has been in the habit of overlooking these most important affections, and consigning cases presenting such objective symptoms to the hopeless category of nervous deafness, evidently mistaking the cause for the effect. These are his words, "In nervous deafness, I have almost always found the membrana tympani white like paper, and opaque."

Such results are very interesting, and worthy of implicit confidence, as they are based upon the recorded observations of the same person, whose accuracy cannot for a moment be doubted, as he gives us all the data in the form of a very complete tabular description of the cases, compiled from notes taken when the patient was examined. His conclusions, too, entirely confirm the results of Mr Toynbee's dissections, recorded in the Medico-Chirurgical Transactions for 1849.

Classification of disease is a matter of very great difficulty, as adherence to any one system forms a great impediment in writing, reading, or consulting a work. It necessarily implies constant repetition; and, at the same time, separates affections which are closely connected, from being either described together or following in a natural sequence. It gives a great stiffness to the style of a book, and requires a very copious index to make it available. Mr Wilde, by a happy combination of a general anatomical with a pathological and nosological arrangement, avoids this difficulty. He treats,—*1st*, The diseases of the auricle and external ear, with the mastoid and preauricular regions; *2d*, Those of the external meatus and auditory canal; *3d*, The affections of the membrana tympani; *4th*, Those of the middle ear; *5th*, The morbid conditions of the Eustachian tube; *6th*, Diseases of the internal ear, so far as anything is known about them. The further divisions are nosological; *7th*, Otorrhœa; and *8th*, Otalgia and Tinnitus aurium. In addition to the separate chapter on Otorrhœa, Mr Wilde treats of it under the various affections which produce a discharge from the meatus. This is therefore distributed throughout the work, according to the original locality or structure affected.

Chapter Fourth is devoted to a consideration of the first of these divisions. *Diseases of the Auricle, Mastoid Region, and External Meatus:*—Congenital malformations of the auricle are chiefly those of its parts, especially of the lobe, depending upon a partial or complete arrest of development; the contrary condition of supernumerary parts or plurality of auricles is very uncommon; the comparatively late period in fœtal life (the sixth month), at which the development of the ear is completed, may serve to explain this.

The diversity which exists in the length, caliber, and curvature of the external meatus, is much greater than one would *à priori* imagine.

Mr Wilde says of this,

> "Either these peculiarities occur much more frequently than is supposed, or in some way which is at present unaccountable; persons possessing such peculiarities are more subject to aural disease than the rest of the community."

Wounds of the auricle are comparatively rare, in this country at least; when they do occur they are almost always lacerated or contused, and, putting aside sabre wounds, are generally produced by blows or falls, sometimes from other causes. We have seen an ear almost completely amputated by the wheel of a railway carriage, and one partially removed by the bite of an infuriated woman. Such wounds have no tendency to primary union, and the employment of sutures here as in the scalp, is liable to the objection that erythema is apt to supervene; pads of lint, secured by a bandage, retain them sufficiently well in apposition, and as the apparatus is always at hand, it is to be preferred to the cork pad generally recommended. It is a matter of surprise that enlargement of the lobe of the ear does not more frequently follow the barbarous practice of wearing ear-rings. Such results, however, do sometimes occur. Mr Wilde mentions a case in which a small fibrous tumour followed the operation of piercing the lobes of the ears. Similar tumours have been described by Paget, Pruch, and Venzetta.[1] Mr Paget believes these growths to be developments from the fibrous tissue of the cicatrix. The auricle has in some cases been the seat of vascular nævus, involving it either wholly or in part, and in most cases the vascular supply has chiefly been derived from the posterior auricular artery. Mr Wilde describes a case of cystic tumour of the auricle seated between the helix and the concha, which he treated successfully by incision, allowing the sac to suppurate and contract by granulation.

---

[1] See Paget's Surgical Pathology, vol. ii. p. 149.

The auricle has sometimes been the seat of epithelial cancer, and has in those cases required extirpation. Mr Wilde alludes to such an affection in a very few words, the case which he mentions commenced beneath the zygoma and was not one suited for interference.

The auricle suffers from inflammation of various kinds and types affecting its several textures, and it seems particularly liable to such from the number of sebacic follicles with which this part is supplied, together with the very dense character of the subintegumentary cellular tissue. Under the head of inflammation of the auricle Mr Wilde mentions a tumour of the auricle common among the insane as a disease with which he is not acquainted. The affection is one well known in asylums, and though certainly, as Mr Wilde says, local injury is generally the exciting cause, that alone will not account for it. When the tumour is cut into it is found to consist of an irregularly loculated cyst, filled in different cases with partially coagulated blood, serum, or a viscid glairy fluid. The seat of this extravasation or effusion is very frequently between the perichondrium and cartilage, but it may take place into any of the tissues of the auricle. Among the many theoretical causes which have been assigned to account for its production, we may mention, simple injury, local congestion, inflammation, a diseased condition of the vessels of the part and a diseased condition of the blood.[1] The subject is still open for investigation, and meanwhile we must content ourselves with three facts which have been observed in regard to this affection; 1*st*, The tumour consists either of pure blood or some of its elements. 2*d*, That it always attends a cachectic state of the constitution. 3*d*, That where a tendency to this lesion exists, as a general rule, the patient never survives long. It requires no treatment, but when left to itself becomes gradually absorbed.

Erythema and erysipelas either commencing in or extending to the auricle from the head and face, is a painful and very annoying affection, as it prevents the patient from resting upon that side of the head. It is to be regarded in respect to the ear, chiefly from the tendency to extension of the action along the meatus, involving the membrana tympani, causing exudation of lymph between its layers and permanent thickening, or more commonly suppuration of the meatus about its middle at the seat of the circle of ceruminous glands.

---

[1] See a Paper by Dr Fischer in June and August Nos. of Bucknill's Asylum Journal.

Of the various cutaneous eruptions affecting the auricle, and the specific forms of inflammation of which it is liable to become the seat, we have no time to speak, but pass on to affections of the external meatus; of these we alone see occasion to speak of exostosis of its osseous portion, a subject which has excited some interest here from two cases of pedunculated exostosis of the ear having occurred lately in the practice of Mr Syme. The forms of exostosis to which Mr Wilde alludes are not the pedunculated, and seem to partake more of the character of nodes of the osseous part of the canal. Mr Wilde represents this affection as a very common one, if so it is not noticed by any other author upon the subject.

Chapter Fifth is devoted to a consideration of the diseases of the membrana tympani, which Mr Wilde prefaces with a very accurate description of that membrane in its natural healthy state. The works on anatomy in the hands of students contain nothing but a most meagre account of its position and structure; and as a great diversity of opinion still exists among authors with respect to its appearance, such a detailed account was far from unnecessary. When viewed through the speculum, the whole of the membrane cannot be seen at once, as the second curve of the auditory canal prevents both the anterior and posterior portions from being in the direct line of the instrument. A little manipulation is requisite before a thorough view can be obtained; and any one just commencing the investigation of diseases of the ear will do well to accustom himself to its examination by the speculum, as there are few so dexterous as to accomplish this the first time they attempt it, without giving unnecessary pain to their patient. To obtain a satisfactory view of the membrana tympani, it should only be examined by a bright day-light; the speculum should be introduced to only one-half the depth of the meatus, and a very little practice will very soon teach the further manipulation requisite more easily than it can be expressed in words. The position of the membrana tympani is oblique, its outer surface looking downwards, outwards, and a little backwards; its appearance is on the whole slightly concave, and the handle of the malleus is a prominent white line from above downwards to about the centre of the membrane, when its extremity becomes a little larger. The appearance of the membrane is not uniform; the parts anterior, inferior, and posterior to the handle are most transparent, the first of these more so; and that portion of the membrane is also most vibratile, slightly *convex* outwards, and most frequently the seat of perforations. Superiorly, above the upper half of the manubrium of the malleus, the membrane becomes thicker and whiter; a similar thickening is naturally present around the lower part of its circumference, which, as Mr Wilde says, "in disease, and in some old persons, resembles the arcus senilis, except that in the cornea there is always a narrow clear space between the opacity and the sclerotic." The general surface of the membrane is highly polished, and in general appearance it may be said to resemble goldbeater's skin rather than parchment, the substance to which it is most generally likened. The arrangement of its vessels is a matter of importance, as bearing upon the appearance of the membrane when inflamed. When the speculum has been used for some little time, moving it about in the ear, especially if this be done at all roughly, or if any tendency to undue vascular action exist, there may be often observed a pinkish appearance along the handle of the malleus, especially at its base, that is, along the course of the tympanic branch of the stylomastoid; and when the membrane is inflamed, branches may be seen proceeding from this as a centre, forming a minute network of inosculation with the tympanic branch of the internal maxillary artery, or "of the deep auricular which is given off at the angle between the temporal and internal maxillary," by which name and fixed origin Mr Wilde is pleased to dignify the first part of this minute and very variable vessel. But without making any further observations upon the anatomy of this membrane, we pass on to consider its injuries. Most of our readers are aware that Kramer denies the possibility of either rupture or alteration of the curve of the membrana tympani, from direct injury or violent succussions produced by loud sounds, &c. In this assertion Kramer was quite wrong, and the different injuries which lead to this result are thus classified by Mr Wilde:—"Injuries of the membrana tympani, independent of those arising from inflammation, or its consequences, may be caused by penetrating instruments or foreign bodies passing through from the external meatus; rupture in whole or in part from external violence, such as fracture of the temporal bone, blows on the side of the head, or falls, &c., and loud sounds, concussions, or the sudden impression of a stream of air from within the tympanum;" and every surgeon, we are sure, can testify that such cases do occur. And Mr Wilde quotes several excellent examples in point, in some of which rupture, in others collapse of the membrane, followed the injury.

The importance of inflammation of the membrana tympani, or myringitis as it is called, has only been recognised of late years. Mr Wilde treats of its inflammation under the following heads:—1*st*, Acute inflammation of the membrana tympani, commencing in the fibrous layer, usually involving the cavity of the tympanum, and generally of a rheumatic character; 2*d*, Subacute inflammation, as a general rule unaccompanied by pain; 3*d*, Syphilitic inflammation; 4*th*, Strumous inflammation of mucous layer, with mucous collection in the cavity of the tympanum; 5*th*, Chronic inflammation; 6*th*, Febrile inflammation accompanying the exanthemata, and generally originating in the tympanum. We are not pleased with this classification, which we think cumbrous in the extreme; and Mr Wilde seems to be aware of this himself, for in discussing the so-called strumous myringitis, he acknowledges it to be a mere part of a general inflammation of the mucous surface with engorgement of the middle ear. Syphilitic myringitis is certainly not worthy of the very prominent position he gives it in the classification at the beginning of this Chapter, and sinks it more appropriately, in the text and nosological table, into a subordinate position, under the general head of subacute myringitis. In another respect, however, this affection is worthy of a prominent place in Mr Wilde's work, for to him is certainly due the credit of pointing it out to be not a result of the abuse of mercury, but a true secondary symptom. Passing over the subjective symptoms which attend and attract our attention to these various forms of inflammation, we proceed to describe the objective, as observed by means of the speculum. The membrane, at the commencement of an acute attack, becomes dull, and loses its diaphanous appearance; it becomes congested, the vascularity commencing at the upper part of the membrane, and extending along the handle of the malleus; the white semicircular zone we spoke of, as running round the lower part of the membrane, soon also becomes pink, and this congestion may even terminate in extravasation; the intermediate plexus of vessels becomes distinctly marked, and sometimes the whole surface resembles a minutely injected pulpy surface. The inflammatory action so characterised may be general as we have described it, or localised, in which case abscess between the layers of the membrane sometimes occurs. In the more general form we may find it accompanied with the formation of vesicles, and even at times with pustules; but when this is the case, the local inflammation is usually an accompaniment of a similar action over the rest of the surface, and has a constitutional cause. Exudation of lymph is the common result leading to permanent thickening of the membrane, and serious impairment of its movement, or it may be accompanied by ulceration terminating in perforation. If the inflammation retrogrades, it usually leaves traces of its attack in opacities, and a tendency to congestion of those parts which first became unnaturally vascular. If the middle ear participate in the disease, the constitutional symptoms are more severe; the throat and fauces usually sympathize from the extension of the action along the Eustachian tube; and if perforation of the membrana tympani along with suppuration of the middle ear ensue, the óssicula auditus are frequently discharged, and the internal ear becomes affected. If the result of the inflammatory product is exudation of lymph into the middle ear, permanent immobility or collapse of the membrane of the tympanum must almost of necessity occur from the contraction of the bands of lymph which cross the cavity and surround the ossicles. The local remedies best suited to combat and soothe this affection are—1*st*, Depletion, which is best effected by leeches applied round the meatus; and 2*d*, The application of poultices or fomentations over the external ear, while at the same time purgatives and other medicinal antiphlogistics should be strenuously employed. Mercury is a highly appropriate remedy in certain cases; but we can by no means approve of the very indiscriminate use which Mr Wilde would seem to advise. "I do not hesitate to recommend its use in the early stages of all such affections. A pneumonia, a pericarditis, a peritonitis, an inflammation of a large joint, or a serous cavity, or an iritis, may, it is true, get well by simple depletion, &c.; but will any experienced practitioner of the present day risk such a case without having recourse to mercury?" With all our admiration for the drug when discriminatingly employed, we think such a manner of speaking very ill-advised. The employment of counter-irritants and iodine is found useful, but hardly so useful as elsewhere, in helping to produce absorption of the deposits of lymph. As acute inflammation of the membrana tympani, affects primarily and specially, at least in most cases, its fibrous layers, there is a great tendency to extension of the action to the periosteum of the meatus, middle ear, and mastoid cells, by continuity of texture. If this does occur, and from its frequency it should be anticipated, appropriate measures should be had recourse to, commensurate to the graveness of the symptoms. If there be the slightest redness, or acute tenderness on pressure over the mastoid process or cells, a free incision should be made down to the bone, to prevent the confinement of the products of the inflammation and the consequent risk of necrosis.

As subacute myringitis is attended by no urgent symptoms, beyond increasing deafness and tinnitus aurium, it is of consequence that we should be able to recognise it; for when allowed to progress, it becomes a most intractable ailment, and do what we may, the patient usually suffers for the rest of his life from distressing deafness, accompanied by constant tinnitus aurium. We speak the more strongly upon the subject, as from the comparatively slight appearance of disease the symptoms are either overlooked or attributed to a deficiency of cerumen,—a fatal mistake, as stimulating applications are had recourse to, which only do mischief by exciting increased action. This form of myringitis, like the more acute, is seated in the fibrous layers of the membrane, but differs from it in having no tendency to extension to the neighbouring periosteum. The deposition of lymph goes on by degrees between the fibrous layers, while often the auricular surface retains its polish, and it is only the increased vascularity of the membrane, which has now an appearance like parchment, that points out the true nature of the affection. Patients suffering from it have every now and then exacerbation when any symptoms from which they may already have suffered are increased; and in addition they complain of some pain or aching; ulceration of the membrane is generally the cause of these symptoms, and when it is detected it has usually proceeded the length of perforation. When these increased symptoms subside, the ulcer, if not a very large one, may heal, and a permanent opacity be the result. Should the ulcer be so large that it cannot heal, we have a permanent perforation of the membrane, which, however, does not necessarily imply increased deafness, as in some cases (those particularly where the patient has long suffered from the subacute affection), his hearing is improved, and in all cases he is relieved of that most annoying symptom, tinnitus aurium. The explanation of this improvement is evident; while the thickened membrane remained entire, it impeded the transmission of sound, and, was, as we have already seen, a cause of tinnitus; after the perforation, the sound impinges directly upon the membrane of the fenestrae rotunda and ovalis, and, if these are entire, is transmitted to the internal ear.

Passing over syphilitic myringitis, which presents no special characteristic to deserve attention, and is merely recognised as such from its accompanying other secondary symptoms, we at once proceed to consider strumous myringitis, which has no claim to such a name, but ought rather to be classed as a form of otitis.[1] There are two points which distinguish this affection from those we have been considering,—1*st*, There is evidence, from the commencement, of obstruction of the cavity of the tympanum with fluid; 2*d*, The appearance of the membrana tympani. The first of these objective symptoms is that one which, by most authors, has been considered as characteristic; in fact by them the claim to its being a form of myringitis is denied, and no appearances of the membrana tympani are described.

It is an affection of early life, though cases are described of it existing in patients of the age of 40. It accompanies other general marks of struma, particularly enlarged tonsils, and a weak œdematous condition of the mucous membrane of the upper part of the pharynx, so often associated with enlarged cervical glands. It is not necessarily attended with pain; and both ears are often affected. Patients, for the most part, complain of a sense of fulness and weight in the ear, with a feeling as though a veil were hung between them and the sound; and these symptoms are increased in damp weather. In coughing, sneezing, yawning, or swallowing, there is frequently a crackling noise, after which their hearing is improved for a short time. In this disease, the patient can generally inflate the cavity of the tympanum; but if not, the employment of the Eustachian catheter is necessary, though by no means so frequently as some authors have represented, for the inflation of air affords only temporary relief, and stimulating injections are not found to be productive of much benefit. On examination with the speculum—"The auditory passage is usually dry, but seldom red, the tympanal membrane will be found of a uniform pinkish hue, but without either thickening or opacity, at least in the early stage. This colour, which is somewhat the tint of pink blotting-paper, appears to be seated in the mucous layer, and shines through the other laminæ of the membrane, which still preserve their polish and transparency."—"In some cases, the redness assumes a dark, damask-rose colour, and then we may generally rest satisfied that the entire of the middle ear is engorged." If the case be left to itself, it may fluctuate about, now better now worse, depending much upon the season of the year and the general health. If it become much worse, perforation of the membrane usually takes place, and internal otorrhœa is set up. The enlarged tonsils which frequently accompany the affection have, in the milder cases, been considered as the cause of the deafness, by pressing upon the mouth of the Eustachian tube. Mr Wilde denies this, and assures us that enlarged tonsils in themselves are never the cause of deafness, but a mere index of a similar state of the mucous follicles of the Eustachian tube, producing obstruction of that canal. It may be so in the majority of cases; but we would ask him, how does it happen that the deafness which is a frequent accompaniment of enlarged tonsils is instantly and permanently removed by the excision of the projecting portions of the glands?

---

[1] As it is by all other authors under the names of "Inflammation of the mucous membrane of the middle ear," "Chronic internal catarrh," "Internal catarrhal otitis," "Deafness from engorgement of the internal ear," "Accumulation of mucous matter in the cavity of the tympanum," "Humid catarrhal otitis."

In treating of typhoid and exanthematous inflammation of the membrana tympani, Mr Wilde has a paragraph well worthy of attention:—

> "In this country, the deafness which occurs during the progress of fever, is generally regarded as a favourable symptom; but I believe that that which arises in the commencement is not so favourable. I can only speak as to the results. In a few cases, the membrana tympani was perfectly natural, and the tympanic cavity free; and the patients presenting such, generally had a bad form of fever, and very severe head symptoms. Such cases, I suppose, to be analogous to the amaurosis, which sometimes arises during the progress of fever, and often remains permanently, being a cerebral and not an ocular disease. But, in the great majority of instances, the membrana tympani presented evidences of disease, being generally thick, opaque, and collapsed. I am therefore led to believe that, in many cases, the deafness occurring during the course of a fever is the result of inflammatory action in the ear itself, and not in the brain."

These observations of Mr Wilde are borne out by the dissections of M. Passavant.[1]

The chronic myringitis of Mr Wilde has a more limited signification than the chronic inflammation of the membrana tympani of Kramer and others. If we look into Kramer, we find that he included as chronic, cases which Mr Wilde disposes of as subacute. This chronic form, which we have now to consider, has much the appearance which we might expect to find in the membrana tympani of a patient several months after the subsidence of an attack of acute or subacute myringitis; while, at the same time, there can be no doubt of its

[1] See *Med. Times* for April 19, 1851.

existence as a primary disease. The frequency of its occurrence, the way in which patients disregard it till too late, and the insidiousness of its approach when an idiopathic affection, gives it an urgent claim upon the attention of all practitioners. Its frequency is not overrated by Mr Wilde, who speaks of its occurring in one out of 6 of the cases of ear-disease which come under his observation. He describes the affection as presenting three different phases,—1*st*, As attending upon external otorrhœa; 2*d*, A painless form; 3*d*, A form attended by paroxysms of pain, coming on at intervals, between which the patient is perfectly free from all uneasiness.

> "The appearance of the membrana tympani is too peculiar to be mistaken; it presents a general thickening and opacity, particularly at its lower portion; besides which there is almost invariably a number of spots about the size of pin heads, of greater density than the rest, and of a pearly lustre, studded over the surface of the membrane. In many cases it presents the appearance of crumpled parchment. During the quiescent period we only remark a few straggling vessels carrying red blood spread over the surface of the membrane, and for the most part coursing from above downwards, parallel with the handle of the hammer. From any provocation, however, such as cold, or other exciting causes, the membrane will, in a few hours and often without any increase of pain, become of a uniform dark red colour."

The amount of vascularity and the opacity in this chronic form of inflammation of the membrana tympani are always in an inverse ratio. Mr Wilde illustrates this affection by a hypothetical case, which is, we regret, too long for insertion, as it is clever, and an excellent example of the piquancy with which he can write. The treatment of this chronic form of myringitis is extremely unsatisfactory, if much opacity of the membrane exists. Mercury acts by no means so beneficially here as in the subacute affection. The local application of a solution of nitrate of silver often helps to relieve but seldom effects a restoration of the membrane to its healthy state. We have no confidence in the use of glycerine, which has been so fashionable for a year or two, and fear that the promiscuous use which has been made of it in this, as in other ear diseases, has too often allowed the time to pass when something might have been done to remedy the condition so trifled with.

Kramer denies, and even ridicules, the existence of collapse of the membrana tympani. Now there can be no doubt that this occurs under two distinct forms,—one the result of disease which leaves traces of its presence in thickening and opacity; the other in which the membrane is perfectly healthy, with the exception of its falling inwards toward the cavity of the tympanum. The cause of the former is pretty evident, and is proved, by dissection, to be bands of lymph binding down the membrane towards the tympanum; that of the latter is still a matter for inquiry. The affection has been described by some as an effect of too loud noises, such as the discharge of artillery, a violent clap of thunder, or the like; or to continued noises, such as the rattling of machinery; others again have gone further, and conjectured that the tensor tympani muscle had snapt, or the ossicula had become disarranged. It is at all events certain that the Eustachian tube, in some of the cases, has been found obstructed, and in these inflation of the tympanum is a feasible proceeding.

The operation of perforating the membrana tympani, and the ingenious instruments devised for this operation, are discussed by Mr Wilde at some length. He arrives at the same conclusion with Kramer, that thickening of the membrane, unaccompanied by any other disease of the ear, invariably affords the only true indication for its performance. In speaking of cases of accidental perforation of the membrana tympani, the value of some substitute for the perforated membrane is fully admitted, and its employment is strongly advocated. He appears, however, not to have employed Mr Toynbee's late invention, and seems to doubt its general applicability. What he recommends is a small piece of cotton, of a size suited to the opening, moistened with oil or glycerine, and gently inserted with a probe, as first employed by Mr Yearsley in 1848; Mr Wilde, curiously enough, saw this employed so long as eight years ago; but let him tell his own story. "I became aware of this fact about eight years ago through a lady resident in Clonmel, who discovered it, she told me, by accident. Considering hers an isolated case, and having my attention directed particularly to other subjects at the moment, I thought no more about the matter."

As our space does not admit of a thorough consideration of Mr Wilde's Chapters (vi. and vii.) upon diseases of the middle ear, Eustachian tube, and internal ear, we think it better to pass them over without further comment than to recommend them to the attention of our readers. There is still great room for inquiry on these subjects, for little is known of the pathology of the different parts of the internal ear, and still less of its symptomatology and treatment; and as nervous deafness, that fruitful source of theoretical disquisition, has been in a great measure curtailed of its importance by the more thorough knowledge of disease, in parts within the reach of our inspection, so we may hope before long that the number of such cases will still more diminish as our acquaintance with the internal ear becomes more intimate.

> "A discharge from the ear," says Mr Wilde, "is by far the most frequent aural disease in these countries; but whether owing to the variableness and humidity of our climate, the prevalence of scrofula, or to neglect, it is difficult to say."

In the practice of our profession we have often to encounter prejudices which exist both among the public and many of the profession in regard to checking a discharge from the ear; for it is a common fallacy that any interference with it is fraught with danger.

The affection is one almost entirely confined to the period of childhood, youth, and adolescence; and, if met with at a later period, we are usually informed by patients that they have had it since ever they remember anything. Its frequency, as calculated by Mr Wilde, is about one in every 3½ cases, which is certainly not an over-statement, as, if not actually present, it has at some time been a prominent symptom in all long-continued ear affections.

Otorrhœa is generally described under the heads of external and internal. In the former the discharge depends upon some disease external to the membrana tympani. In the latter it comes through a perforation in that membrane. This distinction is practically a matter of great importance; as in the former case the disease is usually amenable to treatment; in the latter it is far less so, as disease of bone frequently accompanies it. The causes of external otorrhœa are too numerous to detail; they may be included under three heads—1*st*, Inflammatory affections of the soft parts of the external meatus and membrana tympani, terminating in a granulating surface, an altered condition of the skin lining the meatus, perverted secretion from the ceruminous glands, or ulceration of the membrana tympani; 2*d*, Caries or necrosis of the osseous part of meatus; and, 3*d*, Polypi of the meatus, considered a complication of otorrhœa by Mr Wilde. The first and third classes may be said generally to be amenable to treatment; the second less so, as more important parts of the temporal bone are usually similarly affected. The causes of internal otorrhœa may be classed under two heads—1*st*, Inflammatory affections of the soft parts of the middle and external ear, with a puriform discharge through a perforation of the membrana tympani, depending upon an altered state of the mucous membrane of the tympanum and mastoid cells, granulation covering their surface, the so-called polypi of the tympanum, which are in reality only exuberant granulations projecting through the opening in the membrana tympani; 2*d*, Caries or necrosis of the petrous and mastoid portions of the temporal bone.

While Mr Wilde takes no notice of such a division of the subject, he adopts a far more accurate arrangement, by treating individually the special causes of otorrhœa. We had hardly anticipated his doing so; for these causes he has already described in the former part of the work, though not so fully as he does here; and though in this way he gives greater precision to what he says, it necessarily implies a certain amount of repetition. We would merely, therefore, glance at these causes, and glean what we think may prove interesting. The ordinary skin diseases which affect the scalp in children, by spreading to the meatus auditorius, frequently give rise to a discharge from the ears, which may be either serous or puriform, according to circumstances; and, unless the patient have a very strumous habit, such affections readily yield to cleanliness and appropriate treatment.

The impaction of foreign bodies in the meatus often gives rise to very serious symptoms, and usually to a copious discharge from the ear. This naturally occurs most frequently in children among whom putting small pebbles and peas into the ears is a common trick; and as they are very apt to forget all about their having done so, the offending object may be allowed to remain till the complaint of some pain or a discharge attracts the attention of their friends. In all cases of otorrhœa, especially in healthy children, it is well to be on the outlook for such a cause, as nothing short of extraction of the foreign body will relieve the symptoms. The eye end of a probe slightly curved, an aneurism needle, or a scoop made for the purpose, are the best instruments for effecting this end. Any attempt to remove them with forceps is apt to make matters worse, by pushing the pea, or whatever it may be, only further into the canal; and washing out the meatus with a syringe, unless the foreign body be of small size, is sure to fail. It is a curious fact that impaction of cerumen, although at times, from pressure, a cause of opacity of the membrana tympani, never produces a discharge from the ears.

Too forcible syringing of the ear, and sea-bathing, have at times produced otitis, terminating in suppuration, perforation of the membrana tympani, and otorrhœa.

After an attack of erysipelas of the meatus, a complete cast of the cuticle may be thrown off, accompanied by discharge, or a pulpy secreting surface may remain for some time, from which a milky discharge oozes away, containing portions of the cuticle which has separated in scales. If no disease of the tympanum, with perforation of the membrane, exists at the same time, the secretion is not constant; and on examining the meatus with the speculum, we do not find the vascular granulating surface so common in external otorrhœa.

A large proportion of children suffering from measles and scarlatina, are attacked with a purulent discharge from the ears, and this symptom is so constant that it is looked upon as one of the sequelæ of these affections. In measles the discharge almost always depends upon a diseased state of the soft parts of the external meatus. In scarlatina, (we quote from Mr Wilde), "it occurs in three ways, either by direct extension of the inflammation of the skin into the external meatus and membrana tympani, and there producing a muco-purulent discharge, while it desquamates all over the rest of the body; by the diseased condition of the mucous membrane of the throat (and which may be styled scarlatinous), passing up through the Eustachian tube, and producing suppuration of the cavitas tympani and perforation of the external septum; or again, by the abscesses which take place in the neck, and around the meatus, opening into the fibro-cartilaginous portion of that tube, and there inducing and maintaining, even after they themselves have healed, otorrhœa, from the diseased state of this portion of the organ."

Much has been said of gonorrhœa, syphilis, and gout, as causes of otorrhœa; the first of these, we believe, innocent of any such result; and if otitis does follow in the train of gout, as some late writers would wish to prove, it is highly improbable that it will terminate in perforation of the membrana tympani and otorrhœa. In syphilis, thickening of the mucous lining of the tympanum, followed by obstruction of the Eustachian tube and perforation of the membrana tympani, often accompanies the sore throat, which forms a symptom of the secondary and tertiary stages of the disease; otorrhœa, under such circumstances, may be regarded with serious apprehension as to the results, especially when it occurs with other tertiary symptoms, as disease of the petrous portion of the temporal bone almost certainly follows, if the disease in the tympanum proceeds unchecked.

Discharge from one or both ears, or "welling from the ears," as it is called, very commonly comes on in fracture of the base of the skull some days after injury. The source of this clear serosity is still an unsettled point; many different views have been propounded, but there is none of them, to us at least, that is quité satisfactory. We may divide these opinions into two classes:—1*st*, That the fluid is from within the cranium, either serum separated from a clot, formed from the blood effused at the time of the injury, or cerebrospinal fluid from the cavity of the arachnoid. 2*d*, That its source is independent of the membranes of the brain, or interior of the cranium, and may be from the lateral sinus, from the surface of bone, or from the cavity of the labyrinth. Mr Wilde adopts the view of its being cerebrospinal fluid, and quotes at some length from the well known paper of Messrs Chaissaignac and Robert.[1] In attributing it to this source there was always a difficulty started that the dura mater often, indeed generally, was entire along the line of fissure. These gentlemen describe the serous fluid as escaping along the loose arachnoid sheath of the seventh pair, passing some distance into the internal auditory canal, where the dura mater is very thin, and where a very limited laceration would give vent to the fluid. This view of the matter is very ingenious, but it would require the confirmation of several well conducted observations, before it can be considered as conclusive. To one such observation we beg to refer our readers, in the Transactions of the Pathological Society of London.[2] Of the other opinions, these are the only tenable, that it is the liquor Cotunnii, or the liquor sanguinis, from the fractured surface of bone. But returning from this digression, before speaking of the complications of otorrhœa, we would briefly allude to the treatment employed by Mr Wilde.

When the external meatus, without being the seat of granulations of polypi, becomes a mere secreting surface, he recommends, in addition to carefully syringing out the meatus, to paint the pulpy surface carefully over with a solution of 10 grains of nitrate of silver to the ounce of water. As this secreting surface is generally situated in the neighbourhood and about the ring of ceruminous follicles, this mode of application is to be preferred to the common one of dropping the solution into the ear. If the discharge be very copious, astringent injections are useful as auxiliaries. Those of alum, lead, zinc, copper, and tannin, will be found most useful, and may either be dropped into the ear, or employed in a very diluted form as a wash, after the ears have been carefully cleansed with tepid water. Cleanliness is of paramount importance in this as in all forms of discharge from the ears. "Allowing the discharge to accumulate is undoubtedly one of the principal means of perpetuating otorrhœa in any of its forms."

---

[1] Archives Générales for 1845, November and December.

[2] Vol. iii., p. 232; "Fracture of the Temporal Bone, with injury to the Brain, attended with hemorrhage and serous discharge from the ear." The dissection was performed by Mr Prescott Hewitt.

In all cases of otorrhœa, whatever the cause, if any symptoms of inflammatory action are present, leeching and other appropriate measures should be used to subdue it; when the discharge proceeds from the cavity of the tympanum and mastoid cells, great improvement may be expected from the employment of counter-irritation, applied over the mastoid region. From the frequency with which the strumous diathesis accompanies otorrhœa, the employment of dietetic measures, and the use of cod liver oil, should be especially attended to, but at the same time, the employment of these means will not suffice, so long as they alone are had recourse to, for the disease is in all cases a local one, although it may be kept up, and the natural process of cure interfered with, by the impaired state of the constitutional health.

Vascular growths and caries of the temporal bone, are the most frequent accompaniments of otorrhœa. The former or vascular growths are of two kinds,—granulations or fungi, as Mr Wilde calls them, and polypi. Fleshy granulations very often project into the meatus, through the perforation of the membrana tympani, and are liable to be mistaken for granulations covering the surface of the membrane. These fungi have by some been mistaken for polypi, which has led to the statement, that polypus of the membrana tympani is a common affection, whereas the polypus never grows from its surface. The treatment of these fungi consists in avulsion and the application of nitrate of silver in the solid form. The exuberant granulations which in otorrhœa so frequently cover the whole surface of the external auditory meatus, are best *stimulated* by the application every second or third day of the solid nitrate of silver, or of sulphate of copper.

Polypus of the external meatus is quite a different affection from the fungi and granulations of which we have just spoken. They are not necessarily accompanied by otorrhœa; they are vascular growths of various consistencies, but all covered with true cuticle. Their seat of selection, if we may use such a term, is in the middle part of the external auditory meatus, about the situation of the ring of ceruminous follicles. Mr Wilde describes six forms of these growths. The first is a small ovoid body attached to the wall of the meatus by a pedicle, corresponds to Mr Toynbee's[1] "globular vascular polypus," and is an affection most frequently met with in children who suffer from otorrhœa. The second is a lobulated mass, not unlike a bunch of grapes, friable, and of a salmon colour, and seems to correspond to Mr Toynbee's "gelatinous polypus." The third form has a firmer consistence, and instead of a lobulated, a botryoidal appearance, and a more extended base; and seems to correspond to Mr Toynbee's simple "vascular polypus." The fourth has a fibrous structure, with a fleshy appearance, and a broad base, and is traversed by several superficial vessels. The fifth seems to be only a special form of fibrous polypus, which he says is very rare, and from its shape, may be denominated pyriform. "It grew from the anterior wall of the meatus, was remarkably tough and unyielding, and came away with its extensive base and roots;" in fact, was attached to the periosteum of the meatus, and possessed the characters of a fibrous polypus, as met with elsewhere. Such an arrangement of simple mucous and fibrous polypi, into a variety of divisions, according to their mere form and resistance, without any specialty in structure, symptoms, or history, to deserve particular attention, is only suited to distract attention, and render his description unintelligible. Mr Wilde's sixth form of polypus is the malignant—which is of a totally different nature from all the rest; it is not a growth from the meatus, but the projection outwards of malignant disease of the temporal bone. As there is generally evidence of this in the mastoid or preauricular region, or behind the angle of the jaw, there can seldom be any difficulty in recognizing its true nature. Should such difficulty arise,—the great vascularity of the growth, shown in its tendency to bleed, its ulcerated surface, the fetid smell of the discharge, with the appearance of the patient, and the history of the case, will generally be sufficient to excite our suspicion, and lead us to watch the progress of the case.

---

[1] See Medical Times and Gazette for 1852.

The simple forms of polypi almost never undergo a spontaneous cure; and as they are a cause of deafness and a source of increasing annoyance from their gradual enlargement, coming at length to fill up the ear and project externally—they should be removed. Various methods have been employed to effect this. Those commonly employed are the use of caustics: such as potassa fusa or Vienna paste; the use of cutting instruments, and instruments fitted to tear them away. The first is specially recommended by some authors, in cases where the growth springs from the meatus, near the membrana tympani—of all places the last where we should like to employ it—in fact, the use of caustics is to be altogether deprecated. Almost any knife, scissors, or forceps, of suitable size, is sufficient to remove the mucous growths; but in these days of misspent ingenuity, in the invention of elegant and complicated instruments, the following are considered most orthodox:—Hook-shaped knives, with the cutting edge on the inner margin; double-edged spear-pointed knives; small curved scissors, with long blades; Charriere's ear polypus scissors and forceps (the latter adopted, we understand, by Mr. Toynbee), closing by means of a spring slide;—most ingenious toys! Such an armamentarium is quite as appalling as it is unnecessary; and we would dismiss them all by saying, that the common ring forceps, which is in almost every pocket-case, for the softer kinds, and a pair of small scissors, or Mr Wilde's wire snare,[1] is all that is required.

Caries of the temporal bone is a common complication and consequence, but is rarely the commencement of otorrhœa. It frequently follows neglected otitis, especially in strumous patients; and it is quite astonishing to what an extent it may proceed before head symptoms occur, from which the patient usually dies. It has been doubted by some, whether the disease commences in the bone and afterwards affects the soft parts, or commences, as we have described it, in the mucous membrane of the middle ear, and extends to the periosteum, and thence to the bone itself. We think there can be little doubt upon the subject; the history of all such cases proves most distinctly that the soft parts are the first attacked by disease. Such considerations only the more urgently demand that something should be done to arrest the discharge from the middle ear, and the inflammatory action which gives rise to it; especially if we remember that when caries once gives decided evidence of its presence, practically speaking, it is incurable. Necrosis of the temporal bone, unassociated with caries, is a rare affection, except we take the ossicula auditus into consideration, or the exfoliations of small pieces of the meatus. We have no time longer to delay coming to the conclusion of our examination of this work; though we might spend much time most profitably in considering the lesions of the facial nerve, and the cerebral symptoms which generally are the cause of death in caries of the temporal bone;—still less have we time to speak of ear trumpets. What we should regret most would be passing over in silence

---

[1] This instrument consists of a steel conducting-rod, with a small fixed ring upon the end. Through this runs a wire loop, with the ends of the wire on either side, attached to a cross bar, which slides upon the conducting-rod; by means of which the loop is drawn tight, and anything in its embrace is firmly captured.

Mr Wilde's Appendix upon Deaf Dumbness, were it not that in our last number we gave a large extract from his tract upon this subject, in the report upon the Statistics of Disease in Ireland.[1] We have given this book our best attention. We think it might easily have been compressed into smaller bulk, without one whit diminishing its interest; and having noticed this discursive tendency of Mr Wilde's genius, we have exhausted all we can say in the way of criticism. Mr Wilde possesses a reputation, both on this and the other side of the channel, which this work cannot fail to extend; we congratulate him on the success it has already gained, and leave him, confident that his exertions will be duly appreciated by the profession.

ART. III.—*Ärztlicher Bericht über das k. k. allgemeine Krankenhaus zu Wien, im Solar-Jahre* 1852. Wien, 1854. S. 104, mit 16 Tabellen.

Medical Report of the Imperial and Royal General Hospital of Vienna, for the solar Year 1852. Vienna, 1854. Pp. 104, with 16 Tables.

It is but a truism to state, that medical science approaches its aims the most swiftly and the most surely, where it can establish its progress over extensive masses of fact. But where these facts, however diversified and complicated, have been gathered in a single wide field, under conditions in a large degree similar, and therefore fitted to give proportionately harmonious and uniform results, and that by a group of observers justly distinguished as men of prime attainments and energies, they have an additional authority conferred upon them, such as attaches to them the stamp of almost as high a value as can be fairly assigned to any similar efforts or products of the human intellect. If, beyond all this, we have these facts carefully and laboriously collated, and duly categorized, it is manifest that the advantages to be derived from them are still farther increased, by their being presented to us in a form in which they can be best considered and appreciated.

The report, to which we now wish to direct the attention of our readers, is one to which we cannot hesitate to assign the high qualities at which we have just hinted. The General Hospital of Vienna, whether from the vastness of its extent, or the completeness of its arrangements, is entitled to take the first rank among the similar institutions of Europe. Usually reported to contain 2600 beds, it has, not rarely, considerably upwards of 2000 patients actually within its walls; and 1600 inmates is the lowest number we have been able to observe, at any single period, through a glance at the movements of its sick population during the past decennium. As many as 26,000 patients have been received, or been under treatment, in a single year; while the average, for the space of ten years, has been nearly 21,000 annually, the smallest aggregate number, in any separate year, having been nearly 20,000. The average annual number of deaths, during the same space, has been somewhat under 3000; the sum having risen as high, in 1847, as 3991, and fallen as low, in 1851, as 2446. Thus, during the ten years terminating in 1852, upwards of 200,000 cases of disease have been treated within this institution; and nearly 30,000, who had undergone the last lot of mortality, rendered a final service to the science which had failed in its efforts towards them, through the large opportunities afforded by the Institution for placing the doctrines of pathology upon a more certain basis, by the instrumentality of somewhere about 15,000 necroscopic examinations, made under the superintendence of the eminent Professor of this department.

---

[1] See No. CC. of this Journal, p. 610.

This prodigious range of medical experience is presented, through the original plan of the hospital, in a form the most easily accessible. Numerous courts, some of very considerable dimensions, communicating with each other, and adorned with alleys of acacias and other ornamental trees, parterres of flowers, and fountains, are surrounded by extensive ranges of buildings, the whole of which are devoted to the complicated arrangements of the Institution. While the courts are open for air and recreation to the patients who are convalescent, or who may be enabled, through the nature of their ailments, to resort thither, and are provided with seats and reclining benches on which they may repose at pleasure, the buildings contain upwards of 100 large halls or wards, through which the sick are distributed, and classified under their different categories, with a greater amount of precision than can usually be admitted in less extensive establishments. Besides these, there are about 60 separate wards, for the reception of patients requiring greater seclusion; a large lying-in institution; museum and lecture-rooms; laboratories for the preparing and dispensing of medicines, and for pathological chemistry; apartments for pathological anatomy and for medico-legal dissections; residences for the principal and assistant physicians; a chapel, with residences for the officiating clergy and their subordinates; accommodation for the administrative officials; bath-rooms; and even an eating-house, in which the students can be provided with the more common refreshments, so that those whose fastidiousness does not stand in the way of their zeal, may supply all physical, as well as intellectual, wants, from dawn to eve, without quitting the precincts. The large lunatic asylum, like the hospital a government foundation, is in the immediate vicinity.

It is this concentration of the means for acquiring a knowledge of the natural history and treatment of disease, bringing all the multiform departments of medicine into immediate proximity with each other, and, as it were, localizing the science without dwarfing its proportions, that confers upon this establishment its leading distinction as a school for instruction. In a merely economic view, it presents another feature which widely distinguishes it from the hospitals of our own country. No patient is admitted wholly gratuitously. In as far as we could ascertain, the sick inmates are divided into four classes, with different rates of payment. The first class, with separate apartment, supply of necessaries, medicines, and medical attendance, pays about £4 sterling monthly; a second class pays about 1s. 8d. daily; a third class, embracing non-naturalized strangers, pays 1s. 1d. daily; and the fourth class, including residents and strangers who are Austrian subjects, is charged 7d. a day only. Where the circumstances of the patient are so reduced that he cannot himself sustain these charges, the corporation of which he may be a member, or the parish in which he is resident, is held liable. Neither can admission into the lying-in wards be regarded as absolutely gratuitous for any condition; the poorer class being held bound to officiate afterwards as nurses in the Hospital for Foundlings, which they have helped to populate, and in which their pallid looks and drooping features prove visibly enough that this contribution of service is no light exaction. As for that thrice unfortunate class, from whom the highest payments are demanded, who enter here secretly to sacrifice to Lucina, and who may come and depart veiled, and without utterance of articulate speech, and go forth leaving their unhappy progeny behind them, truly to suffer for the sins of its parents, we only know that it exists, but we have neither the desire nor the power to penetrate into its mysteries.

Although the report before us be for the solar year 1852, it has only been published at a recent period of the current year; the task of collating the extended series of facts, and of constructing the tables, being too laborious, and demanding too great accuracy, to admit of a hurried execution. The number of patients under treatment during the year amounted to 21,190, of whom 12,779 were males, and 8411 females; a preponderance, on the part of the males, which seems to have held good in nearly similar, or even in greater, proportions in former years. Of this number, the deaths amounted to 2684, of whom the males constituted 1433, and the females 1251: an excess of mortality, in relation to the latter, which the reporters appear to pass over without satisfactory comment, but which is too considerable, rising as it does to 16·61 per cent. against 12·12 of the former (*Table, No.* 6), not to merit attention; although other data as to comparative age are requisite, before the facts can be appreciated at their just value. This excess of mortality on the part of the females depends, however, on a set of causes which operate with considerable uniformity, as we find it to be more or less regularly prevalent during the ten years, of which 1852 is the terminus; the average for the whole period being 16·89 per cent. for the females, while it is only 12·54 per cent. for the males. The deaths in 1852, in both sexes, average about 7 daily; 18 being the maximum, and one the minimum, in a single day. Any one who can assume the privilege of entering the dead-rooms on a Monday, when there is a two days' accumulation of corpses, will occasionally encounter a spectacle well fitted to convince him, among a variety of other considerations, that a great public hospital is but a mingled good, and that it has its shocks to humanity as well as its solaces and its blessings. The highest mortality was in the month of May, when it rose to 321, the number of patients treated during the month having been 3872; and the lowest mortality was in September, when it was reduced to 162, the number of patients under treatment having been 2830. This amount of cases constituted also the maximum and minimum for the whole year. As to the duration of treatment, it amounted at an average to 38·1 days for each patient.

It is an especially interesting part of such an inquiry, and one which the reporters have not neglected, to determine what particular disease, or class of diseases, has had the most marked influence upon this rate of mortality. Thus we learn that pulmonary consumption causes not less than 0·38, or upwards of one-third of the gross mortality; and that next to this, but at a wide interval, is ranked inflammation of the lungs, which produces nearly an eighteenth; typhus, which produces a twentieth; pleurisy, which causes about a thirty-first ; the puerperal process, causing a thirty-second; and diseases of the sexual organs in females, a forty-seventh; while old age only represents a twenty-first of the aggregate. Nearly one-half of the whole amount of deaths in the Institution is caused by diseases of the respiratory organs. When we look to the results of treatment of any individual disease, considered apart, we find that of 100 cases of phthisis, 60·9 died, 38·9 were improved or remained stationary, but unhappily only 0·2, or at the rate of 1 in 500, in spite of cod-liver oil and all other *quasi* panaceas, could be reported as cured. Inflammation of the brain, apoplexy, Bright's disease of kidney, puerperal affections, dropsy, and organic diseases of the sexual organs in females, were among the other ailments, which were most fatal in proportion to the numbers brought under treatment. Taking these in this point of view, and under their simplest aspect, there were of consumption 99·8 per cent. not cured, and 0·2 cured: of inflammation of brain, 77·3 and 22·7 ; of apoplexy, 77·1 and 22·9 ; of Bright's disease, 87·0 and 13·0 ; of sexual diseases of women, 88·1 and 11·9 ; of puerperal affections, 46·4 and 53·6 ; and of dropsy, 63·8 and 36·2. Of organic diseases of the heart, no example of cure is given: a fact which may be regarded as saying much for the candour of the physicians, as well as something for the accuracy of their diagnosis. But in what proportions to the general mass of disease were these affections admitted into the Institution? Consumption, which we have seen produced a third of the total deaths, constituted 64·7 per mille, or about a fifteenth of the total cases; inflammation of brain ranked at 13·2 per mille; apoplexy at 2·3; Bright's disease occurred in precisely the same proportion; organic diseases of sexual organs in women appeared as 7·2; puerperal affections as 10·3 ; dropsy as 6·1 ; and organic diseases of heart as 8·4. These interesting facts, which are not without somewhat of an air of intricacy when they are thus presented in relation to each other, must yet be so considered and estimated, before we can assign to each its just position in any rational view of the conditions and prospects of human suffering. It is through the neglect of such checks, by which facts become subjected to reciprocal control, and each has assigned to it its due value and dimensions, that many exaggerated notions regarding the importance of particular classes of maladies insinuate themselves not only among the public, but among many conscientious members of our profession. It has frequently annoyed us, as lovers of science, to see physicians, men of zeal and ability, give themselves up in this way to one limited sequence of ideas, and all but refuse to see any thing beyond it, or otherwise than through its medium. Hence some favourite doctrine, or method of practice, rendered too universal in its application, comes prominently forward and obtrudes everywhere: a real illusion to themselves, and a delusion to others, who do not pause to weigh the little consistence there is between the comprehensiveness and boldness of the writer's statements and the distinctness of his proofs. With such enthusiasts, the special morbid condition which each has selected as his fondling, of whatever description it may be, lies at the root of every thing, and is capable of producing every thing. Its occurrence with other diseases, and with all kinds of diseases, however notoriously the greater part of these are proved by experience to exist frequently dissociated from it, is not regarded as a coincidence, but as a pure relation of cause and effect:—

> ——— Circumsilit agmine facto
> Morborum omne genus, quorum si nomina quæras,
> Quot Themison ægros autumno occiderit uno
> Percurram citius——
> Ille humero, hic lumbis, hic coxa debilis, ambos
> Perdidit ille oculos, et luscis invidet, hujus
> Pallida labra cibum capiunt digitis alienis.

Minute as the details we have already quoted may appear to some of our readers, we have not given a tithe of the elaborate information conveyed in even the merely introductory part of the report. We must pass on, however, to a few notices of those portions of it, which are devoted to a more special consideration of the principal separate forms of disease. Under the head of congestion and inflammation of the brain and its membranes, it is stated that the greater proportion of the cases of meningitis were of a tubercular nature; another proof of the large predominance here of the tubercular diathesis, or dyskrasia, as the Viennese pathologists have loved to term it, though the doctrine of the dyskrasiæ is now beginning to narrow its limits amongst them. Indeed this prevalence of disease of a tubercular origin seems to be conspicuous throughout Austria. At least, in the extensive hospital at Prague, it appeared that nearly a fourth of the diseases under treatment stood more or less closely in relation with the same type of organic degeneration; while in another celebrated hospital here, that of the Brothers of Mercy, consumption, in 1853, appears as 7·7 per cent. of the cases, but as 36 per cent. of the deaths, and not one cure is reported. In a case where there was capillary apoplexy, and softening of the white substance of the brain, there were found numbers of the cysticercus cellulosæ distributed through the brain, as well as in the muscular system throughout the body.

These parasites, it is known, may exist in the brain, in remarkable abundance, without any symptom, objective or subjective, to indicate distinctly their presence, or that of any other source of important organic mischief. We recollect ourselves having assisted at the examination of the brain of an individual, who had enjoyed good health, with the exception of slight headaches, till within two hours of the period of death; yet the brain was found studded with the cysticercus, not fewer than a hundred in number in as far as they could be reckoned. A careful examination was unable to detect them in any other part of the body. In a woman of 36 years of age, who had suffered for three months from violent, piercing headaches, and frequent vomiting, and had become emaciated and of a cachectic appearance, there were found in the medullary substance of the cerebrum scattered deposits of medullary sarcoma, from the size of a lentil to that of a pea; in the cerebellum one the size of an egg; degeneration of like character in the thyroid gland, and in the right lung; and one, the size of the fist, in the posterior mediastinum; the pseudo-plasm likewise presenting itself in both the ovaries. In how far the diagnosis here was established during life, we have no means of determining: but who that has himself seen an instance of medullary sarcoma within the thorax, and is acquainted with the celebrated case of the Marquis of St Auban, as related by Boerhaave, and transferred graphically to the lively pages of Zimmermann,[1] would not congratulate himself that, thanks to the progress of physical diagnosis and of morbid anatomy, even a moderately instructed practitioner may now stand, in relation to such a case, in a position of high advantage, when compared with a physician so great as Boerhaave in his own times? It is occasionally less than the truth, to state that the science of one age becomes the everyday experience of another.

Passing over the department of diseases of the eyes, in which we note nothing of especial interest, unless that, in twenty cases of hard lenticular cataract, the cures by operation, chiefly extraction, were at the rate of 68·75 per cent., and in sixty-five cases of mixed lenticular cataract of 77·78 per cent., we come to that of affections of the respiratory organs, under which we find the following case recorded. A man, 42 years of age, was admitted with catarrh of the left lung. This had already yielded, and the patient was about to leave the hospital on the following day, and had been out of doors. Returning leisurely up stairs, he felt suddenly unwell; and had scarcely reached his bed when a stream of blood gushed from his mouth, and he perished in a few minutes. On dissection, after a laborious search with the sound, there was at last discovered, in the lower lobe of the right lung, a distension of an arterial twig, of the size of a pin's head, the bursting of which had occasioned the fatal hemorrhage. We quote this case, because it is usual in the Vienna school, in cases of hæmoptoë in advanced phthisis, to point to its cause in the existence of a small aneurism, in a branch of the pulmonary artery lying in contact with the walls of an excavation; the distension of the vessel being produced by the pressure of the blood upon its coats, when deprived of the support proper to them in the healthy tissue. Of this interesting fact we have seen various distinct examples. Most of the cases of pleurisy only presented themselves after exudation had already established itself; and in many, tuberculosis had previously existed in the lungs, whilst in others, it seemed to be first developed after the effusion. In a few instances, while the patient appeared to be in a comparatively easy condition, death took place unexpectedly from œdema of the lung. In one case, the operation of paracentesis was performed with the aid of the apparatus devised by Professor Schuh. This apparatus, which seems little known in this country, consists of a trocar and canula; the latter provided with a stop-cock towards its upper extremity. After the chest is penetrated, with the usual precautions, the trocar is withdrawn just beyond the site of the stop-cock, but not wholly out of the canula, and the stop-cock is turned, cutting off all communication between the cavity of the chest and the external atmosphere. The trocar is now withdrawn; and a small silver trough is adjusted to the extremity of the canula, in such a way that the tube opens into near the bottom of the trough, the orifice being covered by a small flap valve suspended before it. At the further extremity of the trough, and near its upper edge, is another aperture, conducting into a terminal channel. The apparatus being thus connected, the stop-cock is opened; and the fluid, flowing into the little trough, rises in it above the level of the lower aperture, to pass away by the upper one, so that the former is doubly secured against the ingress of air by a flap and a water-valve. With this apparatus, which is simple, and acts with the greatest ease and efficiency, Professor Schuh has tapped upwards of a hundred times, in about fifty distinct cases of pleuritic effusion; and we believe that twenty of the cases recovered permanently. We suspect, however, that if certain of the physicians in the hospital had more faith in therapeutics, and less faith in their own want of faith, fewer of their patients, in this condition of disease, would require to be transferred to the wards of the surgeon; for we have undoubtedly, in our own country, seen many cases recover under the use of the ordinary resources of art, beside which a continental admirer of physiological medicine would stand, possibly after some triumph in diagnosis, with his hands folded, courageous only in his scepticism. We can only reason with this class of doubters in some such terms as those of Rousseau:[1]—"Je ne sais si l'art de guérir est trouvé, ni s'il se trouvera jamais: ce que je sais, c'est qu'il n'est pas hors de la nature."

---

[1] Von der Erfahrung in der Arzneykunst (3te. Auflage), p. 152.

We have already seen that pneumonia is a very prevalent and widely fatal disease in the hospital of Vienna, where it produces $\frac{1}{18}$th of the gross mortality from all diseases. Considered with reference to the results of treatment, its fatality is also very considerable, the proportion of deaths being 22·8 per cent. of the cases. But the followers of Hahnemann have no cause to point to this result in triumph, for assuredly it has little connection with the employment of any energetic means of cure. Professor Skoda, and the school of Vienna generally, entertain the idea that pneumonia reaches its acme naturally on the sixth day, when the infiltrative process ceases, and, under favourable circumstances, the disease tends to recovery. To bleed during the period of increment is only to disturb the process without checking it; and beyond this period venesection is dangerous. Very moderate doses (2 to 3 grs.) of Dover's powder, decoction of marshmallow, and afterwards quinine, with, occasionally, corrosive sublimate in the quantity of $\frac{1}{4}$ gr. in the 24 hours, constitute the leading resources. These ideas of practice certainly do not prevail absolutely in the hospital, but their influence is, not the less, too generally felt to permit the customary employment of measures of real efficacy. For our own part, we know of no more successful method of treating pneumonia, than a slight modification of that which is laid down by Laennec,[2] and which differs widely from that of Rasori, with which Laennec was only very imperfectly acquainted, though it is in some degree suggested by it. We have ourselves no *practical* acquaintance with the proceedings of the Italian contro-stimulant school; and we look almost with incredulousness on such statements as those of Strambio[3], who cites cases in which six drachms, or even an ounce of tartar emetic was exhibited in the course of a single day, and one instance in which it was carried to the extent of upwards of six ounces during ten days of treatment. But with a single venesection, and one, rarely two, grains of this remedy every two hours, or, in a few instances, every hour, we can venture to promise to any practitioner, whose mental attributes at all fit him to approach a formidable disease with a heroic remedy (for it is not the possession of the sword which constitutes the fencer), a success not only far beyond that indicated in the report before us, but beyond that of a Fleischmann or a Dietl, whose equally expectant methods are distinguished by little else than a diversity of designation. Under the head of consumption, two cases are mentioned in which the existance of tubercular deposits were diagnosed during life, in both lungs, yet in which neither cough, dyspnœa, nor expectoration preceded death, following upon extreme emaciation. Most practitioners of experience will remember to have seen similar, though rarely occurring, cases in the course of their practice. Of diseases of the heart, by far the greatest proportion consisted of insufficiency of the bicuspid valve, either with or without hypertrophy. A labourer, twenty-four years of age, who died of œdema of the lungs, was found to have had insufficiency of the bicuspid, with marked stenosis of the aorta, along with an unclosed condition of the *foramen ovale*, without any tokens of cyanosis having ever presented themselves.

---

[1] Lettres écrites de la montagne, P. I., L. III.

[2] Traité de l'Auscultation, t. i., p. 425. (2d ed.)

[3] Intorno il modo di agire delle Sostanze Emetiche e Purgative, e principalmente del Tartaro Stibiato, pp. 86, 99.

We are already leaving unnoticed many of the abundant and instructive materials of the report, and our space will scarcely permit us to do greater justice to the division for venereal diseases; a department which is under the superintendence of the judicious and eminently accomplished Professor Sigmund, of whose courtesies, and more than courtesies, many of our countrymen who visit Vienna will doubtless retain a lively recollection. In this division of the Hospital there are 300 beds, and the number of patients admitted during the year exceeded 1300. The lengthened experience of a man of talent and energy, in a single disease, and on so wide a basis, must ever be entitled to respect. Gonorrhœa is here chiefly treated by injections, the salts of zinc in solution being usually preferred; and so far is it from being found that this method of treatment favours the formation of strictures, we observe it hinted elsewhere[1] that there is actually a want, among patients re-entering these wards, of examples of this form of disorder, to serve the purposes of clinical demonstration. Professor Sigmund considers that he has proved by his investigations, and especially by his numerous inoculations with the venereal virus, that the pustule and chancre subsist as a wholly local affection till the full termination of the fourth day; and he justly points to this fact as one of leading importance in its practical applications. In the diagnosis of secondary syphilis, much weight is attached to the condition of the lymphatic glands, and especially of those situated immediately above the elbow, which are found slightly swollen and indurated. In the true indurated chancre, Professor Sigmund resorts to mercurial inunction: in secondary forms, a solution of corrosive sublimate is his favourite remedy. Upon the whole, a close observation, hitherto, of upwards of 1000 cases in his wards, under the most various external conditions, and in the most diversified constitutions, has shown that the mercurial treatment presents the fewest relapses, and deserves a general preference over that with iodine. Still, where special indications of cure present themselves, the preparations of iodine, and other remedies, are resorted to as the occasion may appear to require. The advocates of the non-mercurial treatment will find as little here to encourage them, as in the pages of old Wiseman, of whose opposition they probably wotted little, while believing themselves to be the expositors of a new doctrine. Yet this patriarch of English surgery speaks very shrewishly[1] of "the wickedness of many pretenders, who will in this cure declaim against the use of mercury, in which, if they speak honestly, and follow their judgments (and do not give it at the same time when they speak against it, as many do), they will prolong their cure to no purpose, and meet with disgrace at last; it being very sure, that no species of it will be cured without it." At Vienna, salivation was not desired as any part of the cure, and it followed only twice incidentally after the internal use of mercury, but more frequently after the inunctions.

---

[1] Zeitschrift der k. k. Gesellschaft der Aerzte zu Wien, Mai, 1853.

We learn from the report of the department of diseases of the skin, that, of 517 cases of smallpox, infection could only be traced distinctly in 77 instances: one of those lessons of caution which might have induced our late Board of Health, had they chosen to seek them, to hesitate in gainsaying unconditionally the infection of cholera, proceeding, as they did, upon the unstable ground of negative proofs. To deny the existence of infection in a disorder, the movements of which are never compatible with any restricted notions of its origin in local, atmospheric, or climatic influences, and that because all who are exposed to it are not affected, is to mistake the laws of mediate contagion, on the one hand; and to deny it, because we are unable to trace its progress through the tangled paths of commerce, or among the mazes of a dense population, is to overrate our powers of observation, on the other. No fewer than 1202 individuals were treated for itch; and the theory of this affection was illustrated by Professor Hebra's demonstrations of the distinction of sex in the acarus. Passing over the observations of Redi, Moufat, and others, and even the distinct, though restricted, notice by Morgagni, for the work of the great Italian is no longer a common text-book, it has often surprised us that the knowledge of the existence of an insect which is so plainly described by Peter Frank, the most widely esteemed and followed clinical teacher of his day, whose singular fortune it was to have taught in five different universities, and many of whose pupils are still living, should have died away so completely from the recollections of the medical world, as to have been hailed lately, when again introduced to notice, with the eager curiosity which only greets such discoveries as the public believes to be new, and as chance to hit its fancy or its humour. Yet how clearly and unhesitatingly has this eminent observer pointed to the existence of the acarus, as the sole and undoubted cause of the disorder in question :—" ab acaro humanæ cuti infesto, contagii præmissi sub aspectu." And he adds the more complete description, in another part of the rich treasure of fact and of comment which he has bequeathed to us :—" pustulas vix cutem egressas, limpidoque sero recenter impletas inhabitat; ac epidermide tectus, ad vicinas sub hac ipsa partes, *quasi per fodinam*, macularum striarumque rubrarum, filiformium aspectu detegendam, divagatur."[1] Truly this antiquity of novelty is a curious subject of inquiry. Of eczema 273 cases were treated, 12 of which were universal, and 261 local: of these 90 per cent. were cured, and 8 per cent. improved. The treatment consisted, in recent cases, of cold applications, as slightly imbued with moisture as possible; and of more irritating applications in cases of longer standing, as black or green soap, dilute or strong solution of caustic potass, brushed over the surface, or ointment of iodide of sulphur, the latter in the proportion of a scruple to three drachms of simple ointment, rubbed in twice or thrice daily, combined with an assiduous use of cold applications. When the eczema had passed into its third stage, and appeared as pityriasis rubra, the part was pencilled over with beech tar, or with the less irritating oleum cadinum, or *huile de cade*, and left for some time uncovered till the layer had become dry. When the incrustation separated, if the skin had not resumed its normal aspect, the application was repeated. In two examples of general eczema, of several years' duration, the remedial plan adopted in some of the Swiss watering-places, and especially at Luesch, was resorted to. The patients remained continuously for eight hours daily, during a period of six weeks, in the tepid bath, in which they took their meals. They bore this treatment without injury to the general health, and the eczema was diminished, though whether permanently or not must be a question of time. Cases of psoriasis were successfully treated with vapour baths and pencilling with oleum cadinum. By the way, the use of this remedy, the product of the dry distillation of the Juniperus oxycedrus, is another instance of a practical fact, which had fallen into neglect, having risen again into vogue, and been accepted as a novelty. It appears to have been first borrowed from popular veterinary practice. "Ol. de cade," says Hofmann,[1] "seu de mera, habet in illis regionibus usum, quem olim, in scabie ovium," &c.; and the Sieur Liger[2] recommends the "huile de cade" in "la galle" of dogs. Probably, with those who concern themselves more with what is good in science than with what is new, it will remain a matter of indifference whether it or creosote retain the field in the treatment of cutaneous affections. We have rarely been disappointed, in lepra or psoriasis, with the use of the warm bath and creosote ointment. In the treatment of scrofulous ulcers, cold applications, compression, and caustics, were resorted to, with cod-liver oil administered internally. The cheaper olive oil, either simple or iodated, was tried as a surrogate for the latter, but with doubtful satisfaction. Speaking of surrogates, who has not heard of the *Manteca da Tortuga* of the Amazons river, obtained by extracting the oily matter of the eggs of the turtle by heat, and converting it into butter? It is extensively used as an article of diet among the residents, and is described as always retaining a taste of fish-oil. Surely some of those ready-witted fellows, who turn popular predilections into profit, might drive a commerce in this oily nutritiousness, the very name of which, with its gastronomic associations, should establish its success at once, and shake the supremacy of cod-livers in the market. We record our hint here, and really expect some one to take advantage of it, though we are not quite sure that they will remember its source with proper gratitude.

---

[1] Chirurgical Treatises, B. viii., p. 8 (1676).

---

[1] De curandis hominum morbis, T. i., § 433, 436.

Among the accidents, is narrated the case of a young and vigorous attendant on an itinerant menagerie, who had been bitten by a rattlesnake on the back of the left hand. Within an hour an extensive œdema had reached as high as the middle part of the arm above the elbow, and he complained of giddiness, headache, dimness of sight, thirst, and other symptoms. The wound was cleaned, and cauterized with the hot iron, and bladders with ice applied to the arm. Internally, ammonia was prescribed in a mucilaginous decoction. The patient recovered. Chilblains were successfully treated with lotions of dilute nitric acid, in the proportion of 1 drachm of the dilute acid of the Austrian pharmacopœia (we believe 1 to 3) to from 1 to 3 ounces of distilled water, according to the degree of sensibility of the parts. Pencilling with tincture of iodine, more or less diluted, has rendered us good service in this occasionally troublesome affection. We recollect to have seen in a Swedish medical work, the recent production of an author of character and of high popular repute,[1] a prescription for chilblains, which advises a pair of closely-knit socks to be dipped in melted tallow, wrung out, and worn under other stockings night and day, without being removed, for three successive winters, when the cure will generally be found permanent. We may throw up a straw to see which way the wind blows: and does not such a prescription as this, so promulgated, afford a wide revelation of the domestic and personal habits of a nation?

---

[1] De Medicamentis Officinalibus (1738), p. 306.

[2] Oeconomie generale de la Campagne (1701), t. ii., p. 296.

The number of post-mortem examinations made during the year, in the rooms for pathological anatomy, amounted to 1388. Of these 22 are specified in the report, on account of points of remarkable interest which they presented. We may quote, as an example, the case of a man 31 years of age, who had undergone the operation by incision for hydrocele. Death ensued suddenly afterwards, when a cord-shaped coagulum of blood, three feet in length, was found coiled up like a ball, and obstructing the pulmonary artery. We ourselves saw a similar case, in which instantaneous death was caused by a large coagulum which had passed from the right ventricle into the same artery. In the laboratory for pathological chemistry, the number of investigations instituted were 1140; chiefly of the urine, but also of the fæces, sputa, matters vomited, exudations, blood, pus, concretions, pseudoplasms, and other products of disease. In the clinical wards of Professor Oppolzer, especially, the urine of each patient was examined at his entrance, and then the more essential changes were daily investigated till the issue of the case; viewing them in relation to the treatment and to the appearances on dissection should the disease have terminated fatally. The diminution of the chlorides in the urine continued to be observed as a dangerous, and indeed generally as an unfavourable sign, in puerperal affections, as well as in many other acute diseases. With reference to the process of lactation, it was ascertained that most of the nurses transferred to the Foundling Hospital, whose debilitated appearance we have already mentioned, afforded a milk either nearly or altogether destitute of caseine, while the proportion of sugar was increased. The urine in measles and in scarlet fever was observed to differ through the greater proportion of uroxanthin in the former; and the chlorides were observed to diminish before the occurrence of anasarca in the latter.

---

[1] Hartman, Husläkaren (third edition), p. 391.

The vast scope for medical study, some of the leading points of which we have just faintly indicated, affords occupation to a numerous body of able and energetic teachers; whose courses of instruction, public, as well as for more private and limited classes, are extensively followed by students from various, and often remote, quarters, many of them being already graduates in other schools. Not fewer than about fifty different courses on the multiform departments of practical medicine and pathology are delivered within the Hospital, or, though in small proportion only, within the neighbouring building of the Josephinum; besides lectures on psychology, chemistry, natural history, botany, and other accessary sciences, which are delivered chiefly at the place of the university. The fees for these courses are exceedingly moderate; and are besides, with the exception of those for the more private lectures, still farther restricted, through a privilege reserved by the authorities, of causing certain students to be admitted either gratuitously or at half the usual honorarium, by the free exercise of which we have reason to know that the emoluments of the teachers are frequently encroached upon to a burdensome extent. The charge for a private course, of six weeks' duration, is usually fifteen guldens, or about thirty shillings, at par, of our money. The public courses are of the ordinary length of the academic session. The lecturers are classed into ordinary and extraordinary professors, and *Privatdocents;* the latter being as much under the appointment of the government as the former. Of the five divisions of the Hospital set apart for clinical instruction in medicine, the two which present the greatest general interest, and are the most largely frequented by students, are those of Professors Oppolzer and Skoda. These consist each of two wards; those of the latter containing thirty-eight, and of the former about fifty, beds. The clinical teachers have the usual right of selecting, from among all the entrants into the Hospital, such cases as they consider the best adapted for the purposes of instruction: hence, whatever may appear as a type of a particular form of disease, or may exhibit or illustrate a symptom of peculiar significance, or sustain or impugn a prevailing medical doctrine, or test a practice, may generally be found here in its chief perfection.

The system of teaching is conducted in the following manner, and nowhere with more eminent success than under the superintendence of Professor Oppolzer, whose method we must now be considered as describing. Each new patient, as soon as admitted into the wards, is placed under the charge of one of the more advanced students, whose duty it is to draw up an account of the case, fully and circumstantially, detailing the anamnesis, and describing the condition when received into the Hospital. In discharging this task, he is expected to have recourse, in as far as he is able, to the most approved means of diagnosis, and to present a methodized view of the general symptoms and physical signs of the disorder. This account is read at the bed-side, on the ensuing clinical visit, in presence of the Professor and the assembled students; when the former makes such remarks as he considers necessary to elucidate, correct, or complete the details, questioning the student farther as to the grounds of his statements, and testing his accuracy, where requisite, by direct reference to the condition of the patient. The professor then makes his own investigation of the symptoms, as they exist at the immediate period of inquiry; enunciating each in its due sequence, and accompanying it by such appropriate comments as may secure the correct appreciation of the hearers. The Professor, and student in charge, having now a common ground on which to proceed farther, the latter, with sometimes another in succession, is subjected to a sifting examination, which often lasts from half an hour to twice that period, and rises occasionally into a complete theoretical and practical exposition of the disease under review. The causes, with their mode of operating; the symptoms, with their essential basis, their relations, and their actual significance; the process of analysis and synthesis, directed so as to intercept the sources of confusion and error, and to group the facts into their legitimate form, as the expression of a deranged function or an altered structure; the diagnosis, thus sought after by a strict, double method of conclusion and exclusion; the prognosis, careful to anticipate the possible intervention of secondary causes, and the amount of their influence; and the treatment, whether hygienic or medicinal, become each in their turn the object of a close and systematic consideration.

What is chiefly admirable in the teacher, while conducting so intricate an inquiry, is his extraordinary precision, and power of concentration. Each step with him, in the process of investigation, has its import and value, and its exact succession; and the facts are announced at once, in their due series, so clearly and so unhesitatingly, that they seem almost to be perceived by intuition. In the sagacity of his inferences, the acuteness of his perception of analogies, without the common propensity of suffering himself to be misled by them, and the facility and copiousness of his illustrations, he is equally remarkable; and nothing can be more direct than his mode of impressing everything on the minds of his hearers, teaching them observation, analysis, and deduction, by practising it before them, and inviting their concurrence. In catechizing a student, it is delightful to observe with what dexterity he can surprise a lurking ignorance, and guide it towards knowledge; and with how complete a tact he can lead a pupil to enter into the depths of his own mind, and arrive at a consciousness of his real resources or of his defects. Much of this didactic ability doubtless depends upon the long continued, special direction of his energies, in his particular province, which has given to their efficiency all the force and ease of a habit; but far more must depend upon genuine aptitude, or, in other words, upon a naturally logical constitution of mind, and upon knowledge and talent: and if it be his genial enthusiasm, and kindliness of manner, which first engage our attention, it would be a gross injustice if we were to fail to admire that vigorous intellect, and wide compass of acquirements, which can bring to bear instantaneously, on any given subject which may chance to offer, the most varied and accurate knowledge of healthy and morbid anatomy, of pathological chemistry and histology, of the use of the microscope, of physical diagnosis, of the history and general phenomena of disease, and of the diversified resources of therapeutics, and, where these are capable of exemplification, can immediately demonstrate their application before his audience; and all this with an exactness throughout which elicits the truth, and a vivacity, and clearness, and fluency of expression which make it evident to others, constituting, altogether, as interesting a display of professional learning, and of intellectual acumen, as it is possible to witness. Nor is that candour ever wanting, which openly confesses, where requisite, the failure of the resources of the art, either to discriminate the nature of a disease or to point out its remedy. During the after progress of the case, its symptoms are watched over and discussed in a similar manner. Should its result be unfavourable, there remains a crowning test of the Professor's accuracy, in the necroscopic examination made in his presence, and that of the students, by the Professor of morbid anatomy, or his assistants.

The fame of Oppolzer is that of a teacher and practitioner; and rests upon his admirable method in the one, and the extent and promptitude of his resources in the other. As a writer on medicine, he has neither acquired nor attempted any particular distinction. We are quite aware that the outline which we have given of his qualifications may be regarded as extravagant, and indeed it implies the possession of a very singular and striking combination of talents: but the crowds of students who throng round him in his prelections, limited only by the possibility of approaching him, and the homage of gray hairs and experience which is never absent from amongst them, together with the paramount confidence placed in him, as a consulting practitioner, by his medical brethren and by the public of Vienna, and the fact that this is the third university which has sought his services, show that his reputation is felt as a reality; and assuredly it would be well if medical reputations could be held as founded everywhere on as pure and solid a basis. We have been the more minute in our sketch of his clinique, because we regard it as a type of the main excellencies of the school of Vienna, with the smallest blending of its defects. Therapeutics are with him not only never neglected, but they are taught in their widest sense; and balneology, as it has been termed, or the use of baths and mineral waters, stands forth, in particular, with a prominence, for which, in this country, we neither exhibit the tendencies, nor possess the opportunities. Still, many of the ideas of treatment are such as it would be very difficult to reconcile with our notions; and assuredly the attempt is not made, British medicine being regarded as something too strenuous for the powers of resistance of a German constitution, though not, we are deeply convinced, with any very just appreciation of the general nature of the one, or of the qualities of the other, as they stand in mutual relation.

We believe, in fact, that nowhere more widely than among physicians in this country there prevails the conviction, that, as the public knows so little of the grounds upon which a medical practitioner should be appreciated, is so prone to mistake pretence, and overmeddling, and bustling obtrusiveness for real efficiency, and to catch at one prominent feature of imaginary, or counterfeit, or possibly real excellence, and so concede all the rest, we are bound the more in honour to treat its infirmities with wary gentleness; and to hold its health and life all the more precious and safe from rash assaults, because we see it every day flung at the feet of assumption and charlatanry like a worthless bauble. But it seems a prevailing condition that, wherever the study of the natural history of disease becomes predominant, as at Vienna, that of the practical and sound application of remedies wanes in proportion. There is something in the bent which qualifies for the one, which fosters a growing disinclination and incapacity for the other; and the cultivators of medicine forget that it is an art as well as a science, and that the science becomes barren when it ceases to furnish the rules of the art. We would not willingly be held to undervalue any department of real knowledge; but the departments of knowledge must be kept in their due subordination, and we must remember that the experienced practitioner, who, with a sagacious empiricism, casts a rapid glance over the functions of the system, notes broadly the degree and kind of their derangement, estimates the general condition as to vigour, and prescribes accordingly, may confer an infinitely greater service upon his patient, than he whose chief pride it might be to discriminate, or to fancy that he had discriminated, a chink in an atrioventricular valve, or the twanging of a fibrinous thread between the heart and the pericardium. It is as a school of diagnosis that Vienna may be visited with the greatest advantage; and the student will nowhere find this branch more assiduously cultivated, in its extended applications, than in the wards of Professor Oppolzer. The zeal of the teacher transfuses itself into a whole bevy of *alumni*; and often, we are constrained to confess, to the exceeding prejudice of the patient who may be suffering from an acute or harassing form of disease. No one can be more deserving of sympathy than the unfortunate exhibiter of a mitral murmur, or a friction-sound in pericarditis, of more than ordinary intensity, who must first undergo the clinical examination, such as we have described it, and then the forty-fold, reiterated, percussing and stethoscopizing of a host of eager students. Yet even to note this, and very notable we found it, is but to tag together the remote ends of time:—

Languebam: sed tu comitatus protinus ad me
  Venisti centum, Symmache, discipulis;
Centum me tetigere manus Aquilone gelatæ:
  Non habui febrem; Symmache, nunc habeo.

The teacher of clinical medicine, who attracts the greatest number of pupils next to Professor Oppolzer, is the well-known Professor Skoda. The method of this teacher differs widely from that of his colleague. Less cordial and frank in his demeanour, he is less able to establish a community of sentiment between himself and his pupil, and he is followed in his instruction, not accompanied. From the students themselves he more rarely attempts to elicit anything; neither seeking to test their deficiencies, nor habitually to exercise them in the various branches of diagnosis, &c. Thus, many of his prelections, however shrewd and clearly expressed, might be delivered nearly as profitably *ex cathedra*, in the hall of a university, as *prope lectum*, in the wards of an hospital. As a stethoscopist he is able, acute, and observant; but in his practice, far more than in the pages of his celebrated treatise, an indecision, carried to excess, too frequently mars or even stifles his judgment. Still, though accepted by his countrymen as their leading authority in physical diagnosis, his actual claims to originality seem to us to rest on a narrow foundation; and his theory of consonances, in particular, we believe to be founded on a misapprehension of certain laws of physical science, with the study of which he has evidently little familiarity. We do not, however, impugn the accuracy of his observations. The common imperfection of medical science is to reason ingeniously and accurately on premises defective through the want of well-ascertained first principles, many of our proofs being themselves assumptions requiring proof. With Professor Skoda it is occasionally the reverse. In therapeutics, his habit of hesitating, and waiting for further testimonies and clearer indications, renders him nearly inefficient; there being no worse practical men, as Bishop Warburton tells us, than those who require more evidence than is necessary. With the exception of a successful treatment of delirium tremens, with grain doses of opium repeated every hour, we never saw him attempt anything that was either bold or efficient in medicine. We recollect, however, that his comments on this disorder, and on the occasional causes of risk and failure in its treatment, were both judicious and practical.

Of the two teachers of clinical surgery, Professors Schuh and Dummreicher, we can only offer a hurried notice. The latter, acute, self-possessed, and active, presents the leading characteristics of a good operating surgeon: the former is one of those fortunate individuals, who can join to such qualities the possession of those higher powers of the intellect, which fit them for abstruse research and scientific deduction. The method of clinical teaching here resembles closely that in the medical wards. The patient is primarily placed under a student, who examines his case, and draws up a written statement of its nature, origin, present condition, probable results, and requisite treatment. Before any important operation is resorted to, this statement is read aloud by the student before the class; when the Professor makes such remarks as he considers necessary, questioning the reporter farther, and correcting him where he errs, as well as supplying his deficiencies. The class being thus thoroughly informed of the nature of the case, and farther instructed by a detail of the Professor's knowledge and experience in similar cases, the operation is performed, usually by the Professor himself, sometimes by his assistant, or, in certain cases, by one of a limited class of students, always already graduates, who are advanced by government, for special services under whom they are intended, to the privilege of this description of practical instruction. The clinical teaching, beyond this, is of the ordinary character. In the obstetric department, the field of observation is immense. In one of its divisions, which is allotted to the instruction of physicians, the number of beds is 300; the deliveries in the year are about 5000; and as many as forty child-births have been known to take place in a single day. In the second division, where midwives only are admitted for instruction, the beds are 200 in number, and the deliveries are about 3000 annually. The infants are transferred to the Hospital for Foundlings, where they perish at the rate of 50 per cent. during the earlier months, and 65 per cent. before they are finally removed from the charge of the institution; a Massacre of the Innocents, still on a scale large enough to proclaim the true character of these places of legalized murder under the guise of charity, yet fortunately so far reduced from that of a former period, as to prove that a humane management may effect much, even in spite of a system vicious and cruel at the core. Luckily for humanity, the diseases of infants, in such a locality, present no just analogies with those in ordinary life; and they can only be profitably studied there under a continued reference to the circumstances in which they have originated. Better opportunities may be obtained at the clinique in the St Anne's Hospital for Children, an establishment almost a model of its kind, under the superintendence of the benevolent Professor Mauthner; and, in fact, grafted upon an institution originally founded by himself, so far back as 1837, at his own charges. Various separate courses on diseases of the eye, of the skin, of the chest, and on syphilis, complete the bulk of the clinical instruction of the school. For the department of mental diseases, there is the adjacent, newly completed asylum, containing about 450 beds; contrasting favourably in the excellence of its arrangements with the older, and more nearly contiguous edifice, the conspicuous *Irrenthurm*, which is still fully occupied, and gives a cheerless accommodation to 350 lunatic patients. The museum for pathological anatomy presents a valuable collection of specimens, 2000 in number; but the apartment is dingy, and its arrangements mean and inelegant.

The Medical Faculty of Vienna numbers approaching to 500 doctors of medicine and surgery, who constitute a *Doctoren-Collegium*, presided over by a Dean. Recently this body has been attempting to evince some scientific activity, in its corporate capacity, and has published several annual reports of its proceedings; but it must, as yet, be content to be measured rather by its aspirations, than by the extent and importance of any efforts which it has accomplished. The Imperial Society of Physicians is an elective body, numbering at present upwards of 100 resident members, and embracing all the more distinguished cultivators of medical science in the capital. Its proceedings are conducted with corresponding zeal and talent; the meetings of its different sections, for pathology, therapeutics, hygienics, and pharmacology taking place at short intervals, and the results being communicated in a monthly periodical published under its auspices. The surgeon in Vienna, strictly so called, is still the barber-surgeon; and is bound by law to keep an open shop, and one or more attendants, for shaving the lieges. Of this class, there are about 120 in the city and its suburbs. The greater part of the ordinary obstetric practice is in the hands of the midwives, who number about 600.

The literary activity of the medical profession in Vienna is by no means inconsiderable. Among the more important of the works recently published, we may notice the anatomical treatises of Professor Hyrtl, which enjoy a wide and a merited popularity. His Compendium of Descriptive Anatomy has passed into its third edition in the course of a few years; and his Manual of Topographical Anatomy, first published in 1847, is also already in a second edition. The clearness of exposition characteristic of the style of this distinguished teacher, in his public lectures, is also a prominent feature in the first of these treatises; and in the second, there is a curious, as well as a copious, felicity of illustration, derived from a singular variety of sources, ancient and modern, which renders it one of the most amusing and remarkable productions of modern medical literature. The work of Professor Schuh on Pseudoplasms is of great research and authority. A production of remarkable interest also is the treatise on Pathological Histology by Professor Wedl; in which the morphology of the abnormal tissues, as seen through the microscope, is described with a fulness and a perspicacity which afford striking evidence of that skill and perseverance, through which alone a task so extensive, and so almost exclusively based on original inquiry, could have been successfully prosecuted. The caution and modesty, withal, of this experienced microscopist are not the least conspicuous of his qualities; and we might, on this score, safely recommend the faithfulness of his observations, and the wariness of his inferences, to the imitation of some of our own youthful or less practised observers. The technical execution of the volume is most creditable to the press of the Gerolds; and the designs by the author and his brother, as represented by the wood-cuts of Rosenzweig, are of corresponding excellence. A fifth edition of Professor Skoda's treatise on Percussion and Auscultation has newly appeared; with few essential alterations, but with a reconsideration of some points with regard to the movements of the heart, and the diagnosis of its adhesions with the pericardium. The manual of Percussion and Auscultation, also, of the late Professor Zehetmayer, a work of great practical value, though little known in this country, has reached its third edition, and has been farther recommended to notice in a brief introduction by Professor Oppolzer. It embraces, however, no part of the diagnosis of the diseases of the heart and arteries. Drs Braun and Späth, the assistants in the obstetric wards, are endeavouring to turn to account their wide opportunities of experience, in occasional clinical reports. Dr Edward Jaeger, the son of the celebrated oculist, Dr Friedrich Jaeger, who is still in full vigour as a practitioner, has published a number of observations, from his own and his father's practice; many of which are interesting, but are chiefly noticed here on account of the accompanying description of a modification of the eye speculum which he has contrived, and which, from its simplicity, commodiousness, and portability, we think is likely to be extensively adopted. As to publications in preparation, we are promised a second edition of the great work of Professor Rokitansky on Pathological Anatomy, which has been so efficient a cause of celebrity for the Viennese school. The principal assistant in the Pathological Institution also, Dr Heschl, is engaged in publishing a compendium on the same subject, which, from what we know of his zeal and ability, and his rapidly acquired distinction as a teacher, will, we have no doubt, prove a valuable contribution to medical literature. The experienced superintendent of the laboratory for chemical pathology, Dr Heller, has also a work in preparation, embracing the whole range of the department which he has cultivated and taught with equal success. A compendium of physiological and pathological chemistry by his able assistant, Dr Kletzinsky, is, we understand, already under the press. It is to be regretted that the vigorous sense and polished style of Professor Sigmund is now chiefly known to us only through publications in the periodicals. We learn with pleasure, however, that his various sketches of climatology, and places of resort for invalids, graphic as these are, and characterized by the judicious prominence which he gives to the really essential points of knowledge, are likely to be reissued in a collected form.

We thus close our lengthened, yet still exceedingly imperfect sketches, of the more general arrangements, and recent scope of action, of the hospital of Vienna; regarding it, as in justice we ought, as the great centre of the efforts, and source of the scientific knowledge, of the most distinguished cultivators of medicine in the Viennese school. The materials in our possession might easily have enabled us to have introduced many additional topics, as well as to have extended our information, which we should have still more willingly attempted, upon not a few points which we have merely indicated; but we have already exceeded our limits, and besides our mood was not one for didactic precision, and we had neither the hope nor the intention of exhausting a subject, which is too rich in itself, too interesting, and too widely suggestive, to admit of being fittingly reduced within a narrow compass, even if our means had been commensurate with the effort. We have hoped that some intelligence, which may be inadequate, but which is true, and at least not antiquated, concerning the main field of labour of one of the principal medical schools of the world, might be acceptable to many of our readers; and as such, we present it to their consideration.

ART. IV.—1. *On Gout; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M.D. Third Edition. London, 1854. Pp. 400.

2. *Practical Observations on Gout, and its Complications.* By T. SPENCER WELLS, Fellow of the Royal College of Surgeons of England, &c. &c. London, 1854. 12mo. Pp. 288.

We welcome the appearance, thus early, of the third edition of Dr Gairdner's work, and we congratulate our readers on the additions made to its pages. The author, it is true, observes with regret the growth of his book. He had hoped its distinction would have been its conciseness. Yet why should our best writers on gout be desirous to compress its literature within narrow limits? And how is it that its bibliography contains so many of the neat and pocket-fitting duodecimos, and that so few authors have had the ambition to fill a goodly octavo with the history, the nature, the complications, and the cure of gout? Dr Craigie and Dr Forbes have each pointed out for our reference a long list of authorship on this curious and interesting disorder; and it cannot fail to strike the least attentive observer how much the aim of being concise appears to have actuated the great bulk of the writers. Sydenham, who wrote so well, has only written too briefly on his favourite subject; and in his desire to be concise, has given his *Tractatus de Podagra et Hydrope.* Lug. Bat., 12mo, as an example to be followed by Lister, and Musgrave, and Cheyne, and Cadogan; while the thoughtful reflections of Sir Henry Holland, comprised within fifty pages octavo, have been followed by the short, but admirable and readable works of Todd, and Gairdner, and Wells. We know that we run no risk of offending, when we say that the history and nature of gout have yet to be written; and though much has been done in the elucidation of the subject, still much remains to be done in order to accomplish the much desired task. When the feat has been achieved, the successful author, we apprehend, will not be able to claim, as a distinction of his work, that it is short and concise. We desiderate for gout what Mr Ancell has done for tuberculosis.

It is not our intention to analyse with care the two works at the head of this article. The one has been for some time before the public, and has been favourably noticed in various uarters; the other, of much more recent date, and still in its first impression, has also received its meed of praise, to which we think it is justly entitled. We agree with much that has been said by both authors; we disagree with some things that have been said by each; but we willingly accord to them the merit of much thoughtful reflection on, and accurate observation of, the phenomena of the disease in question. We shall confine ourselves, in the following notice, to a few remarks on some of the more practical observations which have emanated from them in the discussion of the subject.

We coincide with Dr Gairdner in all that he has said of the obscurity and frequency of the disease, and have often had occasion to regret the complete want of anything like uniform notions among medical men on these points. The large proportion of practitioners see gout only when it affects the foot; some are willing to acknowledge its presence in the fingers and in the larger joints; but comparatively few are willing to recognise it without a local manifestation; and can witness and treat numerous disorders of the system, and many acute and chronic affections of internal organs, without the thought ever having crossed their minds, that, in all the phenomena which are daily passing before their eyes there is really nothing more than the features of gout, however mysterious and distracting they may appear. The careful perusal of Dr Gairdner's book will lead many to new and correct views on this subject; and we feel convinced that the study of the disease will compel many to say with him,

> "I believe the gouty diathesis is often very perfectly developed in individuals who never see its local manifestations, and I am convinced that the strumous is not more frequent than the gouty habit." —P. 4.

We cannot agree with our author in his objection to the commonly received opinion, that dyspeptic symptoms usually precede the fit of gout. Our experience amply confirms the doctrine which has long been taught, and we are somewhat at a loss to reconcile the statements in the following paragraphs.

> "All definitions of gout affirm that its external manifestations are preceded by dyspeptic affections of the stomach. This, however, is chiefly true of subsequent attacks of the established disease, and by no means applies to its first invasion, which, undoubtedly, often happens without any preceding dyspeptic symptoms. Persons, indeed, in whom the first stage of digestion is sound and vigorous, and in whom the assimilating process is also complete and apparently healthy, are singularly liable to gout. The earliest sign of an approaching fit of the gout, to which my attention has been drawn, has

> been a dull pain in the left side of the chest, accompanied by an inability to lie on that side, and sometimes by fluttering, irregularity, or intermission in the action of the heart. These symptoms often continue for a great length of time without any perceptible increase. Patients not apt to take alarm about themselves frequently suffer them in silence, and even forget them."—P. 8.

Now, we ask what is the nature of this "earliest sign"—this cardiac disorder? The answer from many quarters will be, that it is symptomatic of stomach derangement; and acknowledging that it is a very constant and a very early symptom of gout, we have never known it but in connection with one of the most common signs of dyspepsia, namely, flatulent distension. But we will allow Dr Gairdner to answer the question, which, we think, he does in the paragraph immediately following that just quoted.

> "These are very often the only precursory symptoms of gout, and they are sometimes relieved by a single dose of medicine, or a gentle course of physic, before they proceed to an actual paroxysm. A visit to any aperient mineral spring, or even a fit of spontaneous diarrhœa will remove them. But if such signs of disturbed health be neglected, they increase so as to become distressing, giving rise to very inordinate action of the heart, to great throbbing and palpitation, which seriously interfere with comfort and well-being. This inordinate pulsation often makes itself very inconveniently felt in the head, making stooping, hurried walking, and running, distressing or impossible. A difficulty of respiration and feeling of stifling sometimes accompany the marks of disturbed circulation. To these are added tumefaction of the right hypochondrium, impeded action of the liver, and morbid deposits in the urine. In the train of the above symptoms is found dyspepsia, which does not in general show itself in the first instance." —P. 9.

We are at one with Dr Gairdner in regard to the train of symptoms. We differ with him only in respect to their cause. He, if we do not misapprehend him, looks upon the "disturbed circulation" as the primary disorder, while we regard the disorder of the digestive and assimilating functions, as first in place, and explanatory of all the subsequent phenomena.

The symptoms next in order of time, and the anomalous morbid indications which fortell the coming fit, are well described; and the importance of noting them is very properly dwelt upon, as the best season for employing remedies.

The second and third chapters of Dr Gairdner's work, embracing the species of gout, the stages of the disease, its duration and termination, its hereditary nature, and the subject of metastasis, are treated of at some length, and deserve a careful perusal. In speaking of the atonic gout, we find our author noting a marked distinction betwixt it and the regular form of the disease in its mode of attack,—a distinction which we are unable to verify, and altogether at a loss to explain.

> "The atonic gout rarely manifests itself in its incipient stage, like the regular disease, by disorder of the circulation. It first shows itself by affection of the stomach, every form of dyspepsia is present, but particularly gastrodynia and flatulency. The bowels are confined, and this state of constipation, if neglected, according to a doctrine now in fashion, is relieved by occasional disturbance and diarrhœa. The urine is sometimes loaded with urates which appear and disappear with the greater or less pressure of the complaint."—P. 57.

The nervous symptoms in connection with this form of the disease are well described. The mental suffering and bodily distress are depicted and illustrated with great ability. We are surprised that no notice is taken of the very frequent, and often persistent, deposits of the oxalate of lime in the urine of those labouring under this form of disease. The phosphates are frequently referred to, and when the nervous symptoms are very prominent their presence might be expected; but we have much oftener found the crystals of the oxalate of lime, notwithstanding the silence of most writers on the subject.

The theory of a morbific matter—a *materies morbi*—in gout, long the favourite doctrine of the schools, and now recently revived through the support given to it by the advance of organic chemistry, and adopted, to some extent at least, by Holland, and Todd, and Garrod, and others, receives no countenance from Dr Gairdner, who appears to us to combat the theory all the more willingly, though not more successfully, that it has followed by a natural and strong impulse, from the fact of the very frequent accompaniment of symptoms of deranged digestion with the manifestations of gout; a relation, however, which he regards more in the light of a consequence and a satellite, than a cause. We must again enter our caveat against the following argument:—

> "If it be considered how often the disease (gout) exists in its severest forms, with little, if any, sign of dyspepsia, and that in its first invasion, when the real nature of the complaint may be supposed to be most clearly indicated, and its accidental complications to be less manifest, the signs of depraved digestion are nearly always absent, it must be admitted that some other cause should be sought for."—P. 85.

We have never found the first invasion of gout with the absence of the signs of depraved digestion. The sufferer has always acknowledged that, immediately before, or at no remote time, there had been marked indications of indigestion and mal-assimilation; and we cannot record an instance of the want of such manifestations during the progress of the disease. The hereditary proneness to the disease may be great, and the gouty diathesis well developed, when a slight excess, or a slight attack of indigestion will prove the immediate cause of a fit. We have seen the children of gouty ancestors, apparently in perfect health, suddenly seized with well-marked gout; but upon inquiry, we have always found that previously to the attack, and often but a short time before its visit, there had been distinct evidences of deranged digestion, in the unhealthy alvine evacuations, and loaded urinary secretion, while pallor of the countenance, and other signs of want of health, had betrayed the real condition. We cannot regard gout in any other light than that of a disordered state of the assimilating functions, the products of which vitiate the blood, which again, under certain circumstances, manifests an affinity for particular textures, and influences various functions and different organs. Dr Gairdner allows that the blood is vitiated, and loaded with urea and its compounds. Surely this is an admission of the doctrine of a morbific matter, though we are not yet in a position to demonstrate what the *materies morbi* really is.

In chapter v., we have some original observations on the altered relations of urea and uric acid, which take place in the early assimilation of the food during the process of respiration; and some valuable deductions (if time confirms the facts), both in relation to the pathology and treatment of the disease. We can only recommend the entire chapter to the careful consideration of our readers, as well as that immediately following, and which appears for the first time in this new edition of the work. It relates, Dr Gairdner informs us in his preface, to a subject of the keenest interest—the connection of a morbid condition with a normal and physiological function.

> "The chemical investigation of sugar in urine (he writes) seems to have been nearly completed by the inquiries of late years. Yet there is one corner of the subject nearly untouched. We are sufficiently acquainted with the qualities of urine in which sugar exists in quantity great enough to constitute a specific disease. But the attention of pathologists has not been directed to small or saccharine impregnations which indicate disturbances in circulating fluids, yet do not amount to any diabetic tendency. That such do occur in many conditions of the system, had long been matter of suspicion with me; and many circumstances concurred to impress on me the conviction that they attend various phases of gout."—P. 127.

Among these concurring circumstances, we wonder if the very frequent presence of the oxalate of lime in the urine of the gouty, and its known relation to sugar, had any share. Dr Gairdner inclines to the opinion, that sugar in minute quantity is a constant ingredient of healthy urine, and that diabetes is but a great exaggeration of a salutary function. Dr Gairdner has elsewhere acknowledged that uric acid is a necessary and constant ingredient of pure and healthy blood. Why should it be pronounced unphilosophical to conclude that gout is, in like manner, a greater exaggeration of a similar function?

The whole chapter, containing some interesting remarks on Trommer's test, and a new modification of it, as one to be relied on, is deserving of careful perusal.

Dr Gairdner devotes two chapters of his work to the consideration of the nature of gout—discussing the question of what gout is not, and of what it is. We apprehend that our author, in order to support his favourite view of languor and disorder of the circulation and visceral congestion in connection with gout, has too readily arrived at the conviction, that "its nature is the very reverse of inflammatory." We by no means contend that inflammation is essential to gout; but we cannot shut our eyes to the very frequent occurrence of the distinguishing phenomena of inflammation with the ordinary local manifestations of gout, and the not unfrequent alliance of inflammatory affections of the serous and mucous membranes, and of various internal organs, with the well marked gouty diathesis. The case of the gentleman detailed at page 140, is interesting and instructive. It is given somewhat, we think, in triumph by Dr Gardiner as an *instantia crucis* that the condition we are considering is not one essential to gout. The severe pain in the ball of the toe, in the case referred to, he argues, could not be of the nature of inflammation; for the conditions necessary to constitute such could assuredly neither be so rapidly created, nor so speedily dispersed. We ask, is neuralgia never an inflammatory affection? We know that in the gouty habit, sciatica, toothache, and various forms of tic-douloureux often alternate with, or take the place of the more ordinary manifestations of pain, heat, swelling, and discoloration; and so, in the case alluded to, we learn that in three weeks after a second attack of violent pain of the same character, but in the opposite toe, "the disease caught the patient in the heel, and he went through a severe fit of the disorder, in which no symptom was wanting." The condition of the system, we apprehend, in the three attacks was the same; the delicate nervous fibre, or its investment, was the seat of gouty inflammation in the first attacks, the tendinous sheaths and the skin were its resting-place in the subsequent seizure.

The gout, we are next told, seldom declares itself at the period of life when men are most prone to inflammation, but infests them in their decline, when the system assumes the congested, more asthenic, and lingering forms of disease. If by this it is intended to prove that the gouty diathesis is developed, generally, in the decline of life, we must demur to its reception. No doubt the large proportion of the sufferers from gout are men who have passed the prime of life; but in them the disease is more commonly seen in its chronic form, while in its acute stages, and in association with inflammatory affections of various parts, it had previously exhibited itself in connection with earlier years and unbroken health; the fit being then at longer intervals, and of shorter duration.

We cannot coincide with our author in the opinion he expresses that "people of a vigorous constitution are little liable to gout, and only become so when repeated and long-continued excess has enfeebled their frame and wasted their energy." Our own experience of the purest forms of gout has been among those who really possessed the least impaired constitution, and who were remarkable for freedom from all such excess; when we have seen it in different circumstances, as no doubt we often have, it has been when, through the long-continued prevalence of the diathesis, we could connect the enfeebled frame and wasted energies, not so much with the causes to which Dr Gairdner has referred, as with the deteriorating influence of the disease itself. Neither can we support the statement that the terminations of gout, and its vicarious and cognate diseases, also mark its nature, in the sense entertained by him. "They are," he says, "suppression of the natural evacuations, spasm, cramp, dyspepsia, melancholy, apoplexy, and dropsy." We desiderate among the vicarious and cognate diseases all notice of such inflammatory affections as iritis, otitis, tonsillitis, pericarditis, bronchitis, synovitis, and a host of other diseases, universally regarded as of inflammatory origin; while we must regard the apoplexies and dropsies, and other terminations of the disease, as the result of a disorganizing process established and carried on in the internal viscera by slow inflammatory action.

Having arrived, as we think, not altogether in a satisfactory manner, at what gout is not, Dr Gairdner is prepared for the much more difficult and subtle inquiry of what it is.

"There is no doubt," he says, "that a general state of vascular plethora of the great chylopoietic organs is always met with in gout."—P. 154.

"It is plain that the heart is oppressed with a flood of returning venous blood, and this, I think, associated also with the impure condition of this fluid, from the non-elimination of urea and urates, and probably of the biliary constituents, is the cause of those symptoms of disordered function of this organ, which I have pointed out as the earliest indication of the disease." "Venous congestion then," he goes on, "I consider the first condition essential to the formation of the gouty diathesis. . . . . . But the great venous canals of the body, as well as the larger arterial vessels, are endowed with a resiliency which enables them to struggle well against the flood of returning blood. This fluid, then, is compressed between two opposing forces; that, namely, which is derived from the heart and arterial system, urging it forward on its course; and, on the other hand, the antagonistic resistance of the great veins leading to the right auricle. Under this compression I believe that the vessels give way, and a true hemorrhage is occasioned in the part affected. If the rupture take place in a minute capillary carrying the serous portion of the blood only, œdema is the consequence; but if the burst vessel be one carrying red blood, a true ecchymosis is formed."—Pp. 157, 158.

"This view of a fit of gout," adds Dr Gairdner, "may startle from its novelty; but I am thoroughly convinced, from long observation of the disease, that I have given the true rationale."

Now, we readily admit that a general state of vascular plethora of the great chylopoietic organs is usually, but not always, met with in gout; while we cannot help thinking that this condition is the result of the gouty diathesis, and by no means the cause of the fit; for, not to instance the case of children and very young people, the offspring of gouty parents who have manifested unequivocal signs of gout without any of the evidences of venous congestion, we have repeatedly seen gout in its purest forms in females as well as in males of adult age, especially in the earliest manifestations of the diathesis, without any well-marked evidence of venous congestion, or of disturbed circulation. No doubt as the diathesis prevails, and the gouty habit is more and more developed, these signs of labouring circulation show themselves. We have a distinct and painful recollection of our own case. We had been long dyspeptic, and had suffered especially from acidity and flatulence. The action of the heart had often raised apprehensions of serious disorder in that organ, and the liver and kidney had manifested, through their different secretions, decided proofs of functional derangement; but we can call to mind no symptoms of venous congestion at that stage of our history. The disorder of the central organ was evidently connected with the process of digestion, was temporary, short-lived, and uncertain, and at a time when we were free from all irregularity, and busily pursuing our professional duties, we were seized with our first attack of gout. It was not till after repeated fits, that the symptoms of disordered circulation, so well described by Dr Gairdner, made their appearance; and it is right that we should state that they were gradual in their development, and that there was no correspondence between the evidences of abdominal congestion and the disturbance of the heart's action.

The very frequent occurrence of all the constitutional symptoms which attend a fit of gout, in connection with an inflammatory affection of a serous, mucous, or synovial membrane, or with some neuralgic attack or other disorder, and the subsidence of such seizure, along with the phenomena which attend the close of a paroxysm of gout, as well as the history of œdema, and the observation of ecchymosis, especially as we see it in the delicate texture of the eye-ball, forbid us entertaining the views of Dr Gairdner on this part of the subject.

The concluding chapters of the work—on the existing causes, the curability, and treatment of gout—deserve a careful and attentive perusal. The observations they contain are founded on the experience of a physician of a deeply thoughtful and inquiring mind, and merit the earnest consideration of all who desire to treat with skill and success the various forms of this obscure but still manageable disorder.

The unpretending volume of Mr Wells also deserves, and will repay, an attentive perusal. It is thoroughly practical; and founded on views which commend themselves to our acceptance. We are not sure, however, that we can adopt his opinions in regard to the subject of gout, as modified by syphilis. The laws which regulate the latter poison are, as yet, but imperfectly understood; and the complications of the disease resulting from the operation of two subtle poisons must, in our present state of knowledge, be a subject of extreme difficulty. There is something novel in Mr Wells's views, and something very interesting in the observations he has made in regard to the effects of the iodide of potassium on these and other complications. We look forward with pleasure to a new and enlarged edition of Mr Wells's little volume.

ART. V.—*Six Lectures on the Pathology of Strabismus, and its Treatment by Operation.* By C. HOLTHOUSE, F.R.C.S.E. London, 1854.

Few surgeons of a dozen years' standing can have forgot the unusual amount of authorship which followed the introduction of the operation for strabismus. What was written so much exceeded what was known, that ten years ago the profession became thoroughly tired of the whole subject, and allowed it to pass silently into the ordeal of time and experience. The operation having now been quietly in the hands of the surgeon for a sufficient length of time, we are desirous of learning the conclusions to which surgeons of experience and judgment have come, and therefore welcome the appearance of the treatise before us.

Mr Holthouse informs us that he had refrained from laying the results of his early experience before the profession, as so many writers were then in the field, but had his attention recalled to the subject last year by noticing a protruding eye as the result of the operation, performed a year and a half before, and he resolved to embody his observations in a short course of lectures. He thinks "that the principles which should guide us in the selection of our cases, is even now not clearly understood by the profession; and states that his principal object is to point out "the description of cases that will be benefited by the operation, together with the proper mode of performing it."

We regret that Mr Holthouse has given his views in the form of lectures. The style required for this is so different from that in which the profession wish to learn what new light a writer has to throw on an old subject. We wish him to come to the point at once, and say what he means in as short a space and as plain terms as possible. But we regret still more that the ample space of the pages before us is chiefly devoted somewhat theoretically to the physiology and pathology of the subject, in which, besides, we meet with little or nothing new or important; whilst the practical points—those of interest to the patient and to the surgeon, as a surgeon, are in some cases but lightly touched, and in others unnoticed. We meet here with what is far too frequently met in the works and lectures of the younger members of the profession, the want of a broad and clear distinction between what is important to the patient, and what is interesting only in a scientific point of view. We do not desire to see any separation between theory and practice, between elementary science and its practical application, between the physiologist or pathologist and the physician, between the anatomist and the surgeon, between the teacher and the practitioner; but we do desire and long to see more frequently a due appreciation of that distinction which is best defined as between that which is important to the patient, and that which is interesting only to his scientific doctor.

We return, however, from this digression, less appropriate, indeed, in a surgical than a medical review, to Mr Holthouse and strabismus, and shall follow him in the more practical and important points of the subject.

These are—in what cases should the operation be performed, and at what age?—what effect has it on the appearance, and what on the sight of the eye? Do failures and relapses occur, or is it followed by unpleasant prominence?—and what is the best method of performing the operation, with the view of obviating its ill results and securing the benefits?

The cases regarded by Mr Holthouse as not suitable for operation are—where central opacities of the cornea render the eye more useful squinting than straight—when the distorted eye is affected with amaurosis or cataract—when the distortion is owing to paralysis of the opposite muscle, as in palsy of the third and sixth nerves—and when the affected eye is much more prominent, or larger looking, than the other; a condition which, according to Mr Holthouse, is not uncommon, and predisposes to the occurrence of unnatural protrusion of the eye after operation.

The cases in which he recommends the operation are "all those which are not included in the exceptional ones just pointed out."

Whatever difference of opinion may exist on the question of the cases suitable for operation, there can, we think, be none as to the propriety of the exceptions made by Mr Holthouse; and we are surprised that he should, on such grounds, charge the profession with not clearly understanding the "principles which should guide us in the selection of our cases." The exception of the cases of the eye being apparently much larger than the other, if such cases do occur, may be peculiar to Mr Holthouse, but he surely does not mean to say that any intelligent surgeon would operate in the other exceptional cases mentioned.

There are still some, and probably not a few, who differ from Mr Holthouse in thinking these "all other cases" suitable for operation. There seems to be a common impression that it is a difficult matter to decide whether a case is suitable for operation or not. This may perhaps be the result of the complicated theories by which the subject was originally involved in mystery; but whatever the reason of it, we must say that those who entertain these doubts appear to do so "without knowledge," and do not appear to have been able to state them clearly, or to lay down any principle of distinction between the suitable and unsuitable cases.

We agree entirely with Mr Holthouse that all the cases not excepted by him are suitable for operation, adding only, that the strabismus must have lasted a sufficient length of time in the child to have become permanent. We would rather say that the operation is suitable in all permanent cases of strabismus proper, reckoning the exceptional cases as really not cases of strabismus at all. It is the custom, however, to apply the term to all cases of distorted eye, irrespective of its condition or of the cause, and we would therefore say that the operation is applicable to all cases of *simple strabismus*, by which we understand all cases not complicated, whether as cause or coincidence, with disease of any part of the eye, or loss of power of the opposite muscle in palsy of the sixth or third nerves. The cornea and humours are clear, the iris bright and lively, the sight likely so imperfect as, before the operation, to be of no use; the opposite muscle has the power of pulling the eye for the moment out of the squinting position, and the squint of not less than a year or two's standing; and if so, as in nineteen cases out of every twenty, the case is, we say, suitable for operation.

It may be supposed that there is a difficulty in distinguishing between cases of "simple strabismus" and those due to paralysis of the external rectus, but in reality there is none. In the everyday case, the eye can be brought straight, or even a little everted, the other eye moving with it, so as to take on the appearance of squinting for the moment; but, in palsy of the sixth nerve, the eye is away into the inner canthus, and remains fixedly there; and in palsy of the third nerve, when the surgeon lifts the fallen eyelid, he sees a somewhat everted eye, which can be moved more outwards, but in no other direction, besides a fixed and somewhat dilated pupil. Farther, their histories are so different; your patient with the paralysis is an adult, perhaps with a palsied limb besides, and his eye was straight till lately. But the everyday case of simple strabismus has the same story to tell; it began in early childhood, and has continued just the same ever since.

In excepting cases of amaurosis, it must be understood that we except only a few cases, which are easily distinguished by the symptoms and history. They are usually divergent, the distortion following the blindness, and coming on in adult life. It must not be supposed that we here except all cases where the function of the retina, or at least the visual power of the eye, is impaired, as we shall see immediately that this is the normal condition of a squinting eye.

The *state of vision* in strabismus, and the effect of the operation thereon, are points of much importance. We are probably correct in saying that many are under the impression still, that an imperfect state of the sight of the squinting eye contraindicates the operation, supposing it to be the cause of the distortion, and, besides, that the operation not unfrequently injures the sight.

Mr Holthouse very properly maintains that the imperfect vision is the result, not the cause of the distortion, and that it is usually much improved by the operation. Many seem not to be aware that the impaired vision is the normal or common condition of a squinting eye. Mr Holthouse is fully aware of this, and observes that the degree of imperfection varies in different cases. Sometimes, he observes, the sight is double or confused, as is probably the case at the commencement of it in childhood in every case, but that these cases are rather exceptional; most commonly the squinting eye is simply useless, and does not interfere with the exercise of the other. This refers to the patient's ordinary condition, when he is apparently using both eyes; but, on trying the squinting eye, the other being closed, upon different sizes of print, the surgeon will discover how very imperfect the sight of it is, and this will enable him to judge afterwards of the degree of improvement. Mr Holthouse admits that it may in some cases be otherwise, that the imperfect vision may have preceded the distortion, or have arisen from the same cause acting on the optic nervous apparatus at the same time that it acted on the muscles, but candidly confesses that he cannot explain satisfactorily how mere impaired vision should cause strabismus, and observes that the everyday case of long-standing cataract has no such effect. But the fact is, we do not know absolutely if impaired vision ever produced strabismus, we merely infer it, although the inference is not a necessary one, from finding a few cases in which the sight does not improve after the operation. In the common case of strabismus, the distortion comes on in early childhood, when we have no means of knowing the state of the eye, but we find as soon as we can know, that the sight is impaired, and farther, that after the operation it is generally very much improved and often entirely restored, and therefore it is but fair to conclude that the impaired sight is the result not the cause of the strabismus.

Whether it be so does not, however, alter the fact of importance to the patient that the operation is generally followed by great improvement to the function as well as the appearance of the eye. It may be doubted whether this fact is commonly understood, nay the opposite belief seems to be held by some. There is a very common impression to this effect out of the profession. We are constantly asked "but will it not hurt the sight?" and our assurance to the contrary is usually productive of a look of incredulity. Some years ago a medical friend, then in extensive ophthalmic practice, told us that he had given up the operation, it often failed, and when it did succeed, the sight was injured. We need not therefore be surprised that the public should still retain these impressions. The impression that the sight is injured has arisen we believe from the double vision which follows the operation, and lasts for a short time until the eye is accustomed to be moved with the other by the re-united muscle. The patient is seen perhaps only for a few days, during which he is loud in complaint about often seeing double, and thus conveys to the unthinking the impression that "the sight is hurt." This, however, is a good sign, it shows that the eye retains its visual power, and the double vision results from its inability for a time to move in concert with its fellow in the direction of its divided muscle. Accordingly, it occurs in looking to the side which requires the use of the divided muscle, but not to the other, as the surgeon may easily satisfy himself by holding up his finger alternately to each side of the patient, who will declare that he sees two fingers on the side away from the cut eye, and perhaps also when it is held straight in front.

We pause here for a little to notice Mr Holthouse's theory, or opinion, regarding the cause of the impaired vision. It is commonly attributed to disuse. The age of the child prevents us being so certain of this as to state it as a fact, but we suppose the commencement of the strabismus to be accompanied by double vision, just as when we push one eye aside; that the image from the mal-directed eye must moreover be imperfect besides confusing the other; that, as a matter of time and habit, the patient ceases to pay attention to the impression from the squinting eye; and that, by this mere disuse, the function of the retina becomes and remains impaired, until placed by the operation in favourable circumstances for renewal. It is moreover supposed that especially the sensitive spot of the retina (so called merely as an inference from the fact that we always use this part for accurate vision, by turning towards the object the axis of the eye, which passes through the centre of the refracting media, not for any other reason, as far as we know) is thrown out of use, as the impression now falls internal to it; and that, after the operation, and the reunion of the divided muscle, this part is again used, but requires some use and time before it regains its former acuteness of function; and that this corresponds to, or to the theory used to explain, the gradual or rapid improvement of the sight which follows the operation.

But Mr Holthouse believes the impairment to be "in the dioptric parts of the eye, which have undergone some change of form, or tension, by muscular action." With regard to the theory of disuse causing impairment of the function of the retina, he observes, that no such effect follows the existence of a cataract of many years' standing although the function or use of the retina has been all but suspended, an argument, it must be admitted, of some force towards rendering it probable that the impairment is not essentially connected with the nervous apparatus. That it is due to the refracting media, he infers from the improvement effected by the operation, which he states is not gradual but *immediate.* Our ophthalmic brethren will probably participate in the surprise with which we read this statement. Mr Holthouse maintains positively that the great improvement of the vision occurs not gradually, not merely rapidly, but immediately; that nearly all the improvement which occurs takes place immediately, and comparatively little afterwards. It is possible that we and others may have overlooked this. The eye is usually kept shaded for some days, and our patients may have deceived themselves and us in reporting a gradual improvement of the sight; and the fact, if it be one, is very apt to remain unascertained unless the power of the eye before and after operation had been carefully tested upon different sizes of print. But we are surprised to find Mr Holthouse reporting this immediate improvement and not referring to the double vision which we have found to be so much complained of by the patient, especially for a few days after the operation. This we invariably explain to the patient, showing him how it occurs to one side and not to the other, to allay his apprehension that "the sight has been hurt."

We do not mention this as incompatible with immediate improvement of the power of the cut eye separately from its fellow, but are surprised that Mr Holthouse should have overlooked this and not the other.[1]

Supposing it to be true, as a matter of fact, that the improvement is immediate, it does not, we think, therefore follow, that the impairment is due to the refracting media being altered as to form or tension by muscular action. We can easily understand why and how there should be imperfect vision when the light has to go through one side instead of the centre of the media, and be little better than the sight we have of a person standing at our side when we are looking at one in front of us. But except on this principle we do not see how muscular action, or what muscular, could produce such an effect. It has no doubt long been supposed that the muscles of the eye had some influence on its form and focus, but opposite views have been held as to which muscles would produce this change of focus, whilst all the time it was overlooked, that to have given the muscles in general, or any one or two in particular, this power, of course as a secondary use, would have been a very unfortunate thing for us, as leaving the focus of the eye at the mercy of the said muscles when primarily used for moving the organ about, when, at the same time, no change of focus was desired.

The cause of the impaired vision in an eye affected with strabismus must be held, therefore, as not yet explained, if indeed explainable, but amidst these doubts and theories regarding disused retina, displaced refracting media and compressing muscles, it is satisfactory to be able to fall back upon the facts, of interest to the patient, that the impairment of vision does not contraindicate the operation, and that he may expect considerable improvement of it, either ere long after the operation, or if immediately so much the better.

In taking leave of this part of the subject we would suggest to Mr Holthouse whether it would not be better for all teachers and writers at once to abandon the suppositive.

---

[1] Since the above was written, we have had a favourable opportunity of ascertaining the exact effect of the operation on the sight. The result confirms Mr Holthouse's statement. The patient, æt. 19, could not make out the type on the above page with the right eye, except very close to the eye, and with the aid of a convex glass, and then but indistinctly. Immediately after the operation he could read it easily at twelve inches distance, and nearly as well as with the other eye. There is the usual temporary double vision to the left side.

There is no doubt it would be for the benefit of science, and would also, we maintain, contribute to the personal benefit and comfort of each individual writer and teacher. What a relief it would be if we could get quit of suppositions! Our works in two volumes could come out in the next edition in one, we could give our students their holidays without a grudge, and effect a large reduction in the periodical department.

If we could confine ourselves to *facts*, not omitting those against us as well as for us; and, having got a fair statement of the premises, then draw our *inferences*, and these might be stated in their various degrees of strength—certain—probable or fair—possible but doubtful, and so on—always, however, stated as inferences in which others could agree with us, and not as facts; and when it is really impossible to draw any conclusion, let us at once get quit of the difficulty by saying so. Let our opinions be inferences, and the grounds thereof assigned; and if new facts show that our previous inference was wrong let us be ready at once to give it up for the new one, or for none at all if the facts do not warrant any.

Had space permitted us we might have illustrated these views from various parts of the work before us, and had noted the remarks and conclusions on the altered state of vision from the action of belladonna for this purpose, but must forbear. We are especially anxious to have Mr Holthouse as a convert to these views, as he promises other two treatises in the course of this one; and in the one which is to relate to the physiology of vision, he will find ample scope to carry them out.

Mr Holthouse believes that the operation sometimes fails, and also that relapses sometimes occur; "the eye may remain inverted after division of its adductor muscle; or the squint, though removed at the time, may return some days or weeks afterwards; or again, the previously straight eye may become affected." The inversion remaining after the operation, he has little doubt, is due usually to a portion of the muscle being left uncut, that is to say, to the operation being left unfinished. But he thinks the inversion may be kept up by some adhesions of the so-called ocular fascia, or remain in consequence of a weak condition of the external rectus. He divides only the internal rectus, reprobating strongly the practice of dividing part of the upper and lower recti even when the inversion still remains. In this we entirely agree with him. He would bind up the sound eye, trusting to the squinting eye eventually righting itself, but if it does not do so within eight or ten days he would then operate on the other eye.

The *relapses*—are the cases in which the inversion was removed at the time of the operation, but returned afterwards. It may be due, Mr Holthouse thinks, to the cicatrization of the wound, to the muscle readhering to the globe too near its original attachment; or, according to a strange theory of Mr Radcliffe Hall's, it may be due to a hypertrophied condition of the internal rectus of the opposite and non-squinting eye. In the event of a relapse Mr Holthouse would, as far as we can gather from his remarks, operate again on the same eye. We have never seen a relapse in the strict sense of the term, and believe the great majority of the cases of so-called relapse to be merely cases of failure from the non-division of part of the tendon. In a true case of relapse, we would deem it a matter for the surgeon's determination, from the circumstances of the case, whether to operate on the other eye or again upon the same one.

As to the occurrence of *failures* we are still more sceptical. By failure we mean that the inversion still continues. That it often does so we admit, but that it ever should we do not admit. A portion of the tendon is left undivided and it is unfair to call this a failure; it may be a failure on the the part of the operator, but is certainly not a failure of the operation; the operation is simply unfinished. This is well known, all writers and teachers speak of it; but yet it continues to be common; a portion of the tendon has been left, and another case is added to the list of failures or relapses, and to the discredit of the operation. We speak strongly on this point, but from what we have repeatedly seen. The inversion was plainly not destroyed at the time; or, in cases where we did not see the first operation, we have found on repeating it even the entire tendon to have been undivided. We do not wish to be understood as making any charge of incompetency against the operators in such cases. It happens in the best hands occasionally. Particular care is required to see that the whole tendon is divided. Even with the use of the blunt hook, a slip of tendon may be omitted unless care be taken to pass it gently once or twice to see that no such slip remains, for even a blunt hook slips easily enough in between the parallel fibres of the tendon. We have seen cases in which even the entire tendon had been left. In one of these we recollect only to have learned afterwards of the first operation having been performed, the patient having concealed it for some reason; but there was nothing unusual about the eye or tendon to lead to the discovery. In another the operation had been performed on both eyes without any benefit. We repeated it on the most squinting eye, and found the tendon quite natural. This was enough, and when the patient happened to come to the dispensary six years afterwards, the pupils had some difficulty in saying which eye had been cut. In another the operation had been performed three times, at short intervals, on the same eye, but without any benefit whatever. We did not think of touching the other eye, but, without hesitation, performed the operation as if for the first time, and so it was; the tendon was natural, and when divided, now for the first time, the inversion was at once destroyed. Such cases no doubt present themselves to others not unfrequently, and we mention them to illustrate our practice under the circumstances.

We do not absolutely disbelieve the possibility of inversion remaining after the complete division of the internal rectus, as some writers maintain that they have met with it, but we have never seen it, and have seen so many where it was due to the muscle not being completely divided, that we must confess to entire scepticism as to its occurrence. We too have seen it remain after we thought the tendon was divided, but a second or third passage of the blunt hook has picked up the cause, a small portion of the tendon not divided, and when this was done the inversion was gone. Without this care, and, we are convinced, without our blunt hook, we also would have met with failures, and been believers in the permanence of inversion theory. We regard the loss of the inverting power, for the time, as the only test that the tendon has been completely divided. When the patient is made to look away with the other eye, the cut one moves a very little inwards from its now slightly everted, or, more usually, straight position, but only a little, as far as, we may suppose, the inner fibres of the upper and lower recti could help it. Here it suddenly stops and vibrates outwards again, but farther in than a very little inwards from the straight position, it cannot go. The operation is then complete. This is altogether different from the power of inversion which remains when a portion of the tendon is still undivided. The inversion is in this case still great, it may be as great as before, and cannot be confounded with the very little inward motion above referred to. Perhaps we are wrong in even expressing it as if there was any power of inversion at all, and should rather say that we see the eye try to move inwards but that it cannot.

We agree with Mr Holthouse that on no account is any other muscle to be divided, for convergent strabismus, than the internal rectus, or the external rectus for the divergent variety. This has long been settled as regards the oblique muscles. No benefit has ever been reported to have followed their division, except in two cases in which the inferior oblique was said to have been divided, but in one it was for convergent, in the other for divergent, strabismus. There may be some, however, who would still think of dividing part of the upper and lower recti, either in cases where there was a little upward or downward obliquity of the inward squint, or when the supposed complete division of the internal rectus had not destroyed the inversion. If the inner fibres of these two muscles naturally possessed the power of inverting the eye, they would always do so; but even those that maintain this view, will admit that, at least generally, the power of inversion is destroyed by division of the internal rectus, whilst we maintain that it always is so. We see then that they have no such power naturally, but that the internal rectus alone is the inverting muscle. We do not mean to deny that it would form a very rational theory to suppose that the inner fibres of these two muscles would become contracted by the inward position of the eye, but meet all such views by the fact that it is not so, that experience has shown the division of the internal rectus to be sufficient, providing it is complete.

The division of more than the internal rectus, Mr Holthouse observes will be followed by an outward squint and great protrusion of the eye, bringing the operation into disgrace. Even if the inversion were not destroyed, he would not touch the upper and lower recti. If the eye did not right itself in eight or ten days he would then perform the operation on the other eye, and has never seen eversion follow this double operation as he terms it.

We scarcely learn what are Mr Holthouse's views on a question formerly much spoken of—whether one or both eyes should be cut. It was formerly supposed that in what are called cases of double strabismus, which, according to the common and loose application of the term, constitute the majority, it was essential to operate on the other eye also, either at the same time or some days or weeks after. We once, eight or nine years ago, divided both internal recti at one sitting, but hope never to do so again. That boy has now certainly a diverging and prominent eye. We also, during the prevalence of the belief in the necessity of cutting both eyes, in several cases performed the operation, according to rule, some weeks afterwards on the other eye; but we soon found this to be totally unnecessary when the internal rectus of the first and worst eye had been completely divided, and would never think now of performing the operation on the other eye, even in cases of what are called double strabismus. What constitutes a single, and what a double strabismus it is not easy to define. Mr Holthouse defines a double strabismus as one in which both eyes squint equally and at the same time, rendering the deformity apparently less than in single strabismus, from the resemblance between the two eyes. On the other hand, most surgeons apply the term simply to a severe case. Cases of double strabismus, according to Mr Holthouse's definition, are rare, whilst, according to the other acceptation, they are very common, and, indeed, the only distinction then between the single and double forms is one of degree, for the other eye always squints more or less when the worst eye is for the moment brought to a straight position. Practically considered, the question is of little moment. One eye is worse than the other. This the patient calls the squinting eye, being surprised to hear that the other one squints also, and the surgeon easily finds by a proper mode of examination which eye is worst. The operation having been performed on this eye, the surgeon will see in a few weeks what the result is, and may then if necessary operate on the second eye. This, however, we think, will be found to be unnecessary, the common occasion for it has its origin in the unfinished state of the first operation; and such a case presenting itself the surgeon must consider whether it is best to cut the other eye or complete the operation on the first one. We have had cases where the distortion was very great, so much so that we warned the patient beforehand of the probability that an operation might be needed on the other eye, but the first proved sufficient. In the case of a double strabismus, as defined by Mr Holthouse, we presume it would matter little which eye was cut to begin with. It is possible that to this class of cases belonged one related to us by a friend, in which he discovered after the operation that he had cut the "wrong" eye, but it cured the strabismus very well.

We have often seen others in a difficulty to determine which eye was really the worst one, and various methods of doing so have been described. Our plan is this: Standing before the patient, shut one of his eyes with your finger, and make him look with the other eye at a finger of your other hand held up straight before him. Now quickly raise the lid of the closed eye and notice how much it squints. Then do the same with the other eye. One will be seen to be more inverted than the other; and that one the patient will readily tell you is the squinting eye. He knows quite well which it is, and is surprised at your slowness of comprehension; but in the case of children, the surgeon must trust to his own observation, and the above test is simple and decisive.

Mr Holthouse alludes to the interesting fact in connection with strabismus that it occurs in the lateral direction only. The hypotheses which he throws out in explanation of this are, as he is aware, merely hypotheses; the greater size of the lateral recti; their supply from separate nerves. Or, again, the greater frequency of the convergent variety, he suggests, may be due to the greater size of the internal rectus, or the greater use made of the internal than of the external recti, or the more inward than outward position of the upper and lower recti. These circumstances might account for all of us being the subject of strabismus, but scarcely, it would seem, for some of us being so and others not. We have always thought of the circumstance that it is in the lateral direction only that non-corresponding recti are employed, with their separate nerves; and, again, of the more frequent use made of the internal rectus, acting sometimes with its fellow and sometimes without; as possibly in some way connected with the occurrence of squint in the lateral direction only, and with the much greater frequency of the convergent variety, but by no means propose this as a satisfactory or even very intelligible explanation, resting content with the fact that it is so, and its application, to the effect that one muscle only requires division.

Mr Holthouse mentions that other forms of squint are occasionally seen, either directly upwards or downwards, and not unfrequently that in which a convergent strabismus has also some obliquity upwards or downwards. The latter may now and then be observed, but we have always found it completely removed by division of the internal rectus, which was evidently the only muscle concerned. We know that systematic writers allude to cases of upward and downward squint. We ask Mr Holthouse if he has ever seen any, and if so, under what circumstances. A moderate degree of ptosis looks, on the street, remarkably like an upward squint, but we doubt very much the occurrence of a simple strabismus in any but the lateral direction. There is, of course, nothing impossible to us in the idea of the upper or lower rectus becoming spasmodically contracted, or their branches from the third nerve happening to become involved in some neighbouring disease, and so paralysed. We once saw a case of upward squint. The eye occasionally or frequently moved up under the lid, but did not remain permanently there. Of course we advised the lad not to have an operation; there were other nervous symptoms, like chorea, and about a year afterwards he became deranged. This case was seen and mentioned to us by a surgeon as against our "theory" that strabismus is always lateral. It is, however, not a theory but a fact. There is no reason why it should be so, except that it is so; and if the history of surgery could furnish a few cases of simple strabismus in other directions, uncomplicated with disease, the exceptions would but serve the more to attract our notice to the rule.

In the *operation*, Mr Holthouse rather prefers to use the sharp hook for the purpose of fixing the eye. We agree with him that it is much preferable to the use of forceps with a similar view, which hold only by the conjunctiva. He has seen no evil result from the use of the sharp hook when it is well used. The difficulty, however, is to use it well. It is no easy matter for the assistant to fix this double hook into the sclerotica and keep the eye steadily out with it. If the patient is unsteady, or the assistant inexperienced, or if he omits to rest his hand on the temple, it is pretty sure to come out, and is not a safe instrument so near the cornea.

It is not absolutely necessary, and we have begun to dispense with it, especially since we have had the advantage of placing the patient under the influence of chloroform. Mr Holthouse thinks it of little moment whether a knife or scissors be used to divide the conjunctiva, but rather prefers the latter. He appears not to use forceps for pinching up the conjunctiva to facilitate and render safe its division with the scissors, the latter apparently, probe or blunt pointed, of course, being the only instrument he uses in the operation proper. With it he opens the conjunctiva a little below the level of the cornea, and one-third or one-fourth inch internal to it; divides down to the sclerotica; pushes up one blade below the tendon, and divides all between the blades, as high as the upper margin of the cornea, by a few sweeps. The use of a director and knife is mentioned as another plan. The blunt hook is also mentioned as an instrument which may be used.

Mr Holthouse's description of the operation is meagre and too general. We are surprised to find so little notice of the blunt hook for catching up the tendon. We would on no account agree to dispense with the well-known blunt hook, without it we believe there is no security that the tendon is all divided, and this result is very much facilitated by its use. We use forceps to pinch up a fold of the conjunctiva, and divide this with the blunt pointed scissors vertically over the breadth of the tendon. There may then be the subconjunctival cellular tissue to divide. We now lay aside the forceps, and slip the flat blunt hook gently in at the lower margin of the tendon, which may or may not be visible according to the amount of bleeding. The tendon is at once gathered on the hook; one blade of the scissors is now passed between the hook and the insertion of the tendon, and the latter divided as near its insertion as possible. The blunt hook we now pass gently upwards and downwards to assure ourselves that no slip of the tendon has been omitted. If it has, it is at once caught upon the hook and divided with the scissors.

The operator may generally be satisfied that all the tendon is divided by the liberation of the eye which immediately occurs, but the gentle use of the blunt hook should not be omitted. It is unnecessary to divide any loose bands of cellular tissue which the hook may bring up; if there is a piece of tendon, it is tight and catches the hook. It must not be supposed that we here recommend a rash use, or plunging movements, of the hook, or the passage of it away beyond the region of the tendon; but merely that it is to be gently slipped in below the tendon, and then once or twice afterwards in the situation of the tendon, to see that its upper or lower part has not been left. This is no uncommon occurrence even with the blunt hook. The fibres are parallel, and the hook very readily slips through the tendon. To make perfectly sure that the tendon has been all divided, we always make the patient try to invert, by directing him to look outwards with the other eye; and if any inverting power remains, we again seek gently with the blunt hook, catch a piece of the tendon, which is sure to be there, and on dividing this we have never found the inverting power to continue. This is the only inconvenience attending the use of chloroform; we require to wait some time till the patient is able to use his eyes as we tell him, but it is of great advantage to have the patient quiet during the operation. We are not attached to any particular bend of the blunt hook. A probe is a coarse instrument. The hook should be simply blunt and flat, and probably is best with a less abrupt bend than it is usually made with. We are quite sure that in some instances we would have left a portion of tendon but for the use of the blunt hook, and no surgeon who omits to use it need be surprised if sometimes the eye retains its inverting power, and if he finds afterwards that the operation has failed. We do not mean to aver that the operation may not be completed without using a hook to catch up the muscle, but merely that a portion of tendon is very apt to be overlooked without the use of the hook, and that it renders the whole proceeding more easy. The eye is subjected to less instrumental roughness; and by the use of it we may best attain the two objects of our operation—to divide the whole tendon, and as little more as we can.

Mr Holthouse unites the edges of the divided conjunctiva, in what exact manner he does not mention. This he says may secure union by the first intention, and if neglected, the wound granulates, a fungous mass forms, the conjunctiva retracts, the plica and caruncle disappear, and an unpleasant vacuity is left. He appears, however, not to have been very successful in securing this primary union, as he says these results so frequently follow that he has lately adopted the method by subconjunctival section. The granular mass which occasionally forms is easily removed by dividing the pedicle by which it is attached, and no harm is done. That there often remains a certain amount of vacuity at the seat of operation there is no doubt. This is especially the case when too much conjunctiva has been cut, or some of it even removed as has been done. But this deficiency is much less if the operation has been neatly done and in early life. It is usually not perceptible to the non-professional eye, and sometimes a close examination is necessary before even the surgeon can say which eye had been cut.

We regret that Mr Holthouse has said so little of what he has himself seen of the state of the eye some months or years after the operation. Cases are to be seen in which the eye is prominent and a little everted, and it is a question whether the patient would not have been better with his original squint. Such an eye wants the sinister appearance of the squint, but has on the whole an uglier look, perhaps only because we are less accustomed to see it than the squint. Some seem to have the erroneous impression that this is a common result of the operation. Although such eyes are to be seen not unfrequently at present, we must recollect that we have still with us the fruits of the first operations, many of which it is well known were most rashly performed, as well as often in unsuitable cases, and some surgeons were in the habit of cutting far too much conjunctiva, as well as sometimes part of other muscles besides the internal rectus. We believe that prominence and eversion of the eye will be in future still more rarely seen. We cannot promise that it will not occur, but may say that the event is so unlikely that the fear of it should not in the least deter the patient from undergoing the operation.

Independent of actual prominence and eversion, there is not unfrequently the vacuity already referred to, but still this is a vast improvement on the original look of the eye. These results are to be avoided by cutting as little as possible, consistent with complete division of the tendon. We think also the tendon should be divided as close to the eye as possible. We are never afraid for it adhering too near the original attachment. The fear is rather that it be too much shortened, and have too little power over the eye afterwards. Usually it has power enough to invert the eye until the nose comes in the way of further vision to that side. If it is cut too far back, through the muscle for instance, we think there is more risk of prominence and eversion, and of imperfect power of inversion.

Another question of importance Mr Holthouse seems to have altogether omitted—viz.—the age at which the operation should be performed in the case of children, who, by the bye, now form the great proportion of the cases on which we are consulted. There seems to be a prejudice against the operation in children, founded apparently on the supposition that the squint may disappear spontaneously, and also somehow, besides, that it will be better to leave it till they are adolescent. Whatever the various remote causes of the common simple strabismus may be, we know that it usually appears in very early childhood. It may last for hours or weeks, and then disappear, all the more rapidly if the irritation of the bowels or gums be attended to. But the usual history is that it began at that time—the first few years of life, most generally the earliest of these—and has continued so ever since. Now, we presume, no one would think of recommending the operation in a case of a few weeks or months standing, or until it had become permanent. We would not say it had become permanent till it lasted at least a year or two; but after this we believe the expectation of cure to be hopeless except by operation. We operated once in a case of a child as young as between two and three years, but would not recommend it so early. When the squint has lasted for years and the child has attained the age of six or seven we would certainly recommend the operation now as preferable. It restores the use of the eye, and there is likely to be less appearance of the operation when the child has grown up. The expectation that the squint may disappear at the period of puberty is, we believe, without any foundation in fact. We have indeed heard a case mentioned in support of this view, but it was not stated how long it had lasted. We ask whether any surgeon knows of an authenticated case in which a strabismus which he is sure had lasted for years, disappeared at this or any other period of life. If such cases did occur we would know of them, and it is surely a vain thing to encourage parents in the hope that their children will get quit, after years of it, of what no child has been known as yet to get quit of. When a squint has lasted for years, it is no longer, as apparently at first, a functional affection; and we might as well expect that period of life to be attended by a sudden down-coming of the tendo Achillis of a simple club foot as by a setting loose of the internal rectus of a distorted eye.

The question of age, on the other extreme, is less important; but if an adult wishes, there is no reason why the operation should not be performed. We have performed the operation at 40 with as good result as at any previous age, both as regards appearance and improvement of vision.

Mr Holthouse mentions that he is engaged with experiments to ascertain how far strabismus is curable without operation, and promises to detail the results on a future occasion. Considering the results of the endeavours to remove club foot without previous division of the tendons, we are not sanguine as to the result of his endeavours, seeing especially that the foot has this advantage over the eye that it may be laid hold of mechanically. When the squint is quite recent something might possibly be done were it not that the ordinary subjects of it are so young.

We noticed above that Mr Holthouse has lately had recourse to the operation by sub-conjunctival section. This our readers know to be an old operation, which, in this country at least, did not find much favour. The advantages are the absence afterwards of the vacuity arising from the division and retraction of the conjunctiva, and the less chance of prominence and eversion. He thinks it especially adapted for those cases in which the squinting eye appears larger than its fellow. He operates thus: An opening is first made in the conjunctiva, as in the other operation above described. Into this hole he passes a small bistouri-caché, and slides it up between the tendon and the eye. Now on touching the spring the blade advances, and by a slight sawing movement the tendon is divided, but not the conjunctiva over it.

The disadvantages of this operation, we presume, are the uncertainty of effecting complete division of the tendon, and that, in most hands, there is no saying what might happen to the eye during those manœuvres. If the operation was certain or safe, there is no doubt it would be preferable to the plan of free division of the conjunctiva over the tendon.

These remarks have now extended so far that our space will not permit us to follow Mr Holthouse in his anatomy and physiology of the muscles. The tendons of the recti muscles are very exactly described. It is surely unnecessary at the present day to print a lengthened refutation of Sir Charles Bell's views regarding the use of the oblique muscles. John Hunter was not the first to point out the use of the oblique muscles, and moreover his short paper on the subject is by no means of itself convincing that the use of the oblique muscles is to roll the eye on its axis. Mr Holthouse is apparently not aware of the principal evidence by which this view is supported, as a broad physiological fact, and we take the liberty of referring him to the Edinburgh Monthly Journal for 1849 and 1852. Mr Holthouse alludes to the "ocular fascia," the membrane of Tenon, the tunica vaginalis oculi, or subconjunctival fascia, of various authors. Moreover, he says (p. 7) that there are three layers of this fascia between the conjunctiva and sclerotica, all of which must be divided in the operation. As we perceive Mr Holthouse to be a lecturer on anatomy, we make bold to ask him if he ever saw a fascia here, and to confess that we have never been able to understand the descriptions given of it. That there is cellular tissue below the conjunctiva, and cellular tissue investing the muscles and eyeball, and cellular tissue reaching therefrom to the eyelids, cannot be doubted, but does it form a "fascia?" The term fascia is a well understood one in descriptive anatomy; and though we have dissected orbits enough, and been accustomed to look at things as they are, and apply the language of descriptive anatomy, it never occurred to us that there was a fascia here, nor have we yet seen any arrangement even of cellular tissue specially in the form described by some as the "ocular fascia." We wish anatomists would reconsider this fascia, and not admit its existence merely because some one had described it as existing, and affixed to it a name. Teachers of anatomy should take care not to multiply and confuse. It is this which tends to make anatomy wearisome. There is in it matter of real importance more than enough to fill a longer book than has yet been written, and a longer course of lectures than has yet been delivered; and one feels sorry both for the science and the teacher when a false importance is claimed for trifles.

We must apologize to the reader for the length to which our remarks on strabismus have extended. We had intended a much shorter notice of the work before us, but have been led on by our desire to clear an operation from some of the prejudices by which it is still by many regarded, and of which we may say in Mr Holthouse's words—"There is no operation in the whole range of surgery that is attended with more success or so little risk."

ART. VI.—*Highley's Library of Science and Art*, 1854. *Highley's Popular Medical Series*, 1854.

Mr Highley has commenced the publication of a series of works adapted for students in general, and medical students, we presume, in particular, under the general name of the "Library of Science and Art." The four which we have seen are all more or less closely related to medical studies, and are, on the whole, useful additions to our educational resources. The Botanical Letters of Unger,[1] and the work of Schacht on Vegetable Histology,[1] are both of them works which we are glad to see in an English dress. The same may be said of the work of Weber on Auscultation;[2] a brief and clear exposition of the doctrines of the "New, or German School" (as the author calls it, in contradistinction to the "old, or French school") of auscultators. We might, indeed, take exception to the sentences in which the author lays down the characteristics of these schools, as savouring of one-sidedness; perhaps also some other passages in the book lie under the same objection; but in the main, Dr Weber is no bigot, nor yet a schoolman; he is a thoughtful and accomplished physician, and writes with remarkable vigour and elegance, on a subject on which he is evidently well informed, both theoretically and practically. We should recommend this work to our students with little reserve, were it not that our suspicions have been aroused as to the accuracy with which the translator's part in it has been performed, by some very glaring instances of careless editing, which have been forced upon our attention in the cursory perusal of it. Thus, in p. 12, Dr W. is made to tell us that a *pelvis measurer* "has been made available for purposes of percussion;" and at the top of page 15 is a piece of nonsense, which, though we have not the original by us for comparison, we are very sure Dr Weber never wrote. Again, we find Kürschner misprinted Kurshner; Arantii, Arrantii; Chassaignac, Chaussignac; and one or two other manifest clerical errors or slips in translation, which render us distrustful as to the whole work. Worse still, we find in the illustrations introduced by the translator from the works of Bock and Piorry, several glaring faults, which a competent and careful editor would scarcely have passed over, even if they existed in the originals from which he took them. Thus in the frontispiece, the thorax is out of all proportion, being much too short and broad; the heart is too large in proportion to the thoracic cavity; the aorta appears to arise from the right ventricle instead of the left; the pulmonary artery, instead of concealing the origin of the aorta, seems to retire entirely behind it; and in a drawing professedly topographical, the heart and lungs are represented in a totally false relation to each other, as any student of six months' standing will at once perceive. We strongly recommend Mr Highley to cancel this plate immediately in the remaining copies. In fig. 4, the thorax is again out of proportion; its breadth being more than equal to its height; the substernal percussion dulness is greatly exaggerated, and the cardiac dull space passes too far upwards and to the left; the hepatic dull space is too great in the epigastrium, and too little laterally; the stomach is likewise in an unnatural position, and (unless the references are transposed) the diaphragmatic region is represented as extending into the epigastrium; finally, the spleen is wrongly placed, but this error is noticed in the text. In fig. 6, the difference of height of the two kidneys is exaggerated, and the splenic dulness is too large and absolute. In fig. 8, the middle lobe of the right lung is incorrectly represented as extending to the back part of the thoracic wall, and the reference to the urinary bladder is erroneously given. Neither is fig. 10 altogether satisfactory to our eye. The diagrams of the heart, from Bock, are ingenious and useful; but in one of them, fig. 13, the aorta is represented as concealed behind the vena cava superior, and the eye of the student is thus puzzled and misled. These things require only to be pointed out to justify our reserved expression of opinion on a work which we greatly wish we could more heartily recommend.

---

[1] Botanical Letters, by Dr F. Unger. Translated by Dr B. Paul.

---

[1] The Microscope in its special application to Vegetable Anatomy and Physiology. By Dr Hermann Schacht. Translated by Frederick Currey, Esq.

[2] Clinical Handbook of Auscultation and Percussion, &c. From the German of Weber. By John Cockle, A.M., M.D., F.R.C.S. (By the bye, *who* and *what* is this "Weber?" The translator has nowhere vouchsafed us his Christian name, or any information, in fact, about him.)

Dr Beale's work[1] on the Microscope requires nothing more from us than a welcome. It is well-adapted to the wants of clinical students, and we trust it will soon find its way into their hands. It is a good book, well conceived, and, so far as we have dipped into it, well executed. It was also much wanted, and is moderate in price, considering the large number of illustrations. The analysis of the work is best performed in a word, by saying, that it contains everything that is indicated by the title.

We cannot say much for the single specimen which has reached us of the "Popular Medical Series." The public requires something more instructive, or, at all events, more interesting, than the well-worn rags of physiology and pathology which Dr Hartwig has patched up into a "Practical Treatise on Sea-bathing and Sea-air."

---

[1] The Microscope and its Application to Clinical Medicine. By Lionel Beale, M.B., London, Professor of Physiology and General Anatomy in King's College, London. 250 illustrations.

PART III.

PROGRESS OF MEDICAL SCIENCE.

I.—SURGERY.

Treatment of Orchitis by Collodion.

M. Bonnafont has lately strongly advocated the employment of collodion in the treatment of inflammation of the testicle. He has treated seventy cases with success, and has found that the cure was accomplished more rapidly than by any other method. His results show, that the more acute the affection, the more speedy and effectual is the cure. In only one case did the collodion cause intolerable pain, which he attributes to its having been applied over uncicatrized scarifications. Its mode of action M. Bonnafont considers twofold:—*1st*, Chemically; the sudden volatilization of the ether abstracting caloric from the inflamed tissues: *2d*, Mechanically; by the compression which it exercises on drying.

MM. Ricord, Velpeau, Robert, and Pûche, have all made trial of this remedy in orchitis, but their experience does not corroborate the results of M. Bonnafont, as they either found acute pain frequently to be caused by the application, or no apparent beneficial effect produced on the disease.

M. Costes of Bordeaux, in the "Journal de Medecine" of that town, records three cases in which this method was employed with rapid subsidence of the symptoms; and he sums up with the following conclusions: "These facts appear to me conclusive in favour of the remedy of which I speak; and if I compare the results with those which I have so often obtained under any other treatment directed against this affection, I do not hesitate to affirm, that it is in every respect preferable to them all, even that so much praised by M. Bouisson of Montpellier,—I mean the application of chloroform. In effect, leeches, emollients, re-percussives, punctures, compression, either alone or accompanied by anæsthetic agents, none of these appears to me to procure more speedily than collodion, the resolution and the cure of orchitis. Above all, the pain is instantaneously subdued."—(*Journal de Medecine et de Chirurgie Pratiques*, June 1854.)

For our part we believe that we have already got sufficiently effectual remedies for arresting the malady, (viz., in the acute stage, local abstraction of blood, strong evaporating lotions, or anodyne fomentations, and in the more chronic stage, strapping), without having recourse to what appears to have given such conflicting results in French practice.

Since the above notice of M. Bonnafont's paper has been placed in type, another discussion on the employment of collodion has occurred at the Academy of Medicine in Paris. M. Ricord, on the 12th September, detailed the result of his treatment of 38 cases by collodion, 11 by compresses dipped in sugar of lead lotion, and 20 by wadding compresses. The average length of treatment in all these cases was much the same, from 17 to 18 days, but the collodion appeared to act less advantageously, producing more pain, and occasionally eczema of the scrotum.

M. Velpeau's experience of the efficacy of collodion tallied closely with that of M. Ricord. He had had 35 cases of orchitis since the treatment by collodion was suggested. These he had divided into 3 groups, 6 of them were treated merely by rest and compresses dipped in sugar of lead lotion, to 8 collodion was applied, and in 21 punctures were resorted to. The average duration of treatment was much the same, 18 days in the two last-mentioned groups, and 17 where *no* treatment was employed. The collodion did not in general produce much pain, but in several cases troublesome eczema of the scrotum. For his part, he rather gave a preference to the treatment by punctures.

M. Bonnafont adhered to his original statement, alleging that the reason of the less favourable results obtained by MM. Ricord and Velpeau, was their not seeing the patients at a sufficiently early stage. M. Velpeau, however, explained that when he treated orchitis from its very commencement by other means than collodion, the cure was effected much more rapidly than when entered upon at a later period.

The Perchloride of Iron for the Cure of Aneurism.

M. Jobert de Lamballe has treated successfully a varicose aneurism at the bend of the arm, by injection of the perchloride of iron. The first attempt failed, producing neither pain nor marked results; on the second occasion, six drops of a solution of the perchloride, in from fifteen to twenty parts of water, were injected and caused severe pain, fever, and want of sleep for some days. At the end of that time, however, the tumour had entirely disappeared. The brachial artery was reduced to a sort of cord for a portion of its length—nevertheless the pulse in the radial continued unimpaired.

M. Jobert has concluded from this interesting case that the employment of the perchloride of iron is useful in some varieties of aneurism, but that its injection causes violent irritation in the artery, and produces inflammation attended by danger.—(*Journal de Medecine et de Chirurgie Pratiques*, June 1854.)

As this experiment in the treatment of aneurism is still *sub judice*, we think it right to place on record this apparently favourable employment of the perchloride of iron; though we believe it has not as yet been shown to possess, in a sufficiently active degree, the valuable quality of inducing coagulation in aneurisms, which has caused its recommendation in practice.

Treatment of Ascites by Iodine Injections.

In the "Journal de Medecine" of Bordeaux, of August 1854, the first part of a paper by M. Henri Gintrac is published, "On the Treatment of Ascites by Iodine Injections."

M. Gintrac traces back the introduction and adoption of this mode of treatment, mentioning the various substances which were previously in vogue ere iodine was suggested.

M. Bonnet of Lyons was the first who proposed the employment of iodine solutions in dropsies of the articulations, but it was M. Velpeau who brought it prominently before the public. Reasoning from analogy, M. Dieulefoy of Toulouse was the first who put in practice the injection of iodine into the peritoneum (March 20, 1840), and since that time it has been frequently advocated. The following are the most important conclusions deduced by M. Henri Gintrac, from his experience.

1st, "The injection of the tincture of iodine into the peritoneum for the treatment of ascites, is generally free from danger.

2d, "The dose of the tincture may vary from thirty to eighty grammes, added to a hundred grammes of distilled water, and two or four grammes of the iodide of potassium.

3d, "This remedy produces in certain cases the radical cure of the ascites.

4th, "It is especially indicated in idiopathic ascites, whether active or passive. It may succeed in dropsy, coincident with simple enlargement of the liver or spleen; with tumours of the abdomen of small size, indolent, not degenerating, and for some time arrested in development.

5th, "It is contraindicated in organic affections of the lungs, of the heart and bloodvessels, of the alimentary canal, kidneys, and uterus; in short, in any case where a serious lesion of any of the viscera exists. It is also not to be employed when the liquid in the peritoneum contains a certain quantity of blood or pus.

6th, "It may act in two ways:—1. By producing adhesive inflammation, that is to say, an inflammation promoting the formation of false membranes, and adhesion of the intestines to the abdominal parietes. 2. By producing a special modification in the vitality of the serous membrane, in virtue of which the proper equilibrium between exhalation and absorption is re-established."

When we consider the admirable effects produced by the tincture of iodine on other less important serous cavities, such as the tunica vaginalis, cysts in the thyroid gland and ovaries, and cystic tumours beyond the reach of the knife, we are led to expect good results to ensue from the adoption of iodine injections in ascites. At the same time we must recollect that foreign bodies cannot be brought in contact with the peritoneum, without occasionally producing the most alarming consequences; therefore it can only be after all other less severe remedies have failed, that we may consider ourselves justified in adopting the treatment recommended in M. Gintrac's memoir. With this reservation, we hold the conclusions arrived at by that gentleman to be sound and judicious.

Case of very extensive Fistula successfully treated by a New Method. By HORATIO G. JAMESON senior, M.D., formerly of Baltimore, now of Philadelphia.—(*American Journal of the Medical Sciences*, July 1854.)

It is generally expected, that where isolated cases are published in medical journals, they are intended to illustrate either some rare form of disease, or some peculiarity in the treatment. In the present instance, we presume the object of the narrator is to hold up to condemnation the previous treatment to which his unfortunate patient was subjected, and to bring prominently to notice the efficacy of the remedial measures subsequently adopted by himself. As regards the first indication, Dr Jameson has succeeded admirably, the vagueness of his description of the whereabouts of the fistula adding considerably to its horrors:—"A gentleman of Princes' County, Maryland, was afflicted with a fistulous opening, which had been discharging freely for some years. Surgeon G. S. Pattison was sent for, and attempted to cure the case by laying open the sinus freely. An incision was made about thirteen inches in length, and four in depth, where it passed through the gluteal muscles. Some arteries were tied, and the wound dressed by putting in what sponges had been thought necessary for the purpose; but such was the extent of the wound, that *six napkins* were added to the sponges to fill it."

Next morning hemorrhage occurred to an alarming extent, more arteries were tied, the patient continuing pulseless, or nearly so, for twenty-four hours, though freely plied with wine, brandy, and other stimulants. "His life, it was supposed, was, so to speak, for several hours suspended on a hair. Judging from the cicatrix, I have never seen a wound of such extent, either from casualty or the surgeon's knife, but was forcibly reminded of the bold and successful operation by John Bell for tying a wounded artery at the ischiatic notch."

The difference in the two operations appears to be, that while John Bell's was instituted for the purpose of securing, Dr Pattison's was apparently perpetrated for the purpose of wounding, an artery in the ischiatic notch.

The gentleman (*mirabile dictu!*) recovered from the operation after some weeks, but with no improvement on the fistula. He then put himself under the care of Dr Jameson.

We now come to the successful treatment of the fistula by the so-called new method, viz., the introduction of a seton, composed, in the first instance, of shot strung upon thread, afterwards of gold wire. "By examination of the rectum, I soon found a sensible depression at a point directly opposite to the upper end of the sinus outside. It was firm and glassy to the touch, and would have held about the half of a musket ball. In this depository a passage had been formed for the matter to escape to the thigh." . . . . . "I hoped that if I could by any means abrade the surface and excite a low degree of inflammation, I might hope to cure. But here is a sinus ten or eleven inches in length, turning off nearly at a right angle, deep down among firm parts, and having to rise somewhat up into the pelvis. With a view of fulfilling these indications, I procured a steel probe of spring temper, of the proper length, and furnished with a button point, and an eye at the other end for carrying a thread; from the point it was gradually tapering, so that it would bend most easily for the space of more than an inch; this probe was armed with a strong flax thread." . . . . "It may seem strange to say that I spent nearly an hour in accomplishing my purpose; eventually the probe entered the rectum, and now all was safe. A strong thread being tied to the eye of the probe, the latter was drawn out at the rectum, and the thread drawn through the sinus and rectum. A strong thread was now armed with small shot—the way boys put sinkers on their fishing-lines; the thread was strung with shot at the distance of three-fourths of an inch. The reader is no doubt aware that these shot being all opened to receive the line, and then closed in again so as to fasten the line, become very rough."

Wherein consists the novelty of this method we are at a loss to conceive, as we have long laboured under the impression that the passage of setons through fistulæ had been among the earliest remedies suggested and employed, but which had been very properly entirely laid aside in modern practice, and the judicious employment of the knife substituted.

That our readers may judge for themselves, we transcribe the concluding passage of this eventful history:—

"Perceiving that the shot, after some weeks, were becoming tighter in the sinus, and the gentleman being anxious to go home, I provided a gold wire, which being attached to the shotted line, was drawn in as the other was drawn out. In a few months there was a perfect cure, without any confinement or suffering, and without any remedy save the wire, which being of gold there was no fear of its rusting or breaking; and the two ends being tied on the thigh at the lower end of the sinus, the patient could attend to his ordinary business without hindrance."

We have noticed this case, not so much from its intrinsic value, as to point out what appears to us errors in treatment committed by both practitioners. We must premise, however, that owing to the vagueness of the details it is difficult even to guess the true nature of the affection; but, after mature deliberation, we have come to the conclusion that it was a fistula *in ano*, having an unusually long external orifice.

The first surgeon evidently endeavoured to follow out what used to be considered the rule for practice, viz., dilating the sinus between the external orifice and the gut throughout its whole extent,—in this instance an unwarrantably severe and dangerous procedure; while again Dr Jameson, in order to avoid the dangers encountered by his predecessor, had recourse to an antiquated and very dilatory mode of treatment. It has been shown to demonstration, more especially by Professor Syme, that in every form of fistula *in ano*, however numerous and extensive the sinuses may be, the internal orifice will always be found immediately above the sphincter, and all that is necessary for a radical cure is to cut through the septum between the internal opening and the sinus, so as completely to divide the muscle. Had this comparatively slight operation been performed in the first instance, we have no doubt Dr Jameson's ammunition would not have been entrenched on, nor would a gold wire fence, encircling the gluteal regions, have been deemed necessary.

Successful case of Excision of the Knee-joint. By Charles Cotton, M.D., F.R.C.S., Senior Surgeon to the West Norfolk and Lynn Hospital.—(*Provincial Journal*, August 4, 1854.)

We draw the attention of our readers to the details of this case for the purpose of warning them against being led by the report of its successful termination to imitate the practice recorded.

Excision of the knee-joint has of late been brought prominently before the notice of the profession; and while its merits are still *sub judice*, we know of nothing more likely to damage the due appreciation of the operation than the publication of Dr Cotton's case, notwithstanding its successful termination. It has been laid down as a primary and indispensable indication by the warmest promoters of excision of the knee-joint, that complete anchylosis must be attempted to be established. Any degree of motion of the knee combined with a useful limb for locomotion is held to be impracticable, and accordingly perfect rest, and the most accurate adaptation of the ends of the bones, have been imperatively enjoined; moreover, apparatus for the proper ensuring of these indications has been carefully described.

These rules appear to have been set at nought by Dr Cotton, as the following extracts will show. The operation was performed October 5, 1853. On December 26, "He was directed not to amuse himself so frequently with flexing the knee upon the splint, to which habit attention had been directed."—"February 1. A constantly sitting posture seemed to cause a tendency to throw the end of the femur forwards."

On March 9, 1854, he was dismissed cured, and on July 26 the following is the report:—"He walks firmly, and with tolerable speed, though with a stooping gait." . . "There exists considerable power of flexion at the knee." . . "The limb is equally developed with its fellow, and he is able to project it forwards or backwards with ease."

We are much afraid that medical men, who have never witnessed the operation, and who read the details of this case, may be prevailed on to go and do likewise; and though we do not deny the statement by Dr

Cotton, that in this instance a cure was effected, though we doubt its continuing permanent, we are thoroughly satisfied, that unless complete anchylosis be the main object of our treatment, failure must almost inevitably result, and unmerited obloquy be thus thrown on an operation, which, though not yet fairly established in practice, is, from the importance of its conservative principle, worthy of a fair and properly conducted trial.

II.—MIDWIFERY AND DISEASES OF WOMEN AND CHILDREN.

Review of the Discussion in the French Academy of Medicine on Displacements of the Uterus. M. Gaillard.

Three propositions appear to be demonstrated:—

1. The instruments for maintaining the uterus replaced are often dangerous (Cazeaux). There are serious dangers in its employment (Malgaigne). I adhere to the opinion of (M. Depaul) the reporter (Hugnier). I ask who can follow this method with so many unfortunate risks to run (Hervez). The question is decided by general consent.

2. M. Cruveilhier has laid down this proposition:—That in the unimpregnated state the uterus has no axis, properly so called, its position being so variable and freqently changed by the neighbouring organs. M. Velpeau adds that the uterus floats in a moveable dissepiment, bending and moving in every direction. This doctrine had been contested, but in the meeting of 27th June M. Dubois anew insisted upon the anatomical conditions of the uterus. What is looked upon (says he) as the regular position of the uterus, is only the exception. The anatomist, the surgeon, and the accoucheur, are thus agreed; the situation of the uterus in the pelvis is variable, and no importance should be attached but to great displacements. These considerable displacements are rare, and it is easy to explain how certain practitioners always find some displacement or other in the cases submitted to their examination.

3. The partisans of replacement readily use the following subtle argument, well brought out by M. Hervez;—a patient is taken with uterine sufferings. I examine her. I find a displacement. I apply an instrument for replacement and the patient is relieved. Therefore the displacement was the cause of the suffering. This is the reasoning of Valleix and Cazeaux; we cure by replacing, therefore the uterus requires to be replaced. These cures appear to us, as to M. Depaul, much more common and complete in speeches than in practice; but let us admit that these instruments produce real relief, then we have to meet a new difficulty. M. Cruveilhier has written,—the instrument for replacing the uterus can in no wise remedy the displacements of the organ. M. Depaul discovers the persistence of the displacement after the treatment of M. Valleix. M. Dubois, answering M. Hervez, says this practitioner deceives himself, the replacement of the uterus is not and cannot be but temporary. The uteri treated by Simpson and Valleix are as much displaced after as before, sometimes even more. M. Dubois is confident, and cites numerous cases; he is more decided in his opinion than Depaul, who had been accused of severity. Here then is another fact, the application of these instruments by the inventors themselves produces only a temporary replacement. Thus the comparison by Malgaigne between hernia and displacement falls to the ground; for the patients of M. Valleix are cured, the displacement persisting, while the patients with hernia continue to suffer if the bowel come down again from the abdomen after being kept there hours or days.

M. Dubois examines from a new point of view the effects of the instruments for replacement, and sees certain advantages obtained by this mode of treatment. The legitimate conclusion from his speech is this:—The instruments may do some good, but the physicians who have used them have produced quite a different result than they imagined. Their facts are interesting, but their theory is without foundation. Moreover, according to the illustrious Dean of the Faculty, M. Valleix is working in a good direction. Will M. Valleix agree to it, that his apparatus produces no permanent replacement? Does he accept this praise, which may be put thus,—a surgeon wishes to treat by the lithotomy incision a neuralgia of the neck of the bladder, a painful but innocent affection; to satisfy the patient, who is afraid of the dangerous operation, he persuades him he has the stone!

One word on the speech of M. Cazeaux. This surgeon has sharply attacked the honorable reporter; but his arguments are more severe than just, and in the end he comes almost to the same conclusions. M. Depaul had advanced that in women suffering at the same time from an uterine phlegmasia and displacement, it was sufficient for the dispelling of all the symptoms to cure the phlegmasia. This opinion appears to M. Cazeaux to be exorbitant; it has not, he says, been supported by any one. But hear M. Cruveilhier,—"The same symptoms are observed in simple inflammation of the uterus, independently of displacement of the organ." A surgeon who read a paper to the Academy on the 25th April argues from this fact ordinarily observed in his practice. His patients are cured, he says, without the abnormal position of the uterus being modified in the least degree; from this he naturally concludes that the abnormal position of the uterus had nothing to do with the symptoms.

A contemporary journal publishes, three times a-week, letters in favour of the doctrine of M. Valleix; the advocate is very able, he knows well how to clothe the truth; his object is to prove that the instrument for replacement is good for something, and to support this somewhat controverted proposition, the author leads forth whole battalions of arguments. That the instrument, in a general sense, is good for something, is probable. There is scarcely any plan which does not sometimes prove useful; but to condescend upon particulars, and say for what these instruments are useful is not an easy matter.

The advocate for replacement affirms, that M. Valleix cures his patients by placing the displaced uterus in its normal position. This is clear and logical. You are mistaken in that, says M. Dubois. The instrument never restores the uterus to its position permanently, it cannot therefore be in this way that it relieves the patients. It acts in regulating the menstrual periods, and in modifying inflammatory and neuralgic conditions, which have not been taken into consideration in the discussion. M. Dubois does not inform us what becomes of the uteri not replaced. This orator, therefore, is in opposition to the partizans of the instruments. He does not admit the advantages said to be derived from them, and attributes others to them which had not been appreciated. His speech, however, is the most favourable to the instruments. The words of the Dean of the Faculty have an incontestable authority; but till it be proved, we shall believe that these neuralgic states, so easy of change by the contact of an instrument, occur only in exceptional cases. Upon this point observe the opinion of M. Velpeau:—These inflammations, catarrhs, and neuralgias, appear to us to be very rare; they have been invented to explain away the real symptoms produced by uterine displacements. Thus the displacements, according to this celebrated surgeon, cause numerous sufferings, but we are left in the difficulty as to what influence the instruments exert upon these displacements. My pessaries (in the rectum), says M. Hervez, do, without danger, all the good attributed to the intra-uterine instruments. This assertion appears to us justified by the facts. The caoutchouc pessary attached to the *redresseur* would thus be the essential part of the instrument. Will the intra-uterine instrument, then, which has been produced with so many pretensions, dwindle to the modesty of a pessary? Conclusion: In applying an intra-uterine pessary we do not as yet know what we are doing, nor why we are doing it.

We agree cordially with the speech of M. Gibert. The affections of the uterus are frequent, no doubt, but many women are anxious about them, without reason; some people also are, perhaps, too busy in regard to them. Time will correct this abuse, and the best way of arriving at this reform, is to fix the attention of practitioners upon the importance and real value of certain pathological lesions. Public opinion comes in the long run to be a distant echo of professional opinion; why then ask the public to avoid errors into which we ourselves are too disposed to fall. M. Gibert has seen to pass their day the engorgements so dear to Lisfrane and the beloved granulations of his successors. He prognosticates for the new method the fate of the beautiful flower—soon to fade.

Finally, the actors in these great disputes are not known to us personally, *nec odio, nec amicitia cogniti*. We are the public, that impartial judge, whose interest it is to seek the truth in the name of science and humanity.—(*Gazette Med.*, 15 Juillet.)

The Behaviour of the Pelvic Articulations in the Mechanism of Parturition. Dr Matthews Duncan.

In the guinea pig, there takes place at the time of parturition a very considerable separation of the pubic bones, the ligamentous tissue of the symphysis stretching in this small animal to the extent of an inch, or even more. In the guinea pig, the motions of the iliac bones are analogous to those of abduction of the limbs. In the cow, such movements are absent. The symphysis pubis is consolidated by bony union, and consequently abduction of the iliac bones is impossible. But the sacroiliac joints become much relaxed, as also the sacrosciatic ligaments, and by these changes the ilia become extensively moveable upon the sacrum in an antero-posterior direction, the motions being analogous to those of flexion and extension in the limbs. The final result of these changes in the guinea pig and the cow is to enlarge the genital passages for the transmission of the calf; and these two animals present, respectively, characteristic examples of the two different sets of changes by which this result is obtained in different animals.

In the latter half of pregnancy in the human female, the soft tissues contributing to form the pelvic joints are found softened, as if by serous infiltration; and the joints are consequently relaxed. The softening of these tissues is accompanied by their increase of thickness, a change which will itself have, as a necessary consequence, the separation of the bones and enlargement of the pelvic circle. And this favourable circumstance, along with others connected with the motions of the joints, forms an important part of the explanation of some cases of delivery, by a simpler operative procedure than might at first have been considered necessary. Sometimes this thickening is to a very great extent, as in the cases of Madame Boivin and others, where the pubic bones were separated an inch or more.

The movements in the pelvic joints in the human female are the same as those occurring in man as well, only in a minor degree, as has been demonstrated by Mr Zaglas. They may be described as consisting in the elevation and depression of the symphysis pubis, the ilia moving upon the sacrum. By the elevation of the symphysis (or projection forwards of the promontory of the sacrum), the angle of inclination of the pelvis is diminished, and the conjugate diameter of the brim is lessened to the extent of one, or even two lines; the corresponding diameter of the outlet is increased about twice as much. This different ratio of the effects of the motion upon the brim and outlet, results from the fact of the centre of motion being much nearer the promontory than the apex of the bone (in the second sacral vertebra). The promontory, therefore, will describe in its motions an arc of a smaller radius than the apex.

That the alteration of the dimensions of the brim and outlet by these movements is not insignificant, but the reverse, is a proposition which every obstetrician will confirm. It only remains, then, to be observed, how these alterations correspond with the phenomena of the progress of the child in parturition. Now, in the erect position, the brim of the pelvis is in its enlarged condition, the symphysis pubis being then depressed, while the outlet is correspondingly contracted. In the course of the first stage of labour, while the head is pressing into the brim, the human female is generally standing, sitting, or lying on her back, or in an easy position. But as soon as the head has descended into the pelvis and infringed upon the sensitive vagina, then forcing efforts accompany the pains. These forcing efforts consist, in great part, of powerful contractions of the anterior abdominal muscles, the effect of which, especially the action of the two recti-muscles, will be to tilt up the symphysis pubis, thus throwing the promontory forwards, contracting the brim, and enlarging the outlet, and diminishing the angle of inclination of the pelvis. To all these changes, the position usually assumed by the female in the second stage of labour will contribute. For the simple bending of the body forwards (even in man), has for its effect the tilting upwards of the apex of the sacrum, and enlargement of the outlet. And, it is a curious fact, that a woman in her forcing pains, in the second stage, is found to draw up her legs, and bend her body forwards, thus inducing changes in her pelvis, which facilitate the advance of the child in that stage.

These motions of the pelvic bones in the human female agree with those taking place in the cow at the time of labour. The changes occurring at this time in the guinea pig, find their analogies in the altered condition of the symphysis in woman—changes which in her are generally only to a small extent.—(*Dublin Quarterly Journal of Medical Science. August,* 1854.)

The Entrance of the Spermatozoon into the Ovum. M. Keber.

By the diligence of embryologists, the science of development continues to make wonderful advances. Martin Barry and Nelson had already published observations of the entrance of the spermatozoon into the ovulum; but their observations were not regarded as completely satisfactory, till Keber, following in their footsteps, by a careful observation of the process as it takes place in the fresh water mussel, made out and described the different stages of this wonderful function. Since Keber's work appeared, M. Bischoff has been twice in the press; first of all, with a pamphlet controverting the views of Keber, and then with one fully confirming the same views. Bischoff also takes occasion to make some amends to Martin Barry, whose uncommon accuracy and profound research he has occasionally failed to recognise, till forced by overpowering evidence.

In the fresh water mussel (*unio*), at the time of conception, the ovulum, although still small, projects at one part a minute process which springs from the membrane of the albumen, and perforates the cortical membrane. This process dehisces, lets escape a little albumen, and admits one of the spermatozoa which surround it. This done, the micropyle, as it is called, again closes by constriction or obliteration.

Afterwards (and sometimes earlier) there is formed near the micropyle an adhesion between the membranes of the albumen and of the yelk, then the yelk-bag dehisces, receives the spermatozoon into its interior and again is closed.

The spermatozoon afterwards sinks deep into the yelk, swells and becomes rounded; after some time a nucleus appears in it, while its outer membrane thins and at length disappears.

The nucleus of the spermatozoon splits up into several irregular divisions which at first lie near one another, and then become diffused through the yelk, so that before the dissolution of the germinal vesicle the yelk is filled with particles derived from the male.

M. Keber has also confirmed the observations of Barry in regard to the small pellucid vesicles not rarely found in the abdominal cavity of the rabbit, attached to the ovary, the fimbriæ, the oviduct, the peritonæum, or the uterus itself; within which are observed vibratory motions over the whole surface and rotatory movements of various corpuscles, and of a large mulberry-like body on its axis, which increases under the eye of the observer by the apposition to it of roundish corpuscles from the fluid of the vesicle.

Barry observed a similar vesicle imbedded in the mucous membrane of the uterus. Those found in this situation, are smaller than those of the abdominal cavity. In these bodies Keber has observed changes resembling the formation of the micropyle in the ovulum, and for that and other reasons regards these vesicles as ova. But for farther discussion of this subject we refer the reader to Keber's monograph.

The Fibrous Tumour of the Uterus. Dr Bristowe.

The specimen exhibited was one of some interest; it was removed from a patient whose case is related by Dr Waller in the "Medical Times and Gazette," for April 1853. The following is a very short abstract:—A healthy woman, forty-one years of age, had been married fifteen months, during which time she had miscarried twice, but in a third pregnancy succeeded in attaining the full period. No sign of uterine affection appeared during utero-gestation, except a slight and easily-arrested hemorrhage about the end of the eighth month. When labour came on, a large tumour of bony hardness, supposed to be an exostosis, was found to obstruct the passage of the child. As it was impossible to deliver the woman *per vias naturales*, the Cæsarian section was had recourse to. She survived the operation thirty-six hours.

Post-mortem Examination.—The tumour supposed to be an exostosis, was found to be really a fibrous tumor, occupying the space between the rectum and vagina. The uterus itself measured five inches in length, the incision four. The os, cervix, and anterior wall were healthy, but the lips of the incision gaped widely, being apparently prevented from closing by the traction exerted on them by the tumours behind. The fibrous tumours were connected solely with the upper two-thirds of the posterior wall of the uterus, and formed an irregular lobulated mass about nine inches long, five broad, and three or four deep; the mass scarcely extended higher than the summit of the uterus, but quite three inches below the os, and as much as five below its lowest point of attachment. The depending portion terminated in an irregular, lobulated, pedunculated mass, about the size of a large orange. It was this that had impeded parturition. On section, the muscular tissue of the uterus was found to envelope some of the tumours, but in others to extend merely for a short distance into their base; this was the case with that hanging into the recto-vaginal pouch. The tumours presented the ordinary characters of fibrous tumours; were denser than the uterine walls, somewhat fleshy in appearance, and constituted of fibres curving and twisting in all directions with the utmost complexity.

The present seemed a favourable opportunity for investigating the real nature of the so-called fibrous tumours. Vogel, in his "Pathology," states that some at least of these growths are really muscular, and absolutely identical in structure with the walls of the uterus; but so far as I have been able to learn, the observations of this pathologist seem to have been confined to the unimpregnated organ. Now the tissue of the undeveloped uterus is so difficult of satisfactory investigation, presents so little that is characteristic, and the nucleated fusiform fibres which become developed into muscle are so much like those found in various other organs and tissues, that it might reasonably be doubted whether there was not some little error in the observation, or whether, supposing the anatomical characters to be really the same, the tissue of the tumours was not physiologically distinct from that of the uterine walls. I have, on several occasions, compared the structure of fibrous tumours with that of the uterus, and so far as these observations went, they were confirmatory of Vogel's views; still the examinations had always been somewhat superficial; and I had a thorough conviction in my own mind, that however much they might resemble, they were far from being identical with one another, and that careful investigation under favourable circumstances, would establish the correctness of this, and the error of the former view.

The chance of examining these tumours in the fully-developed uterus, was too good an opportunity for settling this doubtful point, to be neglected; for it was clear, that if the tumours were really muscular, they would become developed with, and in proportion to, the walls of the uterus itself; their elementary structure would resemble that of the gravid organ, and would consequently possess scarcely any features in common (so far as microscopic examination was concerned), with those presented by ordinary fibrous tumours; whereas, if the tumours were essentially fibrous, they (while the walls of the uterus became developed into muscular tissue) would retain their primitive character, or at most undergo such slight alteration as the increased vascularity and activity of surrounding structures were capable of effecting, and therefore would contrast remarkably with the walls of the organ in which they were embedded.

The microscopic examination of the tumours and of the uterine walls was conducted with considerable care. Portions of different tumours (taking care not to mistake any of the muscular tissue enveloping them for the tumours themselves), were submitted to examination; and contrary to anticipation, in all instances, without exception, were found to present precisely the same characters as the muscular tissue of the uterus itself; they were so similar indeed, that it was quite impossible to say of any specimen observed under the microscope, whether it was taken from the normal or from the abnormal structure. The tumours consisted of fusiform, pellucid, fasciculated muscular fibres, identical in size, shape, general appearance, and arrangement, with those of the uterine parietes. There did not appear to be more fibrous tissue in the tumours than in the uterus, and though it is true that the fibrous tumours were denser than the parietes of the womb, this was a distinction quite unappreciable under the microscope. (For a description of the fibres of the uterus, refer to Mr Rainey's paper in the "Philosophical Transactions" for 1850, part ii., p. 515; and for additional drawings, either to the plate accompanying that paper, or to a plate of the preceding volume of Transactions of the Society).

Through the kindness of Dr Crisp, I have had another opportunity of examining the structure of fibrous tumours in the gravid uterus. The case is recorded in the "Transactions of the Medical Society of London," first vol. new series, p. 122. The patient was thirty-six years of age, and pregnant with her sixth child; her last was born nineteen months previously, with no unfavourable symptom: six months subsequently to this event she miscarried. The only sign of anything wrong during the present pregnancy, was the occurrence of occasional slight hemorrhage during the last six weeks of gestation. The tumour did not impede the birth of the child, but the patient died four days after delivery, apparently from exhaustion. The tumour proved to be a fibrous polypus, springing from the right side of the uterus above its neck: it was ovoid, and measured 14 inches long by 5 thick.

In this case the tumour had been for several years in Goadby's solution: still, under the microscope, it was seen clearly to consist of muscular tissue, arranged as above described; the individual fibres being merely somewhat corrugated and irregular in outline from the action of the fluid in which it had been immersed.

These two examinations, the only ones I have had the opportunity of making, confirm in a remarkable—and to me unexpected—manner, the statement made by Vogel; and from the condition of organs with which the tumours were connected, they are clearly free from all fallacy. The questions now to be determined are:—"Do all so-called fibrous tumours resemble these? or are such cases as the present to be looked on as rare and exceptional?"

It is true that uterine fibrous tumours do present certain differences among themselves; some are harder, some softer than others; some redder, some paler; in some the arrangement of the fibres is exceedingly complex, in others comparatively simple; still, in spite of these and other variations, they have all so many characters in common, that they have almost invariably been included under the same name—fibrous tumour. The tumours under consideration resembled these; the arrangement of the fibres to the naked eye was like that of fibrous tumours generally, the only difference being, that they appeared somewhat softer and more succulent than usual, a difference readily accounted for. From these facts I am induced to express a confident opinion that further examination will prove that all so-called fibrous tumours of the uterus, at least in their earlier stages before degeneration has taken place in them, are essentially muscular tumours, not simply fibrous tumours containing a greater or less quantity of muscular fibre mixed up with them, as Mr Paget seems to think, but developments of true and undoubted muscular tissue, to which the term of muscular tumour, and that term only, is strictly applicable.

This view of their character may possibly be advantageously borne in mind and acted on in practice, for from their very nature, it follows that they must during pregnancy enlarge in an equal ratio with the uterus itself; so that a tumour of small dimensions, and unattended with inconvenience, may during this period attain to very considerable bulk, and from its situation and size interfere seriously with the parturient process, or in other ways induce dangerous or fatal consequences. I am aware that there is no novelty in stating that they *do* grow during pregnancy, but the preceding view of their nature may serve to impress that fact more vividly on the mind, since it shows that they *must* grow, not in that vague sense which is conveyed in the statement that they grow from partaking in the activity of neighbouring structures, but in that positive and determined manner which is expressed by the fact that they must, from the very nature, at least attain a bulk the ratio of which to its former bulk is that of the gravid to the unimpregnated uterus.—(*Trans. Path. Soc. Lond.*, April 1853.)

Tracheotomy in Childhood.

M. Guersant remarks upon the increasing numbers in whom he is called upon to perform this operation at the Hôpital des Enfans Malades. Thus, in 1850 there were 10 operations; in 1851, 25; in 1852, 30; and in 1853, 60. M. Guersant concludes from these facts that croup is on the increase in Paris; a conclusion doubted by M. Denonvilliers, who ascribes these increasing numbers to the greater confidence of the public in the resources of surgery in such cases. As regards the success of the operation, M. Guersant remarks that out of 161 cases in which it was performed there were 36 cures; and it must be borne in mind that it was only performed as a last resource when asphyxia was considered to be imminent. The rest of the treatment was much the same as is usual in this country. M. Guersant also frequently employed cauterization of the larynx, but considers that this treatment does not deserve the credit it has attained.—*L'Union Médicale*, tome viii., No. 79.

M. Archambault directs attention to a peculiar phenomenon which frequently attends on recovery in those cases of croup in which tracheotomy is performed. As soon as the glottis is free from false membranes, it is observed that the infant, in swallowing, is extremely apt to be distressed by the passage of fluid matters into the larynx. This difficulty of deglutition has occurred in the practice both of M. Trousseau and of M. Guersant. The former attempts to remove it by giving only solid or semi-solid food: the latter proposes in extreme cases to pass the œsophagus-tube. M. Archambault recommends a simpler method. He removes the canula, and applies the thumb very firmly over the opening. When the child has made several respirations, and these have become regular and natural, he allows it to drink, after directing its attention to the necessity of caution. In this way drink can be taken. M. Archambault considers that this difficulty of deglutition depends on an impaired sensibility of the glottis, and a want of harmony between the act of deglutition and respiration, owing to the artificial manner in which the latter has been effected through the canula. He has not witnessed this symptom except when the respiration was notably accelerated.—(*L'Union Médicale*, tome viii., No. 83.)

III.—PATHOLOGY AND PRACTICE OF MEDICINE.

Influence of Locality on Asthma.

Trousseau, in a clinical lecture, relates the following remarkable case. A young man, 28 years of age, a native of St Omer, had repeated attacks of asthma when resident there. On his going to London they ceased; and during the two years of his residence in *the city* he had not the slightest return of his complaint. At the end of this time he returned to St Omer, and after four days he had a return of his symptoms. Three months later he went to Paris to consult Trousseau, and here the asthma left him in a short time. In the finest season of the year he went to Versailles, and was immediately attacked by asthma. He returned to Paris, and for six months continued free. After this he was obliged to return to St Omer, and was there again attacked so violently he appeared to be dying. By Trousseau's advice he was carried on a hand-barrow to the railway station and brought again to Paris, where his sufferings once more vanished.—(*Gazette des Hôpitaux*, Nos. 29 and 34, for 1853; and Canstatt's *Jahresbericht*, 1853, vol. iii., p. 86.)

Medical Information on Turkey.

A communication by M. Beyran, a Turkish physician, educated in Paris, has been submitted to the Academy of Medicine, from which we gather the following particulars:—

The temperature of Constantinople, although in summer equal to that of Naples, falls in winter to 7, 8, and sometimes 15 degrees below zero (20, 18, and 5 degrees Fahrenheit). The variations of temperature at all seasons are considerable. The greatest mortality is in the hot season; it is, however, probably in part due to the abuse of alcoholic liquors, which, notwithstanding the precepts of the Koran, are consumed in large quantities.

The water supply of Constantinople is abundant and good, but the conduits are sometimes defective, and allow of accidental contamination, which has been productive of occasional accidents. Some of the springs contain free carbonic acid, and large quantities of water so impregnated are consumed without any inconvenience. M. Beyran ascribes the rarity of calculous affections in part to the use of these waters.

Notwithstanding the immense number of dogs which prowl about the streets of Constantinople, as of most Oriental towns, and notwithstanding the great heat and privations to which they are subjected, hydrophobia is an extremely rare affection in Turkey.

Syphilis is common, and presents no peculiarities. Scrofula, chlorosis, heart-diseases, phthisis, pulmonary inflammations, albuminuria, are common.

Typhoid fever is as common as at Paris; the intermittent fevers which are endemic do not appear to give in Turkey (as has been asserted of other places) any immunity from continued fever or from phthisis.

Pellagra is rare,—an important fact, when it is remembered that in Lombardy this disease has been ascribed to the consumption of Indian corn (*grano turco* in Italy), which constitutes a large part of the food of the population of Constantinople.

The Greek Elephantiasis is only known as an importation from Aleppo, Crete, or Candia.

Operations succeed well; as a general rule, wounds do not suppurate. Phlebitis and purulent infections are rare, although great numbers of barbers make a living by the performance of bleeding. It appears, however, that great operations are rare, as it is almost impossible to get a Mussulman to submit to the use of the knife. An exception to this remark is the operation of circumcision, which is universal, and operations for cataract, which are performed by Persians and Albanians.

The universal use, or rather the abuse, of tobacco in Turkey, is well known. M. Beyran warns the authorities against a peculiar Persian tobacco, which possesses almost poisonous properties, and might be productive of accidents and even of death, if incautiously used by the Europeans now in the East.—(*Bulletin de l'Académie de Médecine*, August 15, p. 994, and *L'Union Médicale*, June 1854.)

IV.—MORBID ANATOMY.

Fatty Degeneration of the Cutaneous Nerves of Sensibility and of the Tactile Bodies (Tast-Körperchen) *of Wagner.*

Some interesting observations on this subject have been recorded by

Meissner (*Archiv für physiologische Heilkunde*, 1853). The general result seems to be, that in diseases or accidents producing anæsthesia the texture of the ultimate cutaneous nervous filaments becomes obscured by fatty granules deposited within the nerve-tubes, which lose their double contour, and shrink considerably in volume, leaving, however, portions comparatively unaffected, like swellings in the attenuated nerve-tubes. The tactile bodies lose their transverse striæ and become very pale, sometimes also occupied with fatty granules. The nervous filaments going to these bodies disappear. These changes have been witnessed both in peripheral and central anæsthesia. One of Meissner's cases was that of an injury to the nerves of the fore-arm, another was of apoplexy with greatly diminished sensibility.

It will be recollected by some of our readers that in a case of paralysis of the recurrent laryngeal nerve from pressure, which was made some time ago the subject of a clinical lecture by Dr Todd, a fatty degeneration of the nerve-tubes at the point of compression was demonstrated by Dr Brinton. We find in these observations the commencement of a work much wanted—a series of facts on the pathological histology of the peripheral nervous structures.

Alterations in Gouty Joints.

Schroeder van der Kolk, in a memoir in the *Nederlandsch Lancet*, July and August, 1853, describes the changes in the finger-joint of a hand strongly affected with chalk-stones. He first observes (what has been long known) that the deposit (of urate of lime ?) is ultimately not confined to the tendinous and ligamentous structures, but affects the skin and subcutaneous textures. He likewise shows that in the joints most strongly affected the nerves become imbedded in and infiltrated with the mineral matter, and that the coats of the veins are to a great extent likewise invested with it. Finally, it gains the interior of the veins, and there is reason to believe that their valves are rendered incompetent by the deposit, as Schroeder van der Kolk found no difficulty in injecting the veins of the finger from those of the hand in the most severely affected parts. The arteries were unaffected. From these observations, the author concludes that the gouty deposit is taken up by the veins, and thus at least in part transmitted to the general circulation. The severe pain of gout he explains by the changes in the nerves.—(*Canstatt's Jahresbericht*, for 1853, vol. ii. p. 47.)

Atrophic Degeneration of Muscles.

Recent microscopic observations by MM. Duchenne and Mandl, as well as by M. Lebert, have thrown some light on this subject (Memoir in *L'Union Medicale* 1852). These observers show that the atrophy of the muscles which follows on protracted inaction (as in different forms of paralysis) is by no means universally attended by fatty degeneration. On the contrary, the muscle may for a long time present no change but the diminution in bulk and number of its muscular fibres ; and these may even gradually disappear altogether without the intervention of the fatty change. In other cases, as in the one recorded by Lebert (of spinal paralysis without mental alienation) some of the muscles presented fatty degeneration, while others, also extremely atrophied, had a good colour, and, as to the ultimate fibre, a normal appearance. In the progressive muscular atrophy, described by Aran and Cruveilhier, almost all these different conditions occur ; and some of the muscles presented a quite gelatinous appearance without the evolution of any fatty particles, which nevertheless were at other places present in large amount.

INDEX.

Abel and Bloxam, hand-book of chemistry, notice of, . . . 581
Abercrombie, Dr, sketch of the life, &c. of, by A. D. Maclagan, 429
Albany, transactions of the medical society of (1853), . . . 544
Anatomy, the dissector's manual, by Wilson, reviewed, . . . 576
—— elements of human, by Richardson, reviewed, ... 575
—— surgical, by Maclise, notice of, ... 580
Aneurism, cases of, by Professor Miller, ... 619
—— treatment of, by compression, ... 203
—— traumatic, of the left axillary artery, ... 417
Auscultation, &c., treatise on, by Skoda, reviewed, ... 174
—— in diseases of the heart and lungs, clinical introduction to, by Hughes, reviewed, . . 570
—— traité de, par Barthe et Roger, reviewed, ... 570

Balfour, Professor, class-book of botany, notice of, ... 563
Barclay, the Rev. Dr, on the climate of Egypt, ... 656
Barthe et Roger, traité d'auscultation, reviewed, ... 570
Begbie, Dr, illustrations of gout, &c., ... 1, 209
Begbie, Dr J. W., on facts and opinions in regard to Asiatic cholera, ... 502
Bell, G. Hamilton, Esq., treatise on circulation and nervous system, notice of, ... 580
Bellingham, O. B., treatise on diseases of the heart, reviewed, 379–524
Bennett, John Hughes, on pulmonary tuberculosis, reviewed, 396
Biliary ducts, respective offices of the minute, and cells of liver, 428
Bird, Golding, on urinary deposits, notice of, ... 190
—— natural philosophy, notice of, ... 579
Botany, class-book of, by Professor Balfour, notice of, . . . 563
Bladder, hernia of, caused by cystic tumours, ... 601
Bremner on comparative pathology of post-partum hemorrhage, . 491
Budd, Dr, on diseases of the liver, notice of, ... 551
Burnett's (Sir William), disinfecting fluid a poison, . . . 501

Calomel in tropical fever, . 49
Cappie, Dr, on the nature of inflammation, ... 58
—— on sleep, ... 649
Catalepsy, cases of, by Dr Coldstream, ... 477
Chemistry, hand-book of, by Abel and Bloxam, notice of, . 581
Chloride of zinc, on poisoning by, by Dr Stratton, ... 497
Chloroform, case of death forty hours after its inhalation, by Dr Kelburne King, ... 79
Cholera, Asiatic, on facts and opinions in regard to, by J. W. Begbie, M.D., ... 502
—— on the communicability of, to the lower animals, by W. L. Lindsay, M.D., . . . 275
—— on the diffusion and mode of propagation of, . . . 504
Chorea in pregnancy, Dr Matthews Duncan's cases of, . . . 35

Cinchonism in malarious fever, 53
Circulation and nervous system, treatise on, by G. Hamilton Bell, Esq., notice of, 580
Coldstream, Dr, on catalepsy, 477
Collodion in orchitis, 792
Consumption, pulmonary, Theophilus Thompson on, reviewed, 396
Copaiba, oil of, in gonorrhœa, 683
Cowan, Dr J. M., on the so-called fatty degeneration of the placenta, 254
Cubebs, oil of, in gonorrhœa, 684

Davey, Dr J. G., on insanity, reviewed, 187
Deaf and Dumb in Ireland, statistics of, 610
Dislocation of foot, form of, not before described, 415
Diverticula from the small intestines, on, by Mr Struthers, 230
Dun, Finlay, V.S., on veterinary medicines, notice of, 576
Duncan, Dr Matthews, cases of chorea in pregnancy, 35
—— on the displacement of the uterus, 321
Duncan, Dr J. F., on insanity, reviewed, 187
Dunsmure, Dr, report of surgical cases, 92
Dysentery, tropical, Mr Hare on, 457

Ear, Mr W. R. Wilde on diseases of, and aural surgery, reviewed, 710
Eggs and milk against the poison of chloride of zinc, 497
Egypt, on the climate of, by the Rev. Dr Barclay, 656
Emigrant ships, medical facts connected with, 422
Enteric disease referred to gout, 14
Erichsen, Professor, on the science and art of surgery, reviewed, 150
Erythema nodosum a consequence of rheumatism, 593
Eye, supposed gouty affection of, 8

Face, malignant inflammation of the, 597
Fever, tropical, &c., Mr Hare on, 40, 457

Gairdner, Dr W. T., on post-mortem examinations, 302, 605
—— *Dr W.*, on gout, reviewed, 762
Gibb, Dr, on hooping-cough, notice of, 578
Gluteal and internal iliac arteries, ligature of, for hemorrhage from wound of hip, 601
Glycerine, in xerophthalmia, 22, *et seq.*
Gout and the gouty diathesis, Dr Begbie on, 1, 209
—— Gairdner on, reviewed, 762
—— Wells on, reviewed, 762
Gray, Mr H., on the structure and use of the spleen, reviewed, 697

Hare, Mr, on tropical fever and dysentery, 40, 457
Heart, affection of, referred to gout, 9
—— diseases of, by Bellingham, reviewed, 349, 524
—— by Stokes, reviewed, 349, 524
—— fatty degeneration of, in various diseases, 592
—— thyroid gland and eyeballs, affection of, by Romberg and Henoch, 423
—— lodgment of a musket-ball in the left ventricle of, 417
Hemorrhage, post-partum comparative, Bremner on, 491
Hernia, congenital case of, with hydrocele, radically cured, 603
—— new process for the radical cure of, 605
Highley's Library of Science and Art, 789
—— Popular Medical Series, 789
Hip-joint, amputation at, 418
Holthouse on strabismus, reviewed, 771
Hooping-cough, Dr Gibb on, noticed, 578
Hospital, Report of the Imperial and Royal General, of Vienna, reviewed, 738
Hutchison, Dr, on common salt in intermittent fever, 595
Hughes, H. M., on auscultation, reviewed, 570
Hydrocephalus, chronic paracentesis in, Dr Blackman on, 586

Inflammation, Dr Cappie on the nature of, 58
Insane, Royal Edinburgh Asylum for the, extract from report of the, 614
Insanity, Dr Davey on, reviewed, 187
—— Dr J. F. Duncan on, reviewed, 187
—— on the nature of, by Dr Noble, reviewed, 187
Ireland, census of, noticed, 573
Iron, carbonate of, in the chorea of pregnancy, 37

Juniper, oil of, in gonorrhœa, . 686

Kidney, disease of, referred to gout, ... 15
King, Dr Kelburne, case of death forty hours after the inhalation of chloroform, ... 79
Knee-joint, excision of the, . 418
Knee-joint, gouty affection of, cured by colchicum, . . 78

Labour, instrumental, different results of, ... 422
Larynx, on the inflammatory affections of the mucous lining of, by Dr A. Wood, . . 120
Lemon juice in rheumatism, . 696
—— oil of, in tapeworm, . 688
Lindsay, Dr W. L., on the communicability of cholera to the lower animals, . . . 208
Liver, diseases of, by Dr Budd, notice of, ... 551
—— fatty, ... 582
Lungs, heart, and aorta, treatise on diseases of, by Walshe, reviewed, ... 571

Mackenzie, R., Esq., report on cases in clinical surgery, . 103
Maclagan, Dr A. D., Life of Dr Abercrombie by, . . . 429
Maclise, surgical anatomy by, notice of, ... 580
Meigs, treatise on uterine diseases by, reviewed, . . . 552
Military surgery, proposed chairs of, ... 419
Miller, Prof., cases of aneurism by, 619
Milk, secretion of, in newly born infants, ... 202
Minie rifle ball, wounds of the, . 206

Neuralgic diseases referred to gout, ... 18
Noble on insanity, reviewed, . 187

Orchitis, treatment of, by collodion, ... 792
Ovarian cysts, adventitious structure in, ... 421
Oxalate of lime, characters of urine depositing, . . . 207

Paralytic affections referred to gout, ... 19
Pathological society of London, transactions of, noticed, . 566
Pepper, black, in gonorrhœa, . 685
—— confection of black, in piles, 687
Philosophy, natural, Golding Bird on, noticed, ... 579
Phlegmasia dolens in a virgin, . 202
Placenta, the so-called fatty degeneration of, Dr J. M. Cowan on, 254
Post-mortem examinations, on, by Dr W. T. Gairdner. . 302, 605
Potassa, carbonate of, against poisoning by chloride of zinc, 498
Pregnancy, case of, in a rudimentary horn of uterus, . 201
Psychological medicine, elements of, by Mr Noble, reviewed, . 187

Quinine, sulphate of, superiority over bark in malarious diseases, 53

Rheumatism, acute, acetate of potass in, ... 427
Richardson, Elements of anatomy by, reviewed, ... 575
Romberg and Henoch on affection of heart, thyroid gland, and eye-balls, . . . 423

Salt, common, in intermittent fever, ... 595
Sarcina ventriculi in the fluid of the ventricles of brain, . . 206
Scalp wounds, intra-cranial suppuration after, . . . 205
Seller, Dr, on the remedial effects of the non-oxygenated essential oils, ... 681
Silver, nitrate of, in laryngitis, 144
Skey, Mr, on the prevalent treatment of disease, reviewed, . 193
Sleep, on the immediate cause of, by Dr J. Cappie, . . . 649
Soap suds as a remedy against the poison of chloride of zinc, . 499
Soda, carbonate of, against poisoning by chloride of zinc, . 498
Specific remedy in therapeutics, illustrations of a, . . . 690
Spleen, on the structure and use of, by Mr H. Gray, reviewed, 697
Stokes, on diseases of heart, reviewed, ... 379, 524
Strabismus, Holthouse on, reviewed, ... 771
Stratton, Dr, on poisoning by chloride of zinc, ... 497
Struthers, Mr, on diverticula from small intestines, . . . 230
Sulphur formerly a perfect example of a specific, . . 690
Surgery, clinical, report on, by R. Mackenzie, Esq., . . . 103
Surgical cases, Dr Dunsmure on, 92

Surgery, science and art of, by Erichsen, reviewed, . . 150

Taylor, Mr, on xerophthalmia, 21
Thompson, Dr Theophilus, on pulmonary consumption, reviewed, 396
Thorax, alterations in the circular measurement of, in diseases, . 585
—— on paracentesis of, . . 599
Tic douloureux, cauterization of nerves in, ... 605
Treatment, prevalent, of disease, Mr Skey on, reviewed, . . 193
Tube, long elastic, in the treatment of dysentery, . . 467
Tuberculosis, pulmonary, Professor Bennett on, reviewed, . 396
Turpentine, oil of, and the other non-oxygenated essential oils, Dr Seller on the, . . . 681
—— oil of, in ophthalmic affections, ... 688

Urea, on some of the compounds of, and a new method of determining common salt and urea in the urine, . . . 587
Urinary deposits, by Dr Golding Bird, noticed, . . . 190
Uteri, os, on the pathological importance of ulceration of, by Dr West, reviewed, . . . 552
Uterine, walls, fatty degeneration of, ... 421
Uterus, treatise on diseases of, by C. D. Meigs, reviewed, . 552
—— fibrous tumours of, . . 420
—— inversion of, during labour, ... 421
—— retroversion of, pregnancy, death, ... 422
—— the inner surface of, after delivery, ... 202
—— disease of, referred to gout, 15

Veterinary Medicines, Finlay Dun, V.S., on nature of, . 576
Vienna, Medical Report of the Imperial and Royal General Hospital of, for 1852, reviewed, ... 738

Walshe, W. H., M.D. [illegible] diseases of lungs, heart, &c. [illegible]d, 570
Wilde, Mr W. R., o[illegible] surgery and diseases o[illegible] reviewed, . . . 710
Wilson, Dissector's Ma[illegible] viewed, . . 576
Woillez, on alterations i[illegible] cular measurement o[illegible] in diseases . . 585
Wood, Dr A., on the inflam[illegible]y affections of the mucous lining of the larynx, . . . 120

Xerophthalmia, Mr Taylor on, 21

END OF VOLUME EIGHTY-ONE.

Neill & Co., Printers, Edinburgh.

N 13 f 69

U. HOLZER BINDER BOSTON MASS.